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JESSE WAKEFIELD BEAMS

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A Biographical Memoir by

WALTER GORDY

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Photograph by John Edwards

J. W. Beams

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BY WALTER GORDY

JESSE W. BEAMS ranks among the greatest experimental physicists whom America has produced, a group that includes such men as Joseph Henry, Robert W. Wood, and Ernest O. Lawrence. Although he carried out many ingenious experiments, he is best known for his development and diverse applications of the centrifuge. His experiments with the centrifuge began in the early thirties and continued until his death. Their impact on science and technology has been enormous.

EARLY LIFE IN KANSAS

Jesse Beams was born on a farm in Sumner County, Kansas on Christmas Day 1898. His parents were frontier people in the true American tradition of the nineteenth century. His father, Jesse Wakefield Beams, senior, while yet a boy, went west from Kentucky, across the Mississippi River. At the age of seventeen he was driving herds of longhorn cattle from Texas to the prairies of the Middle West. Later, he settled on a farm in Sumner County, Kansas. Jesse's mother, Kathryn Wylie, migrated with her parents in a covered wagon from what is now West Virginia to Kansas. After a long and difficult journey, the family settled south of Wichita.

Jesse was a son in his father's second family. His father's first wife died after there were four children in the family, two boys and two girls. Sometime after her death, Jesse's father met Kathryn Wylie, whom he married. They had two children, Jesse and a younger brother, Harold, who grew up to be a distinguished biologist, a professor at the University of Iowa.

Those who seek a genetic or social basis for outstanding achievements and academic excellence may wonder why the two children of the second family of Jesse Beams, Sr., reared on the same farm, grew up to be distinguished scientists and professors whereas none of the children of the first family, so far as I could learn, became known scholars or scientists; apparently, they followed the farm life of their parents. Although Kathryn Wylie's family also lived on a farm, one of her brothers became a physician.

Jesse's outstanding accomplishments could hardly be attributed to early academic opportunity. His first seven years at school were spent in a one-room schoolhouse, several miles from his isolated farm home. He walked to school, or skated when there was ice and snow. Skating on the river, he said, was the easiest way to get to school on cold days. Although the teacher he had must have been excellent, the instruction he received in the first seven grades had to be meager. Anyone familiar, as I am, with the one-room school knows that a single teacher of several grades has little time for teaching any one student or even any one grade. After school there was little time for study because of the heavy assignments of farm "homework"—husking corn, pitching hay, and milking cows. Despite his skimpy grade-school training, Jesse went on to graduate from high school with distinction.

Among Jesse's duties on the farm was the turning of a centrifuge cream separator. Can it be that his lifelong fascination with the centrifuge originated from this hand-cranked separator rather than from something he read in a book? From early childhood he was exposed to spectacular displays of natural phenomena. Many times he must have watched the swirling dust of the whirlwinds that frequently dance over the Kansas plains in summer. He certainly was deeply impressed by the awesome displays of lightning streaking over the wide Kansas skies followed by rumbling thunder. Second in importance to the centrifuge in Jesse's physical experiments were those designed to gain information about electrical discharges, including lightning itself.

While it is easy to connect Jesse Beams's remarkable experiments in physics with his early experiences on the Kansas farm, there were thousands of children brought up on farms of the western plains who undoubtedly participated in the same farm operations, who saw over and over again the manifestations of the same natural phenomena without being so motivated to explore them. There must have been something different in the makeup of the boy Jesse that caused him to see more than the others did, to crave more than they to understand what he saw.

Jesse Beams obtained his undergraduate training at Fairmount College, in Wichita, where he worked at various jobs to pay his expenses. He achieved high honors and was president of his senior class. In consideration of his fascination with physical phenomena, it is not surprising that he chose physics as his major subject. In 1959 his alma mater, which had then become the University of Wichita, conferred upon Jesse the distinguished Alumnus Award.

GRADUATE EDUCATION IN PHYSICS, 1921–1925

After graduation from Fairmount College in 1921, Jesse attended the University of Wisconsin for one year and obtained the M.A. degree in 1922 with a major in physics. In the fall of 1922 he interrupted his graduate education to accept an instructorship in physics offered him by Fred Allison, chairman of the Physics Department of Alabama Polytechnic Institute, now Auburn University. Although he remained at Auburn only one year, he greatly impressed Fred Allison with his exceptional ability as an experimentalist. Much credit must be given to Allison for the future course of Beams's career. At this critical period he urged Jesse to complete his graduate education at the University of Virginia, where he had obtained his own Ph.D. in experimental physics. No doubt Allison was greatly responsible for Jesse's being offered a teaching fellowship at the University of Virginia for 1923 and 1924 and for his decision to accept the offer. It is not surprising that Jesse chose as his thesis director Professor Carroll M. Sparrow, who had directed the thesis research of Fred Allison.

The thesis project that Professor Sparrow assigned to Jesse may have been as exciting to him as lightning over the Kansas farm. Sparrow proposed that he measure the time interval between the arrival of the quantum and the ejection of the electron in the photoelectric effect. Although Jesse did not achieve this objective for his Ph.D. thesis, his attempts to do so did lead to the development of experimental techniques and instruments that he and others used later for many important experiments. With light from a high-intensity spark source that was reflected from a mirror rotating at high speed, he produced extremely short flashes of light for which the onset and duration were measured with an ingenious light-switching mechanism he developed. The light switch was a Kerr cell that had electrical delay lines differing in length between the activating voltage, which opened the switch, and the spark gap, which shorted out the voltage and thus closed the switch. This system proved capable of measuring time intervals down to a hundred-millionth of a second. By employing liquids of very low viscosity for the isotropic medium in the Kerr cell, he found that the switching time within the cell itself could be made negligible. He used these devices to measure, among other things, the relative interval of time between the excitation and the emission of certain fluorescent spectra and the relative times of the appearance of different lines of a spectrum after excitation.

THE YALE YEARS, 1926–1928

Upon receiving the Ph.D. at Virginia in 1925, Beams was awarded a National Research Fellowship, which he held for two years, the first year at Virginia and the second at Yale. He had the good fortune at Yale to meet and work with Ernest O. Lawrence, a young experimental physicist of considerable imagination and skill, who, like himself, had been reared on an isolated midwestern farm. Their elementary education, or lack of it, was quite similar. Both attended small midwestern colleges, obtained the M.A. degree from a midwestern university, and received the Ph.D. degree in 1925 from an eastern university (Ernest, from Yale). But these two young physicists had something in common that was far more important than their parallel experiences in farm life and education. Both were fired with insatiable curiosity about the physical world, and both possessed exceptional talent for exploring it. They were destined to become leading experimental physicists of the twentieth century.

At Yale, Beams and Lawrence collaborated on several studies, primarily on experiments concerned with measurements of short time intervals, which probably evolved from Jesse's Ph.D. research. After further refinement of the techniques that he developed at Virginia, Beams, with Lawrence, returned to the problem assigned to him by Professor Sparrow for his Ph.D. thesis: measurement of the time interval between the light quantum and the ejection of the electron in the photoelectric effect. By this time, physicists, including

Beams and Lawrence, had become more aware of their limitations with respect to gaining experimental information about the interactions of individual quanta with single electrons. They consequently adopted the more realistic goal of measurement of the time between impending flashes of light and the onset of photoelectric emission. Although this interval of time proved too short for them to measure, they were able to set definitive upper limits for the intervals. They concluded, for example, that photoelectric emission begins in less than 3×10^{-9} seconds after the beginning of illumination of a potassium hydride surface.

Probably the most widely known collaborative effort that Beams and Lawrence made was their attempt to chop light quanta into segments by means of an air-driven, high-speed, rotating mirror. In a related experiment, they tried to measure the length of a light quantum. These experiments, though doomed to fail, were bold, suggestive ones at this stage in the development of quantum theory. Evidence that Beams and Lawrence recognized these experiments as far out on the border line of the knowable is revealed in their statement: "There is no definite information on the length of time elapsing during the process of absorption of a quantum of energy photo-electrically by an electron, and [furthermore] the so-called length of a light quantum—if such a concept has meaning—is equally unknown experimentally."[1]

RETURN TO VIRGINIA

After the expiration of his National Research Fellowship and a year spent as an instructor at Yale, Jesse Beams returned to the University of Virginia in the fall of 1928 as an associate professor of physics. This appointment proved to be

[1] J. W. Beams and E. O. Lawrence, "On the Nature of Light," *Proceedings of the National Academy of Sciences of the United States of America*, 13(1927):207.

fortunate for the university as well as for Jesse Beams. At that time, L. G. Hoxton, chairman of the Physics Department, was concerned about the state of the program of graduate studies and research in physics and was anxious to build them up. As future events proved, he could not have done better than to attract young Beams back to his alma mater, even at a two-rank promotion over his Yale instructorship. In his history of the Physics Department of the University of Virginia, F. L. Brown, professor of physics at the University of Virginia from 1922 to 1961, began the chapter concerning the period from 1928 to 1936 with this statement: "With the return of Dr. J. W. Beams to the University of Virginia as associate professor a new period of growth and development can truly be said to have begun."[2] Increasing numbers of physics students of high quality chose Virginia as their graduate school and Beams as the director of their thesis research. These students came first from the southern states, then later from throughout the nation as Beams's reputation as a clever experimentalist spread. Two students who came early to work with him were Edward P. Ney of the University of Minnesota and J. C. Street of Harvard, both now members of the National Academy of Sciences.

There were no government grants when Jesse returned to Virginia in 1928 and apparently no state funds allocated for research in physics. At that time graduate students supported themselves by teaching the undergraduate laboratories. Fortunately, minimal funds were required for research equipment and supplies. A year later the financial outlook was notably improved; the Du Pont Company established several fellowships at the University, some of which were available for physics. About the same time, a fund for research in the physical sciences was established by the General Education

[2]F. L. Brown, *A Brief History of the Physics Department of the University of Virginia, 1922–1961* (Charlottesville: University of Virginia, 1967), ch. 5, p. 1.

Board, apparently with an agreement that the State of Virginia would contribute enough to maintain the fund at a level of $45,000 a year, of which the physics department was to receive a maximum of $11,670.[3] Although paltry indeed in comparison with present levels of support for physics research, these funds in support of the ingenious experiments of Jesse Beams had an enormous impact on the development of science in this country. What influence Jesse's return had on these encouraging developments in the physics program at Virginia I do not know, but I suspect it was considerable.

Evidence that the administration recognized Beams's worth to the University was his promotion to a full professorship in 1930, only five years after he received his doctorate there. Lest the reader conclude that the administrators of the University of Virginia in the predepression years differed from university administrators today in their rapid, voluntary recognition of the worth of a young staff member, I shall briefly indicate how Jesse's promotion to professorship came about.

According to his wife, Maxine, while Jesse was an associate professor at Virginia he received a "wonderful offer" from another university. Though she did not mention the name of the university, I concluded that it was somewhere in the Midwest, near his native Kansas. The offer was so attractive that he went for an extended visit to consider it. While away he became inclined to accept the offer.

Upon his return, he went to the president of the University of Virginia to resign his position. The president responded, "Young man, you are just causing me much trouble." Then he quickly offered to raise Jesse's salary and to promote him to full professorship.

[3] *Ibid.*

Having concrete evidence that his talents were appreciated by the highest levels of the university administration, Jesse never again came so close to leaving the University of Virginia, despite the many wonderful offers he received through the years. Whenever he received an enticing offer with a considerably higher salary than he was receiving, Jesse would ask Maxine what he should do. Each time she gave him the same answer, "Jesse, you should do what you want to do, what you think is best." Each time the result was the same—he refused the offer and after the decision was made, again to quote Maxine, "He was so happy."

DEVELOPMENT OF THE ULTRACENTRIFUGE

After 1930 Beams's principal research programs were concerned with axially rotating systems from the very, very fast to the very, very slow. This does not mean that his programs lacked breadth and diversity—far from it. Under his continuous cultivation the centrifuge became a family of instruments capable of solving a variety of basic problems in chemistry and biology as well as in physics; it had many important technological or industrial applications, from testing the strength of materials to the separation of uranium isotopes for nuclear energy. He converted the centrifuge, capable of rotating only a few thousand times a minute, to the ultracentrifuge, capable of rotating a hundred million times a minute ($\sim$ 1.5 million rotations per second), with peripheral speeds greater than 2500 miles an hour. At the highest speed, the peripheries of some of the small, spherical rotors experience a force of acceleration a billion times that of the earth's gravitation. The speed is limited only by the strength-to-density ratio of the material composing the rotor. The rotor is magnetically suspended in a highly evacuated container, in which the resistance to rotation is so small that the rotor, once set in motion and allowed to coast, would continue to rotate for many years without a driving force.

To appreciate the difficulties Beams and his group had to overcome to produce the ultracentrifuges that rotate up to 1.5 million times a second, let us review briefly the history of the development of the centrifuge to the time he began working with it. The simplest centrifuge is one mounted on a shaft and rotated by some external system attached to the shaft, such as the motor-driven wheels of an auto or the rotating blades of an electric fan. Alternately, a moving fluid may be used to drive the shaft-mounted rotor, as was done for centuries in waterwheels and windmills. Serious difficulties are encountered when one attempts to spin the shaft-mounted rotors at speeds up to a few hundred rotations a second. These difficulties come from inability to make the inertial axis of the rotor coincide exactly with the axis of the shaft about which it is forced to turn. Anyone driving a car at high speeds knows the problems caused by wheel imbalance, but the wheels of a car driven at the national speed limit make only a dozen turns a second.

In 1883 a Swedish engineer, Carl G. P. de Laval, overcame some of the difficulties by mounting a steam-driven turbine rotor on a long, flexible shaft that could shift under the force of an imbalance to the inertial axis of the turbine wheel. With this innovation, de Laval constructed a small steam turbine capable of turning at seven hundred rotations a second. Between 1920 and 1925, Theodor Svedberg, at the University of Uppsala, with meticulous design and exceptional workmanship, constructed small centrifuges mounted on nonflexible shafts, which achieved rotational speeds of the order of a thousand rotations a second. When the rotor was mounted under hydrogen gas at subatmospheric pressures to reduce frictional heating, Svedberg succeeded in separating out and weighing large biological molecules through the molecular sedimentation produced by centrifugal fields up to approximately a million times the gravitational field. His well-known experiments won for him the Nobel Prize in 1926.

The early design of the centrifuge from which Beams learned most appears to be that made by two Belgian scientists, E. Henriot and E. Huguenard, who produced a shaftless, air-driven rotor and suspended it in space by a jet of air. The unattached rotor was free to spin in stable equilibrium about its own inertial axis of rotation. The suspension of the rotor in space is an application of Bernoulli's principle, which will be familiar to those who have had a first course in physics. With this type of centrifuge, rotors an inch in diameter can be spun up to four thousand rotations a second. The principal deterrent is the frictional resistance of the air.

This brief summary brings the history of the centrifuge to the time when Jesse Beams became involved with its development and applications. In his article, "Ultrahigh-speed Rotation,"[4] he wrote:

> It was this system [referring to that of Henriot and Huguenard] that came to our attention in the late 1920's when Ernest O. Lawrence and I were looking for a way to make high-speed photographs of the breakdown of electric sparks and of other phenomena of very brief duration. By mounting a mirror on an air-driven rotor we were able to build a high-speed camera that met our needs. This was my introduction to high-speed rotation.[5]

Back at Virginia in the early thirties, Jesse had begun to dream of the many important new applications that would be possible if the rotational speed of the centrifuge could be increased from the few thousand rotations a second then available to a million or more rotations a second. Conse-

[4]J. W. Beams, "Ultrahigh-speed Rotation," *Scientific American*, 204(1961):135–47.
[5]*Ibid.*, pp. 138, 140.

quently, he concentrated on the factors that restricted the speed of previously designed rotors and began his protracted efforts to overcome them. It is interesting that his close friend and coworker, E. O. Lawrence, whom he had left at Yale, was at the same time concentrating his inventive talents on making electrons whirl in circles, faster and faster, about a common axis. At Virginia I was told that a friendly competition existed between Beams and Lawrence, who was then at Berkeley, to see which one could increase the rotational speeds of their respective systems at a faster rate. I do not know the final score, but history seems to indicate that they both won. Jesse succeeded in increasing the speed of centrifuge rotations a thousandfold, from a few thousand rotations a second to more than a million rotations a second.

Beams realized that the rotor must be enclosed in a relatively high vacuum if his model was to achieve higher rotational speeds than the previous "ultra" centrifuges. The high vacuum would also eliminate the frictional heating of the liquid solutions, which seriously interfered with the sedimentation experiments. In his first designs the rotor was suspended in an evacuated container by a flexible shaft that passed through a heavy oil seal to the outside, where it was attached to an air-driven turbine. The flexible shaft could shift its position slightly, thus allowing the rotor to spin about its own inertial axis, as in the system of de Laval. Because of the externally rotating parts, this model was far from frictionless, but it did eliminate the troublesome problem of frictional heating of the samples in the rotor, and it did permit rotors as much as a foot in diameter to be spun thousands of rotations a second. Beams stated that one of his most difficult problems was the development of a practical, vacuum-tight oil gland through which the rotating shaft would pass. Once this problem was solved, the design became a model for many commercial centrifuges for separation of molecules in solution. In 1961 Beams stated that ultracentrifuges of this general type had been the "workhorses" of molecular sedimentation experiments in this country for twenty-five years.[6]

Although this evacuated, shaft-supported ultracentrifuge proved to be enormously useful, it was not the ultimate one that Jesse was seeking. His desired ultracentrifuge was one in which the spin rate would be limited only by the tensile strength of the rotor itself. To reach this ultimate limit, Jesse knew that the rotor must spin in a very high vacuum and that it must not be impeded by a supporting shaft. About 1934 he and his associates began to experiment with magnetic field support of a rotor that was constructed of, or implanted with, a ferromagnetic material. The field of an electromagnet, located outside and directly above the evacuated container, could penetrate the walls of the container and lift the rotor. This ferromagnetic rotor would seek the region of strongest field, that in line with the magnet's core, and, when spinning freely, would also seek to rotate about its own inertial axis of symmetry. Consequently, Jesse cleverly hung the cylindrical core of the external electromagnet by a flexible wire in a loose-fitting oil container so that the spinning ferromagnetic rotor could pull the axis of the supporting magnetic field exactly into line with its own axis of rotation. This feature in the design solved the troublesome problem of stabilization of the spin axis at very high rotational speeds—but other problems remained to be solved.

A symmetrical rotor completely stabilized along a vertical axis could still shift up or down along this axis if the critical balance between the lifting magnetic field and the gravitational pull was not maintained exactly. Beams and his group first solved this problem by focusing a horizontal light beam across the rotor onto a photoelectric cell. If the rotor moved

[6]*Ibid.*, p. 140.

slightly upward or downward, the light intensity on the photoelectric cell would increase or decrease in such a way as to produce a correcting current in the electromagnet that would restore the original position. In later models they achieved stabilization with a conducting loop placed above the rotor. If the rotor should move upward toward the loop, the current would increase; if it should move downward, the loop current would decrease. A servomechanism connected to the loop sent a correcting signal to the electromagnet.

With the rotor thus stably suspended entirely by externally applied fields in its closed, evacuated container, the only remaining problem, that of finding a satisfactory method of spinning the rotor without introducing the mechanical driving shaft, was solved elegantly when Beams and his associates constructed the rotor in such a way that it could be driven by electromagnetic induction fields produced by "field" coils outside the container. In effect, the rotor became the turning armature of a synchronized induction motor.

This was the ultimate ultracentrifuge of which Jesse had dreamed. It would spin rotors ranging in diameter from less than a thousandth of an inch to more than a foot, and ranging in weight from a billionth of a pound to more than a hundred pounds. The rotors could be spun without detectable instability ("sleeping tops") to speeds of more than a million rotations a second, speeds at which they would explode under the enormous centrifugal fields of more than a billion *G* that could be easily produced. The resistance to spin was due almost entirely to residual air in the container. With the vacuums easily obtainable, this amount was so small that a freely coasting rotor would lose only one revolution per second of speed in an entire day. So little was the resistance, that by painting a spherical rotor with one side dark (absorbing) and one side light (reflecting), Jesse was able to increase the speed simply by shining a light beam on the spinning rotor. He thus achieved a new and sensitive measure of light pressure.

This completely stabilized, almost resistanceless rotor developed at Virginia under Jesse Beams's guidance made possible many new experiments. Although the instrument was used in other laboratories, some of the more significant applications were carried out by Beams and his group at Virginia. For example, Beams was the first to succeed in separating atomic isotopes with a centrifuge. I shall give further details about this later. By driving the rotors to explosive speeds, he and his group used the new ultracentrifuge for extensive measurements of the strength of materials. Of particular importance was their finding that thin metallic films (with thickness of the order of atomic dimensions) were proportionally much stronger than the corresponding bulk metals. They found, for example, that the tensile strength of a silver film thinner than 0.000025 cm is thirty times that of the bulk silver.

Extensive application of the ultracentrifuge is made in the purification of materials in solution by the sedimentation process and in the separation of organic and biological molecules and measurement of their molecular weight. Such measurements as these had been made with earlier centrifuges, but the new Beams ultracentrifuge made the separations more complete and the measurements more precise. The centrifugal fields of the Beams ultracentrifuge proved to be sufficiently large to produce sedimentation in all known substances in either the gaseous phase or in liquid solution. It was thus able to purify almost any known substance that can exist in a liquid or a gaseous phase at a temperature ranging from that of liquid helium to well above room temperature. Molecular weights can be measured to a precision of much better than one percent in a range from fifty to more than a million molecular weight units.[7] It requires little imagination to visualize the widespread chemical and biological applications of such a tool.

GAS CENTRIFUGE CONCENTRATION OF ATOMIC ISOTOPES, ESPECIALLY THOSE OF URANIUM

The Beams contribution that is likely to have an enormous eventual impact on the industry and the economy of this and other nations is his pioneering use of the ultracentrifuge for separation of atomic isotopes, especially those of uranium. Sir J. J. Thomson invented the first atomic-beam mass spectrometer in 1907 and five years later used it to show that neon consists of two stable isotopes, ^{20}Ne and ^{22}Ne. Then F. W. Aston, one of his students, greatly improved this type of mass spectrometer and used it to measure the masses of most of the stable isotopes. Other scientists—among them A. J. Dempster, K. T. Bainbridge, and A. O. Nier—further refined the beam-deflection type of mass spectrometer for precise measurements of all known stable isotopes and for concentration of certain isotopes in very small quantities for important tracer studies. This method was recognized as inadequate, however, for the large-scale concentration of the heavier isotopes needed for industrial uses.

The possibility of using the centrifuge for isotopic separation was proposed by F. A. Lindemann and F. W. Aston as early as 1919. Several physicists, including Aston, followed their proposal with theoretical papers and experimental efforts to separate isotopes by centrifugal methods. All the attempts failed until 1937, when Beams and his students succeeded with his newly developed ultracentrifuge in separating ^{35}Cl and ^{37}Cl in chlorine gas. To justify his use of the

[7] J. W. Beams, "High Centrifugal Fields," *The Physics Teacher*, 1(1963):103–7.

centrifuge for isotopic separation after others had met with failure and abandoned it, Jesse said: "This seemed worthwhile because according to theory the separation factor should depend principally upon the differences in the masses of the isotopes rather than upon their absolute values so that the method, if successful, could separate the isotopes of the heavier as well as the lighter [elements]."[8]

In his early history of isotopic separation with the gas centrifuge, Beams further wrote: "Soon after the announcement of uranium fission by neutrons in March 1939, the writer and L. B. Snoddy, at the University of Virginia, like many other workers, became interested in the separation of ^{235}U and ^{238}U isotopes."[9] For their initial work they obtained a small grant-in-aid (March 1940) from the Carnegie Institution of Washington and later, in 1940 and 1941, grants totaling $6,353.57 from the Naval Research Laboratory. With this modest support, in 1941 Beams and his group succeeded in making the first separation of uranium isotopes with the gas centrifuge. After the formation of the Manhattan Project, governmental support of experimental work on centrifugal separation of uranium isotopes increased, as did the restrictions for security of the projects. Throughout the war, the project under Beams's direction was maintained at Virginia, although work was started at other places.

I shall outline briefly the methods that evolved from these early efforts at ^{235}U concentration. Rapidly spinning cylindrical tubes were used to centrifuge circulating columns of UF_6 gas. These tubes were vertical, and the temperature was maintained somewhat higher at the lower ends than at the upper ends. Convection currents circulated up the center of the tubes and down along the outside walls. The centrifugal

[8] J. W. Beams, *Early History of the Gas Centrifuge Work in the U.S.A.* (Charlottesville: University of Virginia, 1975), p. 2.

[9] *Ibid.*, p. 15.

forces increased the $^{235}UF_6$ concentration along the axis and the $^{238}UF_6$ along the outer walls of the tubes. The concentrated samples of $^{235}UF_6$ were drawn off from the axial center of the tubes and passed on to other tubes where the concentration was increased further. This process was repeated in a series of tubes until the $^{235}UF_6$ had reached the desired concentration. To provide the desired capacity, parallel systems of tubes were arranged. Details of the system may be found elsewhere.[10]

Near the end of World War II, the U.S. Army decided to adopt gaseous diffusion as the principal method of separation of uranium isotopes. Consequently, support of the gas centrifuge project was terminated in January 1944. During the following decade, work on the project was dormant, according to Beams, primarily because of strict security classification. Work on the method proceeded, however, in Germany and in Russia. A team of Germans and Russians, working in Russia, apparently made substantial progress in simplification of the technique. Dr. G. Zippe, a leading member of the team, an Austrian who had been allowed to return to Germany, described the work in an interview with M. Shutte, who reported it to K. Brewer of the Naval Research Laboratory. Possibly because of reported progress in other countries, the centrifuge method was reappraised in this country in the late 1940s, and funds were made available to reactivate the project on a small scale at the University of Virginia. A. R. Kuhlthau, who had worked on the project during the war, was given responsibility for obtaining personnel and getting the work started. He was instrumental in bringing Zippe to Virginia in August 1958 to work with the project until June 1960, when he returned to Germany. This

[10] J. W. Beams, A. C. Hagg, and E. V. Murphree, *Development in Centrifuge Separation*, Report 5230, AEC, Washington, D.C., 1951.

association allowed the Virginia group to become familiar with the Russian experiments made during the period when gas centrifuge work was inactive in this country. To summarize I quote from Beams's account:

> While Zippe was still at Virginia, Dr. Ralph Lowry, who was soon to follow Kuhlthau as director when the latter became associate provost of the University, and Dr. Alwyn Lapsley joined the Virginia group and together they set about to assemble and utilize all of the advantages of their own, the Zippe and all other known techniques. As a result it soon became clear (to a number of optimists) that the gas centrifuge might possibly eventually become a competitor with the diffusion method. The progress made at Virginia soon persuaded the AEC to add a group at Oak Ridge and one at the AIR Research Company in California to the project also to shift the responsibility for the project from the Division of Research to the Production Division. The wholehearted cooperation of the three contractors together with the amazing developments in the method since that time is striking testimony not only to the wisdom of this action but to the administrative skill and devotion to excellence on the part of the directors and staffs of the three projects as well as the AEC staff that has had the AEC administrative responsibility.[11]

After his formal retirement at the University of Virginia in 1969, Beams continued to work with the gas centrifuge program as a consultant to the overall program of the AEC, as well as to the project at Virginia. He had the satisfaction of seeing the process brought to the point of acceptance by our government as a major source of ^{235}U concentration for our nation's nuclear energy requirements. In April 1977, three months before Jesse's death, President Carter authorized the conversion to the gas centrifuge process of a large-scale plant at Portsmouth, Ohio, originally planned in the mid 1970s as an expansion of the gaseous diffusion facility. This first large-scale gas centrifuge separation plant in the United States is under construction at the time of this writing (1980).

[11] J. W. Beams, *Early History of the Gas Centrifuge*, p. 39.

Gas centrifuge plants for ^{235}U enrichment are already in operation or under construction in Europe.

The primary considerations that led to the decision by our government to construct its first centrifuge plant for ^{235}U enrichment was the significantly lower energy consumption of the centrifuge method as compared with the gaseous diffusion process. According to information given me by P. R. Vanstrom, vice-president for engineering and development of Union Carbide Corporation, the gas centrifuge plant being constructed at Portsmouth will require about 145 MW of power, whereas the same capacity provided by the gaseous diffusion process would require about 2700 MW, almost twenty times that required for the gas centrifuge process. At the time of the original choice of the diffusion process and the cessation of work on the centrifuge process, we were an energy-rich nation working under the urgency of a world war. Now when this country and the entire world face a serious energy crisis, the pioneering work of Beams and his group at Virginia offers great hope for efficient production of our most promising form of energy.

PRECISE MEASUREMENT OF THE GRAVITATIONAL CONSTANT

With the developmental work on the gaseous centrifuge safely in other hands, Beams again concentrated his thinking on basic new problems. That he was approaching, or past, the normal age for retirement seemed to make no difference to him nor in the results he achieved. Indeed, at this advanced age he may have conceived the most important experiment of his career—one with the potential for increasing the accuracy of measurement of the gravitational constant *G* a thousandfold.

The first laboratory measurement of the gravitational constant *G* was made in 1798 by Henry Cavendish, of Cambridge. His beautifully simple experiment is known to all physicists. Two equal spherical masses connected by a rigid, symmetrical bar were suspended at the center of the bar by a fiber to make a torsional balance. Two much heavier spherical masses were then placed on opposite sides of the two suspended balls so that the gravitational attraction between the fixed and suspended masses produced a twisting torque on the fiber. With the measured angle of twist, the torsional constant of the fiber, and the separation of the centers of the spheres, the gravitational constant could be calculated from Newton's gravitational formula. Since that time, the Cavendish experiment has been repeated many times by many physicists with some variations and some improvement of equipment but with little improvement in the accuracy of the constant. The best of these values is considered to be 6.670 ± 0.015 dyn $cm^2 gm^{-2}$, obtained by P. Heyl and coworkers at the National Bureau of Standards in 1942. This was the accepted value of G at the time Beams began his experiments; Cavendish's value is 6.674.

It is astonishing that in the space age, when many new tests of Einstein's general relativity theory were being planned, the basic cosmic constant, G (if it is a constant!), was known to only three significant figures. The space-age need for a better G must have challenged Jesse as much as had the need to find a way to produce nuclear energy without wasting so much energy in the process.

The method that Beams designed represents the greatest advance in the technique for measurement of the gravitational constant since the Cavendish experiment in 1798. Superficially, his apparatus appears to be similar to that of Cavendish. There are the two very heavy spheres on opposite sides of a smaller, suspended-mass system. In the Beams experiment, the smaller system is in an airtight jar. The gravitational attraction tends to align the suspended bar between the centers of the two large spheres outside the jar. Unlike those of the Cavendish system, these spheres are mounted on a table that can be rotated with the smaller-mass system. The rotation of the table is controlled by the suspended-mass system through a servomechanism. A light beam that comes from a source mounted on this table is reflected from a mirror attached to the suspended cylinder and falls on a photocell mounted on the same table. When the suspended mass system starts to rotate toward the heavier mass system, the reflected light beam begins to move off the photocell, thus sending a signal through the servomechanism to the motor that turns the table. In response to the signal, the motor rotates the table so as to maintain the beam of light on the photocell. The spherical-mass system, mounted on the table, is then rotated so that a constant angle is maintained between the two attracting systems. As the suspended bar is accelerated to align with the massive spheres, the latter system is given the same angular acceleration by rotation of the table. It is just as though the earth, which accelerates a falling apple, were to accelerate away from the apple at the same rate. To a person on the earth, the apple would not appear to fall, but to an "outside" observer, the apple would appear to be unsuccessfully chasing the earth at an ever increasing speed. Likewise, an observer off the rotating table sees the two inertial systems on the table as turning together at a slowly increasing velocity, the rate of increase of which is determined by gravitational attraction between the two systems.

The Beams method has two important advantages that make it potentially orders of magnitude more accurate than previous methods for measurement of G. The first results from the fact that one can obtain G from measurement of a relatively large angular velocity accumulated from a very small gravitational acceleration continuously applied over a long period of time. Within a few days, the system achieves a visible rotation and a velocity measurable with high accuracy.

From the measured time for acquiring a given angular velocity, the gravitational acceleration is easily obtained. With this acceleration and the effective separation of the two mass systems, G can be calculated. Although in the first experiments the smaller mass system was suspended by a quartz fiber to damp out possible oscillation, the torsional constant of the fiber does not enter into the calculations. The second important advantage is that effects of surrounding masses in the laboratory and elsewhere in the universe can be averaged out by a known, constant rotation imposed on that caused by the gravitational acceleration.

While the Beams method for measurement of G probably will be refined eventually to achieve its potential accuracy, estimated to be of the order of one part in a million, only one part in 4000 was achieved by Beams and his associates before his death. In 1975 they reported the value 6.6699 ± 0.0014 dyn $cm^2 gm^{-2}$, with an order of magnitude greater accuracy than that achieved with other methods.[12]

Efforts are continuing at the University of Virginia and at the National Bureau of Standards to realize more fully the potentialities of the Beams method. Some theorists, including P. A. M. Dirac, have proposed that G may not be exactly constant but decreasing perhaps by one part in 10^{10} per year because of expansion of the universe. The Beams method appears inherently capable of measuring variations in G with greater accuracy than its absolute value. At the University of Virginia, R. C. Ritter is leading attempts to adapt the method for detection of the predicted changes in G with time.

An ingenious method for testing the assumption of continuous creation of matter was designed by Beams and his associates: R. C. Ritter, G. T. Gillies, and R. T. Rood. Two

[12]G. G. Luther et al., "Initial Results from a New Measurement of the Newtonian Gravitational Constant," in *Atomic Masses and Fundamental Constants*, vol. 5 (1976), pp. 629–35.

cylinders are concentrically rotated in an evacuated chamber that is acoustically and magnetically shielded. The outer cylinder is rotated with a precise, constant angular velocity, ω. The inner cylinder is magnetically suspended like the rotor in the Beams ultracentrifuge and is given a rotational velocity ω' by phonons of a laser beam. Creation of matter within the inner cylinder would increase its moment of inertia and decrease its angular velocity, however slightly, relative to that, ω, of the outer cylinder. In normal operation, ω' is maintained equal to ω by means of a laser pulse sensor and phonon driver with a feedback correcting signal. The amount of correcting signal to maintain ω' equal to ω gives evidence for matter creation. This proposed experiment, under construction at the time of Jesse's death, is being continued by R. C. Ritter.

A NEW INSTRUMENT FOR BIOPHYSICAL STUDIES

The many applications of the Beams ultracentrifuge for isolation and molecular weight measurement of large molecules of biological significance are widely known and have been mentioned earlier in this biography. Less known is the powerful new instrument for studies of the interactions of such molecules that Beams invented in the later years of his life. This new instrument, a magnetic-suspension densimeter-viscometer, described by Hodgins and Beams,[13] measures simultaneously and with quickness and exceptional precision the density and viscosity of a fluid system. The density is measured to one part in a million and the viscosity to one part in ten thousand.

The idea for this new instrument must have come to Jesse from his magnetically suspended ultracentrifuge. A small

[13] M. G. Hodgins and J. W. Beams, "Magnetic Densimeter-Viscometer," *Review of Scientific Instruments*, 42(1971):1455–57.

cylindrical buoy is magnetically suspended in the fluid. The calibrated electromagnet required to support it gives the fluid density. The buoy is rotated slowly by an induction field externally applied, as in the ultracentrifuge. The period of rotation at a constant power input gives the viscosity. In one design the buoy is held fixed and the fluid container slowly rotated to measure changes in viscosity. The device is capable of measuring viscosities without introducing significant shearing stresses in the liquid. Among other things, measurements with it have revealed that dilute solutions of viruses, when under extremely small shearing stresses, exhibit solid-like behavior.

Jesse worked on the refinement and application of the densimeter-viscometer up to the time of his death. In fact, on the day he died, his longtime friend and collaborator, D. W. Kupke, a professor of biochemistry in the Virginia Medical School, came at Jesse's request to his bedside to complete their latest collaborative paper on the application of this instrument. This paper reported modifications of the magnetic suspension densimeter-viscometer that made possible continuous and accurate recording of the variations in viscosity and density of solutions undergoing change. The results obtained revealed conformation of changes of ribonuclease in the presence of guanidinium chloride and a disulfide cleaving agent. Kupke relates that Jesse was excited and elated over the results. They completed the paper, and evidently Jesse signed the accompanying letter contributing it to the Academy *Proceedings,* for it appears in the October issue for 1977 with the statement, "Contributed by Jesse W. Beams."

PROFESSIONAL ACTIVITIES AND PERSONAL ATTITUDES

Jesse Beams was a respected leader in professional societies devoted to the advancement of science. He held the highest office to which his fellow physicists could elect him, the presidency of the American Physical Society. A listing of the many offices he held, the many councils and boards on which he served, is given at the end of this memoir. He received numerous awards, prizes, and medals, including the National Medal of Science, and honorary degrees from several universities, the last from Yale, where he and E. O. Lawrence worked together as young postdoctoral fellows. These various honors are also listed at the end.

How did Jesse feel about his various decorations and awards? I think he felt humbly grateful for the evidence they gave him that his friends and fellow scientists held him in high esteem. He craved their approval and good will, but he was troubled about being singled out and rated, so to speak, above his friends. Perhaps the Thomas Jefferson influence at Virginia had something to do with his attitude, but I think that humility was a part of Jesse's basic nature. It seems most appropriate that one of the honors he received was the Thomas Jefferson Award. I asked Mrs. Jesse Beams (Maxine) how Jesse felt about his many honorary degrees, medals, awards, and citations. She told me, "Jess was very modest about these things. He never would let me have these framed. They were always tucked away. I often couldn't tell his mother about them. She'd feel proud and put it in the local paper in Kansas. Naturally [for Jesse], this was just too much!"

Although Jesse Beams's contributions to discoveries in physics belong to the world and are known and used throughout the world, the influence of his educational and professional leadership is national. Probably no other physicist had so great an impact on the development of physics in the southeastern states as Jesse Beams had. He was one of the organizers of the Southeastern Section of the American Physical Society and served as its first chairman (1937). In 1973 the Southeastern Section established the Jesse Wakefield

Beams Award, to be given each year for significant research in physics. For sixteen years Beams served on the Board of Directors of the Oak Ridge Institute of Nuclear Studies. One can hardly visit a university in the southeastern states without encountering a professor who was a Beams student, or the student of a Beams student. It is understandable that his impact was greatest on the University of Virginia, where it was indeed abnormally great. In the spring of 1980 when I went to Charlottesville to learn all I could about Jesse's life and work there, I encountered Beams Ph.D. students all over the place. Frank Hereford, president of the University, took an hour of his time to talk with me about Jesse even though he was preparing for commencement ceremonies to be held the next day. Dexter Whitehead, dean of the Graduate School, did the same. This was not surprising; both were Beams's students. I met other students of his who are now professors of physics or engineering there.

It is evident that the University of Virginia recognized Jesse Wakefield Beams as one of the greatest professors in the long history of the University. He was elected to their most select societies—the Raven Society, the Thomas Jefferson Society, and the Colonnade Club. He was given the Distinguished Virginian Award by the State of Virginia.

How was Beams's laboratory regarded by scientists abroad? I answer this by relating an incident that occurred in the late sixties. Sir Harold Thompson, then Foreign Secretary of the Royal Society, when on a tour of scientific institutions in America, stopped for a visit with us at Duke. During the course of our conversation I asked him which laboratory that he had seen during his visit in the States had impressed him most. Of course I expected him to name one of the large laboratories of an institution such as Berkeley, Cal Tech, or MIT; to my surprise, he said that he was most impressed by the laboratory of Jesse Beams at the University of Virginia. He went on to say that the floors of the Rouss Laboratory were rotting through in places and the walls were cracked and unpainted—but that the instruments for Beams's ingenious experiments were firmly mounted on concrete piers and that their vital working parts were cleverly designed, made from materials of the highest quality, and constructed with the greatest care and precision. I couldn't resist adding "in the true Oxford-Cambridge manner?"

In his personal relationships Jesse Beams maintained the same high standards that he did in his laboratory experiments. He spoke freely, but softly, and always in a kindly manner. In my many years of association with him I never once heard him make an unkind remark about anyone. He expected his students and associates to work hard, very hard—and they usually did—but Jesse never coerced them into doing so. Rather, he enticed them by his enthusiasm and encouragement, by his exciting projects and ideas, and, most of all, by his own example of persistence and hard work.

For fourteen years, from 1948 to 1962, Jesse served as chairman of the Department of Physics at the University of Virginia. This was a period of rapid growth and development of the department, and I was puzzled that Jesse could manage all the business of the department and continue working for long hours in the laboratory with his students and associates, as he is reported to have done. Consequently, I asked John Mitchell, a professor in the department during this period, how Jesse, with all his other duties, managed the department. He immediately replied, "With benevolent laissez faire!" This confirmed opinions others had given me. President Frank Hereford remarked that he was a good chairman who kept the departmental meetings short and saw to it that nothing distracted the staff from physics. From Hereford, and also from Dexter Whitehead, dean of the Graduate School, I heard the following example of how Jesse handled difficult departmental problems. Sometime earlier, Jesse had persuaded C. J. Davisson (of the Davisson-Germer experiment) to come to Charlottesville after his retirement from the Bell Telephone Laboratories. Davisson was given an office in the Physics Department, which he used less and less as he grew older. Meanwhile the physics staff grew, and office space became scarce. There was increasing pressure on Jesse to ask Davisson to give up his office. Instead of doing this, he called a meeting of all the physics faculty members. When they were assembled, Jesse quietly asked "Will all of you who are in favor of throwing old Dr. Davisson out of his office, please hold up your right hands." None did, and the meeting was promptly adjourned.

Donald W. Kupke, one of Jesse's colleagues with whom he collaborated for sixteen years on biophysical problems, best expressed in his tribute to Jesse the sentiments of those with whom I talked at Virginia. These are his words:

> Anyone who knew Jesse Beams even slightly would agree that his first concern was for others. This concern was genuine; invariably, he would stop his work, listen attentively without interruption or haste, and be supportive to any who came to him—whether they were of high rank or of no rank at all. He was a gentle, guileless person who sought to be helpful in whatever matter—large, small or even nonsense—which was brought to him. He displayed a remarkably constant good humor, sick or well, troubled or elated. He was also a quiet man who thought deep thoughts about the universe and the role of mankind, but he did no preaching; his lifestyle and deeds preached his scriptural convictions most eloquently.[14]

It is sometimes said that beside every great man of achievement there is an equally great woman. Although this statement probably does not apply for every great man, it certainly seems to have been so for Jesse. Upon her marriage to Jesse in 1931, Maxine Sutherland Beams resigned the

[14]D. L. Kupke, "Obituary, Jesse W. Beams," *Trends in Biochemical Sciences*, 2(1977):N284.

teaching position she enjoyed and devoted her entire time to assisting Jesse in any way she could. She soon found that he wanted to be free of the business matters of living so that he could more freely devote his time and thought to his experiments. To give him this freedom, she took care of business matters, the household, and transportation. When they built their house, it was she who dealt with the architect and the contractor. She kept the records and paid the bills, even those for Jesse's dues in professional societies. Statements of professional dues and other bills that came to him at the laboratory he simply brought home and dumped on a table or sometimes in the middle of the bed. The purchase of clothes that required fitting often necessitated prior arrangement with the clothier, some selections by Maxine, and considerable maneuvering and coaxing before she was able to get Jesse to leave the laboratory to visit the clothier. He said that he simply did not have time to do it. Once there, he wanted to buy two or three suits so that he would not have to come again soon.

Even more difficult for Maxine than buying Jesse's clothes or taking care of business matters was inducing him to stop work long enough to get adequate relaxation. In efforts to do this Maxine tried many approaches, one of which I shall describe. His students wanted to attend the home football games but felt guilty about doing so while their professor continued to work in the laboratory. Maxine detected this situation and concluded that by attending the games Jesse could improve his relationships with his students and at the same time get much needed recreation for himself. She secretly purchased two season tickets and confronted him with pleas to take her to the games. Somewhat to her surprise, he agreed, but at the half-time intermission he insisted on returning to the laboratory to check on the experiments. Maxine also encouraged Jesse to participate in social activities, and she accompanied him to the social events of the many scientific organizations of which he was a member. One of the joys my wife and I anticipated in attending such events was our association with this delightful, kindly mannered couple.

Maxine devoted forty-six years of her life to being a good wife to Jesse; these years were evidently rewarding and happy for her as well as for him. When I asked for her comments about her life with Jesse, she said: "Jess was the most delightful, kind, devoted person in the world, and I was so lucky to have been given the wonderful privilege of sharing his fascinating, interesting life for forty-six years. And those two years of waiting around to decide, them too, I count in the total for forty-eight—forty-eight wonderful, calm, peaceful, devoted years, filled with excitement and the unexpected but always with love and devotion."

A single-sentence remark made to me by President Hereford summarizes this memoir, "Jesse Beams was the ultimate gentleman scholar."

MANY INDIVIDUALS have provided information used in this memoir. Those whom I asked for help were enthusiastically cooperative. Mrs. Jesse Beams (Maxine) graciously gave me information about Jesse's life and personality that I could not have learned from anyone else. His former students, Frank Hereford, Jr., president of the University of Virginia, and Dexter Whitehead, dean of the Graduate School, took time during a busy commencement weekend to talk at length with me.

For essential information about the Beams research programs in physics and nuclear engineering, I am indebted to several of Beams's former students or associates, particularly to John W. Mitchell, Ralph A. Lowry, A. Robert Kuhlthau, John W. Stewart, and D. R. Carpenter, Jr. Information about the biophysical research was obtained from Donald W. Kupke, a professor in the Virginia Medical School. I am grateful to Professor Mitchell also for acting as our host and arranging interviews with other staff members at Virginia. On more than one occasion I have had the opportunity of discussing the life and accomplishments of Jesse Beams with Howard Carr, one of his students, who served for many years as chairman of the Physics Department of Auburn University. Paul R. Vanstrum, vice-president for engineering and development of the Nuclear Division of Union Carbide, gave me much information about Jesse's role in the development of the gas centrifuge process for concentration of uranium isotopes. He also provided the excellent photograph preceding this article.

Finally, I want to thank my wife, Vida Miller Gordy, who helped me in every phase of this memoir.

PROFESSIONAL CHRONOLOGY
HONORS AND DISTINCTIONS

EARNED DEGREES

1921	A.B., Fairmount College (now Wichita State University)
1922	M.A., University of Wisconsin
1925	Ph.D., University of Virginia

POSITIONS

1922–1923	Instructor in Physics and Mathematics, Alabama Polytechnic Institute
1925–1926	National Research Fellow in Physics, University of Virginia
1926–1927	National Research Fellow in Physics, Yale University
1927–1928	Instructor in Physics, Yale University
1928–1930	Associate Professor of Physics, University of Virginia
1930–1969	Professor of Physics, University of Virginia
1948–1962	Chairman, Department of Physics, University of Virginia
1953–1969	Francis H. Smith Professor of Physics, University of Virginia
1969–1977	Professor Emeritus and Senior Research Scholar, University of Virginia

PROFESSIONAL AND HONORARY SOCIETIES

American Academy of Arts and Sciences (fellow, elected 1949)
American Association for the Advancement of Science (Chairman, Section B, 1942; Vice-President, 1943)
American Association of Physics Teachers
American Association of University Professors
American Philosophical Society (elected 1939; Councilor, 1951–1954; Vice-President, 1960–1963)
American Optical Society
American Physical Society (fellow; President, 1958)
American Physical Society, Southeastern Section (first Chairman, 1937)
National Academy of Sciences (elected 1943)
Virginia Academy of Sciences (fellow; President, 1947)

The Honor Five (University of Wichita)
Phi Beta Kappa
Sigma Pi Sigma
Sigma Xi
Colonnade (University of Virginia)
Raven Society (University of Virginia)
Thomas Jefferson Society (fifty years at the University of Virginia)

BOARDS AND COMMITTEES

1942–1960	Science Advisory Committee of the Ballistic Research Laboratory, Aberdeen Proving Ground
1933–1940; 1951–1955	National Research Council (Division of Physical Sciences, NAS Council, NRC Governing Board)
1952–1954	National Science Foundation, Physics Division
1948–1954 1960–1970	Board of Directors, Oak Ridge Institute of Nuclear Studies (which became Oak Ridge Associated Universities)
1954–1960	General Advisory Board of the U.S. Atomic Energy Commission
1948–1969	Board of Directors, Virginia Institute for Scientific Research

AWARDS

1942	Potts Medal, The Franklin Institute
1946	U.S. Naval Ordnance Development Award
1956	John Scott Award, given by the City of Philadelphia
1958	Lewis Award, American Philosophical Society
1959	Alumni Achievement Award, Wichita State University
1963	Meritorious Award, Virginia Academy of Sciences
1967	National Medal of Science
1971	Life Fellow, The Franklin Institute
1972	Atomic Energy Committee Citation
1972	Distinguished Virginian Award
1972	Jesse W. Beams Lectureship in Biophysics initiated at the University of Virginia
1973	Jesse W. Beams Award for Research established by the Southeastern Section of the American Physical Society

HONORARY DEGREES

1941	Sc.D., College of William and Mary
1946	Sc.D., University of North Carolina
1949	Sc.D., Washington and Lee University
1969	Sc.D., Florida Institute of Technology
1976	Sc.D., Yale University

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