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HARVEY FLETCHER

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A Biographical Memoir by

STEPHEN H. FLETCHER

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Biographical Memoir

Harvey Fletcher

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September 11, 1884–July 23, 1981

BY STEPHEN H. FLETCHER

TO CHILDREN OF FAMOUS PEOPLE, a parent is just a parent. So it is oftentimes surprising to grow up and realize that your father was an important and well-known scientist. When I was a boy, I was frequently told that my father, Harvey Fletcher, was a great man, but it wasn't until I was an adult that I learned he had made enormous contributions to the field of acoustics and atomic physics as a teacher, researcher, and administrator.

My father was born on September 11, 1884, in Provo, Utah, of pioneer parents. Provo was then a small town in Utah Valley near a freshwater lake surrounded by high mountains. As my father recalled:

> As I looked across Utah Valley, I thought that the tops of the mountains that I could see in any direction marked the end of the world where people live. On the other side of these was the great ocean. There were cracks in the wall that held the ocean back, so that the water from the ocean leaked through and formed the various streams that come down from the mountains.[1]

This musing and philosophizing about the world around him characterized my father's life. However, he had no ambitions to be a scientist. His father, whose trade was building houses, had only four months of formal schooling. His uncles ran grocery stores in Provo. My father labored with his father building houses and with his uncles delivering groceries, and as a youth he wished nothing more than to follow in their footsteps.

In 1901 he entered Brigham Young Academy. This was the only school in Utah Valley offering education beyond the eighth grade, which he had then obtained. He wanted to enroll in the Commercial Division but opted for the Normal Division because the tuition was somewhat cheaper. In the Normal Division courses in mathematics, physics, chemistry, and so forth were offered to prospective teachers, and he was thus exposed to these subjects quite by accident. He was shocked when he received a failing grade in his first course in physics. In retrospect he felt he had not taken the course seriously enough and repeated it, this time earning an A$^+$, the highest grade in the class. Years later his students were delighted and comforted when he told them that he had failed his first physics course.

Brigham Young Academy at that time was a continuation of high school and college courses, looking toward a bachelor's degree in six years. While he was enrolled in the school, the curriculum was changed to provide four years of high school and three years of college. In 1907, at the end of seven years, my father graduated with a B.S. degree; prior to graduation he was hired by the school to teach physics and mathematics courses. In the summers he put his mathematical and engineering skills to practical use by working on such projects as running a government survey in the unexplored mountain country of eastern Utah and supervising the building of water mains to supply the town of Provo.

After a year of teaching at Brigham Young University (Brigham Young Academy became Brigham Young University with the above-noted change in curriculum), his ambition was completely reoriented, from business to science. He decided to pursue a teaching career in physics and felt he needed to obtain a Ph.D. if he was to be successful. He believed the University of Chicago would offer him the best training. However, he felt that he could not face the big city alone, so persuaded the girl he had been courting, Lorena Chipman, to marry him and join in this great adventure. The marriage took place on September 9, 1908, and soon after the couple moved to Chicago.

They were, perhaps, a little naive to move to Chicago before being assured of his admission to the University of Chicago's graduate program. Indeed, admission proved to be difficult, because, despite his B.S. degree, he had completed only three years of college work, much of which was not accredited by Chicago. The Admissions Office informed him that he must take four years of undergraduate work at Chicago before he could be admitted to the graduate program. Such an extended educational program was beyond his means. Fortunately, Robert A. Millikan, then an assistant professor, came to his rescue. He suggested that my father enroll as a special student in the courses that were usually taken in the first year of the graduate physics program, and, if these were successfully passed, he might then be admitted to the graduate school. He did this and was admitted on the condition that he make up one year of undergraduate work in the college. In the following three years he not only made up the one year of college but also taught high school science in the School of Education at Chicago, assisted in college lectures by operating the projection equipment, took care of his young family, and earned his Ph.D. summa cum laude in physics—the first summa cum laude degree in physics ever awarded at the University of Chicago. He was also elected to Phi Beta Kappa.

This was a remarkable achievement after such an uncertain beginning.

At that time, Professor Millikan, together with Professor Louis Begeman, was conducting research into the nature of the electric charge. After inquiring about a subject for his Ph.D. thesis, Professor Millikan outlined the research that he and Professor Begeman had been conducting at Chicago and also described similar work being done at Cambridge University by J. J. Thomson and Regener. My father described the Chicago experiment as follows (Autobiography, p. 30):

> They had arranged a little box having a content of 2 or 3 cubic centimeters which was fastened to the end of a microscope. A tube was attached from an expansion chamber to the little box. By opening suddenly a petcock, a sudden expansion of the air in the little box was made which caused a cloud of water vapor to form. Then viewed through a microscope this cloud was seen to be composed of a large number of tiny water drops. The droplets would soon drop from the top to the bottom of the box under the influence of gravity. A conducting plate was arranged at the top and another one at the bottom of the box so that an electric field could be produced.
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> When this electric field was turned on it would retard the fall of some droplets. They were trying to make the field just right so that the droplet would be suspended in the air between the plates. From the speed of the droplet, that is the fall speed, and the intensity of the field to stop the droplet, one could calculate the electrical charge on the droplet. This was essentially repeating the experiment that Regener did in England. However, the water forming the droplet evaporated so fast that the little droplet would only stay in view for about 2 seconds. So it was difficult to get more than a rough estimate of the charge.

After discussions with Professors Millikan and Begeman, my father decided to build equipment similar to the Millikan-Begeman equipment but using oil instead of water. He described the experiment as follows (Autobiography, pp. 31–32):

First, an arc light with two condensing lenses in front of it was set up. The combination made a bright beam of light. The experience which I had with projection lanterns for lectures made it possible to get this together very quickly. I then used the atomizer and squirted some oil spray so that it fell through the beam of light. The light made these tiny drops of oil look like tiny stars. This indicated this part of the experiment would probably work. I then went down to the student shop and found some brass sheets about one-eighth of an inch thick. From them I cut two circular plates about 20 centimeters in diameter. Then I fastened (soldered) a stem onto each one so that they could be held by an ordinary laboratory stand with clamps. A small hole was then bored in the center of the top plate. These plates were then set up horizontally being about 2 centimeters apart. In this first set-up the air between the plates was not enclosed. So I moved the stands holding the two plates over into the beam of light. I then put a large cardboard between the light and the plates and cut a hole just large enough to permit the light to go between the plates without touching them. I then found a cathetometer (an instrument commonly used around a physics laboratory) and placed it so the telescope on it was turned and raised and lowered until its line of sight went between the two plates and at about 120° from the direction of the light beam. The distance from the telescope to the plates was about one meter. I then tried out the apparatus. I turned on the light; then focused the telescope; then sprayed oil over the top of the plate; then came back to look through the telescope. I saw a most beautiful sight. The field was full of little starlets, having all colors of the rainbow. The larger drops soon fell to the bottom, but the smaller ones seemed to hang in the air for nearly a minute. They executed the most fascinating dance. I had never seen Brownian Movement before—here was a spectacular view of them. The tiny droplets were being pushed first that way and then this way by the actual molecules in the air surrounding them. I could hardly wait until I could try an electrical field upon them to see if they were charged. I knew there [were] two or three banks of small storage cells in the laboratory. A large number of these small storage cells had been connected in series and mounted in storage compartments on a small trunk. Each of these units would produce 1,000 DC volts at its terminal. So I soon rolled these into place near my crude apparatus. Insulated wires were attached electrically to each of the plates. The other ends of these wires were attached through a switch to the two terminals of the 1,000 DC battery. I finished most of this that first afternoon. The next morning I spent some time adjusting it and installing a meter to read the volts applied by the big storage battery. I was then ready to try the battery on these tiny oil drops.

The atomizer was used to spray some of the oil across the top plates. As I looked through the telescope I could see the tiny stream of oil droplets coming through the hole. Again I saw beautiful stars in constant agitation. As soon as I turned on the switch, some of them went slowly up and some went faster down. I was about to scream, as I knew then some were charged negatively and others positively. By switching the field off and on with the right timing one could keep a selected droplet in the field of view for a long time.

Both my father and Professor Millikan realized immediately the importance of this discovery to an understanding of the basic nature of electricity, and he and Millikan received considerable publicity at the time for the discovery. Many noted scientists came to observe the experiment. My father recalled the visit of Charles P. Steinmetz, the "wizard" from General Electric Company (Autobiography, pp. 33–34):

He was one who did not believe in electrons. He could explain all the electrical [phenomena] in terms of a strain in the "Ether." After watching these little oil droplets most of one afternoon, he came and shook my hand and said, shaking his head, "I never would have believed it. I never would have believed it," and then left.

A paper describing the experiment and its results was prepared and published in *Science* (September 18, 1910). My father described his part in the preparation of this pioneering paper as follows (Autobiography, p. 35):

I wrote more of it than he did, particularly about the modification of Stokes Law and the arrangements of the data. He went over it all and changed the phrasing somewhat to make it read better. All the time I thought we were to be joint authors.

However, the paper bore only the name of Professor Millikan. My father describes how this came about (Autobiography, p. 36):

Phyllis was born May 21, 1910, and as you will see that is about the time we finished this paper. When she was about one month old, I was baby-sitting with her, as Lorena had gone out somewhere with some of her friends there. Answering a knock I went to the door and was surprised to see Professor Millikan. I wondered why he had come to our humble apartment. I soon found it was to decide who was to be the author of the paper referred to above. There were four other papers in the formative stage that were coming out of these oil drop experiments, and I expected they would all be joint papers.

He said that if I used a published paper for my doctor's thesis that I must be its sole author. The five papers on which we did the experimental work together were:

1. The Isolation of an Ion, a Precision Measurement of its Charge and the Correction of Stokes Law. Science, September 30, 1910—Millikan.

2. Causes of Apparent Discrepancies and Recent Work on the Elementary Electrical Charge. Phys. Zeit, January, 1911—Millikan and Fletcher.

3. Some Contributions to the Theory of Brownian Movements, with Experimental Applications. Phys. Zeit, January, 1911—Fletcher.

4. The Question of Valency in Gaseous Ionization. Phil. Mag., June, 1911—Millikan and Fletcher.

5. A Verification of the Theory of Brownian Movements and a Direct Determination of the Value of Ne for Gaseous Ionization. Phys. Rev., August, 1911—and Le Radium, July 1—Fletcher. This was my thesis.

It was obvious that he wanted to be the sole author on the first paper. I did not like this, but I could see no other out and I agreed to use the fifth paper listed above as my thesis and would be listed as the sole author on that paper.

Although my father was disappointed at not being included as a joint author on the pioneering paper, he had no ill will against Professor Millikan. He said (Autobiography, p. 36):

People have frequently asked me if I had bad feelings toward Millikan for not letting me be a joint author with him on this first paper. . . .My answer has always been no. It is obvious that I was disappointed on that first paper as I had done considerable work on it, and had expected to be joint author. But Professor Millikan was very good to me while I was at Chicago. It was through his influence that I got into the graduate school.

He also found remunerative jobs for me to defray all my personal and school expenses for the last two years. Above this was the friendship created by working intimately together for more than two years. This lasted throughout our lifetime. Remember, when we worked together he was not the famous Millikan that he later became. When he wrote his memoirs shortly before he died, he had probably forgotten some of these early experiences.[2]

H. D. Arnold was also a graduate student at the University of Chicago working under Professor Millikan at the time of the oil drop experiment and was acquainted with the new knowledge uncovered about Stokes Law. Researchers at the American Telephone & Telegraph Company and the Western Electric Company became very interested in the research being done at the University of Chicago and offered my father a position with them, which he refused. The position was then offered to Arnold, who accepted, and within three years, building on the research at Chicago, developed the Arnold vacuum tube. My father said later, somewhat wistfully, "It might have been my invention instead of his, if I had accepted the job offering there."

Instead of going to the Bell System, my father returned to Provo in 1911 to resume his teaching career at Brigham Young University. While equipment and facilities were more limited than at Chicago, he persisted in his research with the oil drop and built another apparatus to continue his measurements. He published two more papers in this field while at Brigham Young University.

Teaching performance is always difficult to evaluate, but from conversations I have had with some of his former students, my father was an innovative and inspiring teacher. He lists as some of his outstanding students from this period the following:

Vern O. Knudsen, chancellor of University of California, Los Angeles

A. Ray Olpin, president of the University of Utah

Dilworth Walker, head of Business College, University of Utah

Wayne B. Hales, chairman of Physics Department, Brigham Young University

Joseph Nichols, chairman of Chemistry Department, Brigham Young University

Milton Marshall, chairman of Physics and Math Department, Brigham Young University

Carl F. Eyring, dean of College of Arts and Sciences, Brigham Young University

By the time my father finished a five-year teaching stint at Brigham Young University, his family had increased to five—he and his wife and three children: Phyllis, born in Chicago, and Stephen and Charles, born in Provo.

Every year the offer from the Bell System to join its research effort was renewed. And, because of his loyalty and commitment to building the physics faculty and attracting potential scientists to Brigham Young University, every year the offer was rejected. But after awhile my father began to feel that he was not realizing his full potential as a research scientist at Brigham Young University. So after much soul searching he accepted the offer made in 1916 and moved his family to the New York City area, where he began work in the research and development department of the Western Electric Company.[3]

Entering the Bell System organization started a new chapter in my father's scientific life. This was a relatively large organization with specialists in many diverse fields. A good part of the organization was spending its time helping the United States gear up its defense effort as World War I continued to drag on in Europe and the United States came closer to being involved. He was too new to the system to make much of a contribution to this war effort, although he did devise a capillary microphone that worked under water.[4]

My father's first year with the Bell System was spent getting acquainted. He climbed telephone poles, operated switchboards, and installed telephones. At the end of the year he began formulating a plan for research that he felt was needed by his new employer. Since the business at that time was largely the transmission of speech from the mouth of one person to the ear of another, he felt that a thorough understanding of the characteristics of speech and hearing should be fundamental to the Bell System. He thus embarked on a study of the physical mechanisms of the body used in this form of communication. I can remember as a boy seeing my father with a bellows under his arm connected by a tube to a vibrating reed, which he placed in his mouth to form various speech sounds. He would repeat them until he was sure he understood the position of the tongue and lips that characterized the sound. His first publication on this new research was "On the Relative Difficulty of Interpreting the English Speech Sounds," appearing in *Physical Review*, in August 1920. At Chicago he had been elected a member of the American Physical Society. After his move to New York, meetings were more accessible, and he became a frequent and active participant, eventually becoming its president in 1945.

Many of his friends who knew of his important work in the measurement of the electric charge attempted to dissuade him from leaving that promising field to research the much more prosaic field of acoustics. They warned him that all there was to know about acoustics had already been discovered. However, he felt the same sense of commitment he had experienced at BYU urging him to explore acoustics and more specifically the production, transmission, and recording of sound.

The tools that the modern acoustical beginner uses, such as oscillators and amplifiers, were just being developed. The rudimentary methods for testing hearing extant in those early days are illustrated by an anecdote my father told (Autobiography, pp. 51–52):

The very wealthy Duponts had a history of deafness in their family. One of them, Alfred Dupont, was a friend of Mr. Gifford, president of AT&T Company. Mr. Gifford asked me if we could help his hearing. So I made an appointment to see him. By this time we had oscillators, amplifiers and attenuators, so we could produce a known frequency and intensity of a tone. In other words we had the essential elements of what later was used as an audiometer.

When Mr. Dupont came to my office I asked him about his hearing and he said his ability to hear fluctuated. It was better after a treatment from a doctor he had been going to frequently. But as soon as he got home it returned to the same old level. I then asked him to tell me what was the treatment. The treatment was an X-ray beam directed at his ear. I became very skeptical and asked him if I might accompany him for one of these treatments and he consented. Before leaving the laboratory we made a careful audiogram of his hearing with our embryo audiometer. The doctor's office was in a fashionable part of Manhattan. We took an elevator to the 5th floor. The elevator opened into a large room which was filled with all kinds of electrical gadgets. Large Whimhurst and static machines with large brass balls across which large sparks would fly, X-ray machines and giessler tubes, etc. The doctor said he would test Mr. Dupont's hearing before the treatment and then after the treatment. The following was the method used in the test.

There was a path along the floor marked off in feet. It was about 20 feet long. Mr. Dupont was asked to stand at one end of this. The doctor stood at the other end and said in a very weak voice, "Can you hear now?" Mr. Dupont shook his head. He kept coming closer and asking the same question in the same weak voice until he came to about two feet from his ear, where he said he could hear. His hearing level was found to be two feet.

Mr. Dupont then was asked to stand four or five feet in front of an X-ray tube with his ear facing the tube. The X-ray was turned on two or three times. He then turned his other ear toward the tube and had a similar treatment. He then stood in the 20 foot path and another hearing test was made. But this time as he started to walk toward Mr. Dupont he shouted in a very loud voice. "Do you hear me now?" As the doctor reached the 10 or 15 foot mark, Mr. Dupont's eyes twinkled and he said he could hear.

I could hardly keep from laughing because what happened was so apparent. The intensity of the loud voice from 15 feet was just the same as the weak voice at 2 feet. The doctor must have been aware of this but the patient wasn't. Mr. Dupont was quite elated until we got back to the laboratories to make another audiogram and it turned out to be exactly the same as the one made before he had the treatment. I explained to Mr. Dupont what had happened. This was tried again in about a week with the same result. After that Mr. Dupont never paid a visit to this doctor.

Although my father is credited with the invention of the audiometer, he pointed out that, while the 2-A audiometer was patented in his name, it was not the first audiometer. There was a device made by Robert H. Seashore, dean of the Graduate School of Iowa State University, and also one by Lee Wallace Dean and C. C. Bunch at the same school. There was also a contemporaneous work on audiometers by Vern Knudsen at UCLA and Dr. Jones, a practicing otologist in California, and a preceding audiometer in the Bell System—the 1-A devised by R. L. Wegel and E. P. Fowler.

However, it was the 2-A audiometer that proved most practical and was used most thereafter by audiologists and schools for hearing measurement.

My father became well known for his work for the hearing handicapped. As a consequence, he met many interesting and some famous individuals. He tells this story of a meeting with Thomas A. Edison (Autobiography, pp. 62–64):

He came to the laboratory with one of his Chief Assistants. We made tests of his hearing and found that he was very hard of hearing. If one talked very loudly into his ear he could understand. His hearing loss was about the same for high and low frequencies, which is very unusual for one having as great a hearing loss as he had. The fact that he could understand speech when one shouted into his ear encouraged me to think we could make a hearing aid with which he could hear ordinary conversation and we agreed to do this. The necessary equipment was placed in a box about 16 × 8 × 8 inches; the microphone was on the end of a cord and [the] headpiece on a head band. Instead of a battery for a source it had [a] power pack which operated on AC current.

When I took this hearing aid over to his office I had this interesting experience. When I came into his office he greeted me with the following, "I understand you are a fine mathematician." I admitted that I had studied a little mathematics. He then replied, "You know I started to study mathematics, but I got only as far as Algebra. When I struck the plus and minus signs in this, I became all confused and quit. And I haven't done anything with it since." This shows that at least in his day the lack of mathematical ability did not handicap inventability. He was a fantastic inventor but no scientist.

While I chatted with him in his office, I noticed that he had one of these old fashioned roll top desks. There were small drawers in the top part having labels on them. I noticed one was labeled taxes, another coal bills, and other such items like these. I was very much surprised that a man as noted as Edison did not delegate such matters to someone else. I understood from some of his close associates that he would not let anybody else do the work. I also noticed that he had a peculiar elevator connecting his three or four floors in his laboratory. He would not let an elevator company install a modern one. He made the one that they were using.

Then he said, "I understand you have a hearing aid there, that might help me." I said, "Yes, we would like to try it." Then he said, "I hope the gadget does not work on AC current because we have none of it in the place." As he said this one of his assistants in back of him started to wave his hands at me and whispered, "Yes, we have but the old man doesn't know it." So I only answered, "I guess we can make it work." It was well known that he had a great controversy concerning AC and DC especially for running street cars and always insisted on using DC. Because of this, the majority of the old hotels around New York were nearly all wired for DC and the street cars were using DC. We found he could hear very well with the hearing aid and he used it for a long time after that. He took it to some of the dinners at which he was a special guest. He was a much wanted celebrity. A few years later I met him and asked how it was working. He said it was O.K. "When I used to attend these dinners I sat in silence wondering what the after-dinner speaker was saying and wishing I could hear him, but I was content to turn my thoughts toward some of my inventions. But now with the hearing aid I can hear and understand the speaker but usually find it so dull I turn it off and turn my thought to my inventions." He did tell me the story of how he lost his hearing.

He said when he was just a boy he worked on a railroad train in the express postal car. While the train was stopped he was fooling and kidding the other man in the car who was much older and stronger than he was. Edison played some prank on the older man. To get quickly away he jumped out of the car door to the ground. The older man grabbed at his head and caught both ears and pulled Edison back up into the car. Edison felt a terrific pain in both ears and knew something had ruptured. Since then he had been hard of hearing. This is the story that Edison told. This incident may have been a primary cause, but there must also have been a secondary cause that acted upon the auditory nerve, then or later, to produce the profound deafness that he had. He said it had not handicapped him very much, particularly when it was necessary to solve a perplexing problem.

I had another experience with Edison as follows. His first assistant (the foreman) in the research laboratories came to my office one day and said he would like to have an audiogram of his hearing to compare it with Edison's. He said Edison insisted upon passing upon every phonograph record that went into stock for sale. He would hold one end of a morning glory horn against the phonograph loud speaker. The other end was fitted with a rubber tube which he fitted into his ear. In this way he passed or rejected every record that was sold. The foreman said, "You know and I know that he doesn't hear these records like a person with normal hearing, but I just cannot persuade him to let us do this inspection work. Now, if you make an audiogram for me, we will compare it with Edison's audiogram which you made and probably this will convince him." Well, I made the audiogram for him. It was normal through most of the speech frequency range but dropped very badly above 2000 cycles. Edison's audiogram was much lower but about the same through the whole frequency range. The foreman sort of frowned when he saw the audiogram but thanked me and left. I think he discussed the audiogram with Edison, but I think Edison kept on testing the records in the same old way.

My father's work in aiding the hearing handicapped was not confined to his work on the audiometer; he also worked to improve hearing aids. Mr. DuPont, after his disillusionment with his audiologist, pleaded for something to help his hearing at meetings of his Board of Directors. He asked my father for assistance, and a binaural hearing set was devised. Two microphones were placed in the middle of the board-room table, and the telephone receivers were placed in a headband for Mr. DuPont to wear. The amplifiers, transformers, and condensers were housed in a cabinet under the table. The binaural system enabled him to sense the direction of the speaker, as well as to clearly understand the speech. This was the first hearing aid using vacuum tubes and was in use for many years in the DuPont board room. It was not practical for ordinary use, since it carried a price of $5,000. However, it did lead to the manufacture of simple monaural versions by Western Electric Company.

In 1921 my father began publication of some of his findings about speech and hearing. The first article was "The Frequency Sensitivity of Normal Ears," published in the *Proceedings of the National Academy of Sciences*, November 1921. This was followed by numerous articles throughout the 1920s and culminated in the publication of his book, *Speech and Hearing*, in 1929 by D. Van Nostrand & Company. This book became widely used throughout the world as the definitive work in the field. A review of the book in the *Journal of Speech and Hearing Disorders* stated:

> His book is an embracing summary of a unified research program without equal in the field. The description it offers of the physical characteristics of speech and the analysis it presents of auditory phenomena, particularly in regard to the hearing of speech, have become classical. . . .The intervening years have brought added respect for both the stability and the scope of the pioneering contributions made by the author and his coworkers.

By the mid-1920s, my father had attracted enough attention to the importance of his research work in acoustics that a small group in the research department of Western Electric Company was formed, and in 1928 he was made director of acoustical research, part of the Physical Research Department in the newly formed Bell Telephone Laboratories. By 1935 he was made director of all physical research at Bell Laboratories.

He was also attracting attention outside the Bell System. In 1920 he was elected a fellow of the American Physical Society and in 1921 a fellow of the American Association for the Advancement of Science. In 1922 he became a member of the American Institute of Electrical Engineers and in 1926 a member of the Board of Managers of the American Federation of Organizations of the Hard of Hearing. For the latter organization, most of whose members were hard of hearing, he devised a multiple hearing aid, so that attendees at meetings of the federation could hear. He described the first use of this equipment as follows (Autobiography, p. 57):

> As I remember it small strips were made with five or six jacks and attenuators wired in parallel. Any number of these strips could be connected together by simply plugging connecting cords into the end jacks. There were about 20 of these strips so that about 100 head pieces could be connected in parallel to the transformer coming out of the amplifier. These were arranged according to the seating in the hall. As the hard of hearing person sat down, he could reach for a head set and then turn the attenuator until he obtained the loudness with which he could best understand. The first installation was at Hotel Astor at the annual convention. When everybody became quiet for the beginning of the meeting, they asked me to talk and explain how to use these sets. Some of the people could hear and helped the others to adjust the head bands. When they all had finished fussing with the head band I started to talk slowly in a normal volume and greeted them and told them to adjust the attenuator if they needed more volume. Most did not but sat with tears running down their

cheeks crying, "I can hear! I can hear!" It was the first time most of them had ever heard a public speech. After they quieted down I turned the microphone over to the president and he carried on the conference. It was a memorable occasion and these hard of hearing persons never forgot it. The Bell Labs donated the system to the national organization and it was used for many years after that time. The officers of the Federation took *their* group hearing aid all over the USA for meetings in various localities.

In 1929, my father was elected president of this organization. In the same year he was asked by Mayor James Walker of New York City to be a member of the city's Noise Abatement Commission, headed by C. C. Burlingham. This commission studied and measured the sources of noise in the city and issued a report on their findings. Although many of the sources of noise identified in the report were thought to be unavoidable, steps were taken to eliminate or ameliorate some of them, and antinoise ordinances were adopted by the city.

In May 1929 the Acoustical Society of America was formed, and my father became its first president. This brought together teachers, engineers, and others interested in the broad field of acoustics. The first issue of the *Journal of the Acoustical Society* was published in 1929. In recognition of his work in the field, my father was named an honorary member of the Acoustical Society in 1949. At that time, only Thomas A. Edison had been awarded such an honor.

Most of those who are speech handicapped have hearing difficulties. However, it came to my father's attention that some individuals, who had learned to talk normally, were deprived of this ability by the removal of the larynx, usually as a result of cancer surgery. Since my father had already devised a vibrating reed mechanism to study speech sounds, it was a relatively simple matter to devise an artificial larynx to help those who had lost their larynx through surgery.

The Bell Systems, therefore, manufactured and offered for sale to such individuals an artificial larynx. My father received much publicity around the world for this invention, and, though designed for a relatively small class of handicapped people, it was almost miraculous to those who regained their speech through it. I remember a chance meeting with such an individual while with my father on a tour. The gratitude shone in his eyes as he thanked my father over and over again through his artificial larynx for making the resurrection of his speech possible.

Another outgrowth of this basic research into acoustics was the development of recording and reproductive apparatus that would faithfully transmit the entire audible frequency range. This made it possible to synchronize sound with a motion picture and produce talking pictures to take the place of the prevalent silent ones. When the motion picture companies were ultimately convinced that the public wanted talking pictures, my father was besieged with attractive offers from Hollywood to help engineer their introduction, but he resisted all offers and remained with the Bell Telephone Laboratories. However, for many years most of the sound pictures were made under Bell System patents. Initially, these were projected on narrow screens already in use in the theaters, and no attempt was made to give them a spatial effect, but my father knew that a spatial effect could be created by the use of a binaural system, such as he had provided to Alfred DuPont for his board meetings.

My father was prepared to demonstrate this spatial effect, which he initially called auditory perspective but that has become better known as stereophonic sound or simply stereo. The principal exhibit of the Bell System at the 1932 World's Fair in Chicago was such a demonstration. Twenty or thirty earphone headsets were placed in a semicircle around a glassed-in stage, where a dummy called Oscar sat. Microphones were placed in each ear of the dummy and were connected to the corresponding receivers in each of the headsets. A person would be walking around the dummy on stage and talking. The glass prevented any communication through the air, but those individuals who had the headsets on were startled. It seemed to them that someone was walking around them and talking to them. They would turn and look over their shoulders to see who it was.

At about this time, my father contacted Leopold Stokowski, conductor of the Philadelphia Orchestra, concerning the possibility of utilizing a stereophonic sound system in a concert of the orchestra. He had approached many of the conductors of the leading orchestras of the time as to the feasibility and desirability of such a demonstration, but he had received a very cool response. Only Mr. Stokowski was enthusiastic. My father described these early tests in his autobiography (Autobiography, pp. 90–93):

> It was about 1931 when I first met Stokowski and we made tests of stereophonic sound down at the Academy of Music in Philadelphia where the Philadelphia Orchestra held its concerts. There was a spare room in the Academy building which was large enough to house the orchestra so that we could have them play on the stage and listen to it up in this large room. In this way we tried experiments until we felt we had developed a stereophonic system. Originally, the theory of this system was that it should have an infinite number of loud speakers at one end and the same number of microphones at the other end. However, we found that in stage productions, three microphones, three transmitting lines and three loud speakers were sufficient. I'll not go into the details of the development work that was necessary to produce this. There are several printed papers on it. However, we did make nine loud speakers expecting that we might have to use three across and three up and down. However, we found that most of the action was horizontal and consequently, three loud speakers were sufficient.
>
> For this demonstration the Philadelphia orchestra was in Philadelphia. It was conducted by the Assistant Director Smallen, and Mr. Stokowski was

at the controls in Washington, where the orchestra pieces were reproduced. I'll not go further into the details except to say that there was a very select audience there. The Cabinet to the President, Senators, and Representatives, and other officials of the Government were invited guests, and it was under the direction of the National Academy of Sciences. The President of the Academy at that time was Dr. W. W. Campbell. He introduced me, and I will quote from the little booklet which tells about this. "With the assistance of the orchestra in Philadelphia, Dr. Fletcher then performed several experiments to demonstrate the important characteristics of the new apparatus. On the stage of the Academy of Music in Philadelphia where the pickup microphones were installed, a workman busily constructing a box with a hammer and saw was receiving suggestions and comments from a fellow worker in the right wing. All the speech and accompanying sounds were transmitted over the cable circuits to the loud speakers on the stage of Constitution Hall in Washington. So realistic was the effect that to the audience the act seemed to be taking place on the stage before them. Not only were the sounds of sawing, hammering, and talking faithfully reproduced correctly, but the auditory perspective enabled the listeners to place each sound in its proper position, and to follow the movements of the actors by their footsteps and voices. For another demonstration the audience heard a soprano singing 'Coming Through the Rye,' as she walked back and forth in an imaginary rye field on the stage in Philadelphia. Here again her voice was reproduced in Washington with the exact auditory perspective. The singer appeared to be strolling on the stage of the Constitution Hall.

An experiment which demonstrated both the complete fidelity of reproduction and the effect of auditory perspective was performed by two trumpet players, one in Philadelphia at the left of the stage of the Academy of Music, and the other in Washington at the right of the stage of Constitution Hall but invisible to the audience. Alternately they played a few phrases of the same selection. To those in the audience there seemed to be a trumpet player at each side of the stage before them. It was not until after the stage was lighted that they realized only one of the trumpet players was there in person. The music of the other was transmitted from Philadelphia with such perfect fidelity and reproduced into such true prospective [sic] that it was impossible to tell that one of the players was absent. The auditory perspective effect is not restricted to placing sounds in their correct position across the stage but is 3-dimensional. This was shown by having several sources of sound move around the stage in Philadelphia. Not only back and forth but high up in the center of the stage as well. The movement of each sound was faithfully reproduced by the loud speakers in Washington. Even when the sounds were carried high above the level of the floor." I think that's sufficient to indicate what this first demonstration was. I might say that it was a tremendous success.

This demonstration was with a live orchestra, instrumentalists, and singer. The next step was to record and reproduce stereophonically. This was accomplished and demonstrated in 1939 in Carnegie Hall. I was in the audience for this demonstration, and I remember vividly one aspect of it. A dancer appeared before the curtain in the middle of the stage and began a tap dance. After a few minutes, the dancer danced off to the left apparently still tapping, but the sound of the tapping went off to the right, confusing the audience. The sound was actually coming from a recording, not from the live dancer.

I also remember that the volume on some of the orchestral numbers became so high that, not the rafters, but some of the fixtures in the old building began to ring.

This demonstration was repeated at the Eastman School of Music and in a Hollywood theater.[5] The records of these early demonstrations of stereophonic sound have been preserved at Columbia University. They are on film rather than the magnetic tape or metal disks currently in use in the recording industry. It was on the basis of these demonstrations and the scientific papers describing them that my father came to be dubbed the "Father of Stereophonic Sound."

As noted above, in 1933 my father's responsibilities at Bell Laboratories were broadened to include all physical research. As such, he supervised the work of William A. Shockly, Walter H. Brattain, and John Bardeen in the development of the transistor; Dean Wooldridge in the development of magnetic tape; James Fiske; Charles Towne; and other outstanding physicists.

Just as Bell Laboratories had recognized his organizational ability and put him in charge of physical research, outside organizations in the scientific community were recognizing and calling on the same ability. In 1931 my father was elected a member of the Executive Committee of the American Institute of Physics, which he had helped organize. In 1933 he was elected to the Board of Directors of the American Association for the Advancement of Science. In 1945 he was elected president of the American Physics Society. In 1947 he was appointed to the Committee on Hearing, Division of Medical Sciences, National Research Council. In 1948 he became a member of the National Research Council assigned to the Division of Engineering and Industrial Research for a three-year period. In 1949 he was appointed to a three-year term as a member of the Standing Committee on Meetings of the National Academy of Sciences. He had been elected to membership of the Academy in 1935.

During the period of U.S. involvement in World War II, from 1941 to 1945, my father was again called upon to work for the war effort. He was very circumspect about his involvement. All he states in his autobiography is (Autobiography, pp. 103–104):

> During the war period, my research was in the general field of acoustics and was under the general supervision of O.S.R.D. I had charge of groups working at:
>
> Harvard University,
> California Institute of Technology,
> University of Pennsylvania,
> North Carolina State College,
> Rutgers University,
> Stevens Institute of Technology,
> and three or four groups at the Bell Labs.
>
> Most of this work was secret and a group effort. I received several citations for this work, one signed by President Truman.

It is now presumed that his principal work involved the detection and identification of underwater sounds.

After the war my father devoted most of his efforts to fulfilling his various executive responsibilities, but he also worked strenuously to complete two projects before his impending retirement from Bell Laboratories. The first was "Empirical Theory of Telephone Quality," a project that had captured his attention off and on for twenty-five years. The second project was "The Dynamics of the Cochlea." This research, and other developments since the original publication of *Speech and Hearing* in 1929, were incorporated into a new book brought out in 1953, *Speech and Hearing in Communication*, also published by D. Van Nostrand.

Upon retirement from Bell Laboratories at the age of sixty-five, my father accepted a position as professor of electrical engineering at Columbia University, a position he held for three years. During that time he organized a department on acoustics and persuaded Bell Laboratories to donate some of the stereophonic apparatus, which had been used in his demonstrations, to Columbia. Cyril Harris, who worked with my father during these years, succeeded him as head of the acoustical engineering department at Columbia and made many valuable contributions to the field of acoustics.

My father moved again in 1952. He accepted a position as director of research at his old school, Brigham Young University. He was able to increase the amount of grants for research being given the university and to get the university to use part of these grants to fund the salaries of faculty members and students engaged in the various research projects.

In 1953, at the behest of the president of the university, my father began organizing an engineering course for the university and became chairman of the Department of Engineering Science. This ultimately became the College of Physical and Engineering Sciences. In 1958 he was able to induce Armin J. Hill to be dean of this college, and thus relieved him of the administrative responsibility. After two years, during which he taught mathematics at the university, he again turned his attention to research. From 1960 until his death in 1981, he actively pursued his study of musical tones. He had a vision that, as more was learned about the characteristics of the various musical instruments, completely new instruments would be invented, which would broaden the listening pleasure of music lovers. He analyzed the sounds of the piano, organ, cello, violin, and bass drum, etc.

Although intensely interested in his scientific pursuits, my father did not neglect his recreational opportunities. As a boy he was an ardent fisherman, and this interest lasted throughout his life. He enjoyed all sports, playing tennis and basketball and watching other sports. He enjoyed fine music. I remember as a boy listening to Metropolitan Opera stars, such as Caruso and Galli-Curci, and instrumentalists like Heifetz on our old phonograph. He was active in promoting camping and boy scout activities.

My father was also a devoted family man. He and my mother, Lorena, who was named American Mother of the Year in 1965, had seven children, six of whom lived to maturity. Of the six, he inspired five to follow in his footsteps. His daughter, Phyllis, worked on the editorial staff of the American Institute of Physics. James, Robert, and Paul all earned Ph.D.s in physics, and Harvey J. earned a Ph.D. in mathematics. All have had successful careers in teaching and in industry and government. Although I was the oldest son, and therefore somewhat closer to my father than the younger children, I did not pursue a scientific career, but with his help and encouragement became a lawyer.

After my mother's death in 1967, my father married her sister, Fern Eyring. This marriage lasted until 1981, when both died.

A memoir of Harvey Fletcher would be incomplete without mention of the inspiration he gave his children, his students, his associates, and the thousands who have read and still read his publications. He had a deep religious conviction, which he expressed in books and articles and in countless sermons. Indeed, his life was so full and rich that he wrote a manual for Latter-Day Saint Sunday schools, entitled *The Good Life* (1963). He never felt any insolvable conflict between science and his religious beliefs. He will be remembered fondly by those who knew him and with respect by those who know of his contributions.

He was a working scientist until the very year he died at the age of ninety-six. The curiosity that propelled him as a boy, a young student, a researcher, and leader of others continued to the very end. His influence will continue to be felt for years to come by those who knew him and for those following in his very large footsteps.

NOTES

1. Autobiography of Harvey Fletcher. Unpublished.

2. My father was reluctant to publish anything related to his part in the now famous oil drop experiment for fear that it might detract in some measure from the credit given Professor Millikan, whom he admired and esteemed as a friend. This is the principal reason that his autobiography was not published. After my father's death, an article by Mark B. Gardner, an associate at Bell Telephone Laboratories, concerning the oil drop experiment was published in *Physics Today,* June 1982, pp. 43–47.

3. In 1927 this department was combined with the much smaller research and development group at AT&T to form Bell Telephone Laboratories.

4. A detailed description of this device is to be found in his autobiography (Autobiography of Harvey Fletcher, unpublished, pp. 48–49).

5. The Mormon Tabernacle Choir's music was also recorded in stereophonic sound for use in these demonstrations.

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