R. E. GIBSON

REMINISCENCES

Islay and Edinburgh (1901-1924)

When I was nine years old, we lived in a small village of about 80 people on an island called Islay off the coast of western Scotland. If you went due west, you were at the Atlantic Ocean. We ran barefoot in the summer and had a wonderful time making up our own games in the winter when there was a lot of time on hand.

There I learned to appreciate the sound of bagpipes. When you hear them on a summer evening played miles away by a piper coming across the silent moors, it's an experience you will never forget. I learned also what silence meant. You could hear a startled covey of grouse five miles away. The other experience was darkness – no street lights, no light reflections in the sky at all. And when at night a mist came blowing in from the Atlantic, there was absolute darkness.

My interest in music started as a chore that my mother forced on me. She was quite a musician, playing the violin and the piano. She promoted music in the village and also played the harmonium in church. The church service was held in the school. There was only one schoolroom, and all the elementary classes were held there. This is where I got my early education, and I am rather glad of it, too.

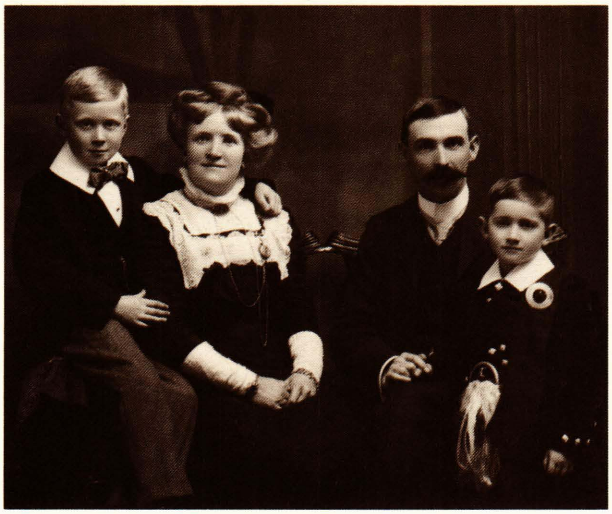
This article is based on a 1976 filmed interview with Robert W. Hart. Transcripts were edited and arranged in a historical sequence.

When I was about thirteen years old, a new assignment for my father took the family to Edinburgh, with an opportunity for good education. In 1914, I went to The George Watson's Boys College, as it was called, just before the outbreak of World War I. The boy next door was three years older and was to become an apprentice to an analytical chemist. He was very keen on chemistry, and it was thanks to him that I first became interested in chemistry.

In 1916, the University of Edinburgh Chemistry Department, under Sir James Walker, built a munitions factory. It was manned by the junior members of the faculty and their students. I got a job with them, even though I was still in high school. We were to produce 20 tons of TNT a week, and there wasn't a week that we didn't get a bonus for the extra tons over 20 that we turned out.

That experience changed my mind about becoming an industrial chemist. I wanted to go to the university and become a research chemist. In 1918 the war finally ended, and in 1919 I found myself at the University of Edinburgh studying chemistry. Professor (later Sir) James Walker was at the time busy designing a new chemistry department, laboratories and all. In company with a number of students, I helped to move the old chemistry department equipment from downtown Edinburgh to the country, where the new laboratory was established. While that went on, I received my bachelor of science degree and continued toward a doctor's degree. When the thesis was about

Islay Harbor



Gibson (left) with parents and brother Hubert (1909)

to be written, I received an invitation to join the Geophysical Laboratory of the Carnegie Institution of Washington. That offer came at a very good time. It gave me one month in which to write the thesis and submit it.

Geophysical Laboratory, Washington, D.C. (1924-1940)

I arrived in the United States in May 1924 without any immigration problems at all. The following month the law that exempted members of a learned profession from quotas was repealed.

Two people at the Carnegie Institution exerted a considerable influence on my thinking and outlook. Dr. Leason H. Adams, whose assistant I was, had started life as an engineer, but became a research scientist. He was one of the most thorough people I have ever met. He was a wonderful experimenter. His experiments had to be just right, and he followed them wherever they led. If they led him into a new, unfamiliar field, that didn't deter him at all.

He read and he asked questions. When he had to know something about thermodynamics, he read Willard Gibbs in the original and learned the mathematics needed to understand him. When his work on high pressure led him into the depths of the earth, he became interested in the propagation of earthquake waves. Again, he learned about the propagation of the transverse and longitudinal waves by reading up on all the work that had been done, becoming an authority on earthquake waves, particularly from the point of view of physics. And similarly, when he was interested in the annealing of optical glass and strains in transparent materials, he read Fresnel in the original and understood it.

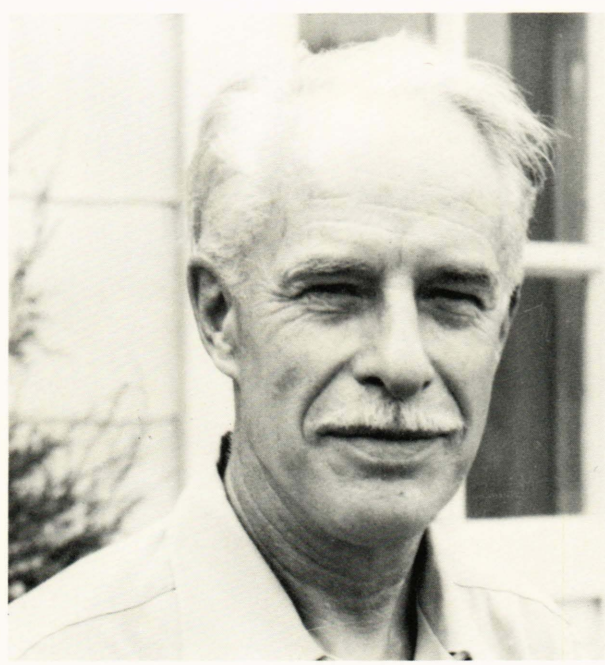
When I hear about the necessity of retreading engineers and scientists, I think back on this man who didn't have to go anywhere to get retreaded. He never lost his tread. Books and conversation with his colleagues were his way of keeping up to date. He did so until he was well over eighty.

Dr. A. L. Day was the director of the Geophysical Laboratory. From him I learned a good deal about the administration of research. The laboratory wasn't very large, but it became world famous during his regime in a very informal way. Nobody cared when you showed up for work in the morning and nobody asked when you left at night. As long as you were interested in doing your work, as long as you were turning out results that were in the fields of interest to the laboratory as a whole, Day left you alone, providing equipment and facilities, and always taking an interest in your work and encouraging you to carry on. In the meantime, he spent a great deal of his time with his own research, particularly in the field of volcanic action.

From these two men I learned the importance of thoroughness, that knowledge is available to you if you look for it, and that if you have enough determination and interest to read, you have no need to be afraid of new fields. I learned from Day the persuasive, kid-glove method of administration of research, the idea of producing results that represented the thinking of all the people involved and not something dictated by the head of a laboratory.

The job with the Carnegie Institution took me into mathematics (but not much), a good deal of geology, and a lot of physical chemistry. It was a very happy place in those days – in fact, still is. The objective was to bring together people interested in doing research in certain fields and leave them alone to carry out their research ambitions.

It is very nice to have time to carry out research of your own interests. In scientific research you deal essentially with inanimate systems. You have to be ingenious enough to arrange circumstances in the prop-



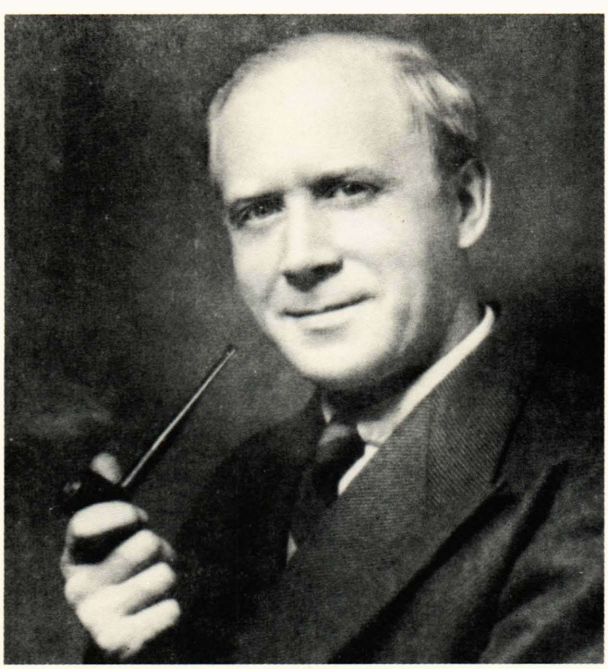
Leason Heberling Adams, Carnegie Institution of Washington er way, and then the systems will perform as you expect they will. They may also perform in an unexpected way, which presents you with an interesting, but not disturbing, problem.

A different aspect of living comes when you deal with people. People don't always perform as you expect them to, and when they don't, you get problems that may be interesting, but are sometimes exasperating.

I was awarded the Hillebrand prize in 1939 for work on the effect of pressure on solutions. Things were getting really very interesting. I had hit upon a vein of examining the effects of temperature on some systems at constant volume, and a lot of interesting results were coming out.

While at the Carnegie Institution, I also taught at The George Washington University – for 17 years. I found it a very refreshing experience. I learned as much myself as I ever taught the students. I found it interesting to teach subjects with which I had only a very superficial acquaintance. In order to do so, I had to keep a few days ahead of the class and the textbooks, at least during the first year. During the second year, I had the material in my head and could rearrange it to make it aesthetically appealing to the students. During the third year this process could be refined even further. But after the fourth year, I became bored with the subject and went on to teach something else.

We had very interesting students. At that time the Civil Service in Washington attracted the best people from the universities to the government laboratories. They came usually with a bachelor's degree and wished to go further. They were not only very well educated in the fundamentals, but were also very interested in working. A number of my students became directors of bureaus; one even became an Assistant Secretary.



R. E. Gibson – as retiring President of the Philosophical Society of Washington (1941)

Washington was a very pleasant place in those days. One could indulge almost any taste. I was interested in scientific activities, and there was a large number of very good people associated with the government and other laboratories. Somewhere around 1925, Gregory Breit (who was then at the Department of Terrestrial Magnetism) decided to organize a small colloquium that met once a week, at which Breit expounded on papers that he had just read from people like Heisenberg, Schrödinger, and others. The quantum theory and the new quantum mechanics unfolded before our eyes in Breit's excellent lectures and the discussions that followed. It was a very interesting colloquium, but was only one of many.

In those days we didn't listen to lectures. We listened to papers by our colleagues, had a good time discussing them, sometimes rather heatedly, but always in good humor. There was a great deal of scientific activity in Washington, and the whole atmosphere was much less dominated by political considerations. One could go to parties and never trip over lobbyists or promoters.

The War Years (1940-1946)

Officially, I was at the Carnegie Institution until 1941. But the coming of the war led me to accept a temporary assignment a year earlier. All permanent changes that I have encountered started with such an air of temporariness; something undertaken for a short time that would extend over years. In the early 1940's, Dr. Vannevar Bush mobilized a large number of scientists, particularly in the physical and chemical sciences, and some engineers, to consider ways of improving the defense position of this country. He organized the National Defense Research Council, of whom the vice chairman was Richard C. Tolman, one of the most outstanding persons I ever met. He did a few odd jobs on barrage balloons, but that very soon led him into the field of rockets.

Rocketry was a black art in those days. I don't believe that its full conversion into technology has taken place even today. However, we made an effort then, and one of the methods was to enlist the services of a number of competent scientists to study the problems, particularly in the combustion of propellants and in the design of the chambers in which they were burned.

One of the leading spirits in rocketry at that time was Dr. C. N. Hickman from Bell Telephone Laboratories. Hickman had worked with Robert H. Goddard during the First World War and in 1918 had developed a solid propellant rocket that never saw use because the war ended. In the 1920's and 1930's, military interest in rockets vanished entirely. When Hickman made proposals to both the Army and Navy in 1940 concerning the desirability of building and developing rockets, the Army showed a complete

Closing-out ceremony of the Allegany Ballistics Laboratory, Cumberland, Md. (1945). From the left, R. E. Gibson, Director; F. Hovde, Head, Division 3, Office of Scientific Research and Development; and Vannevar Bush, Chairman, Office of Scientific Research and Development.

lack of interest. The Navy found one small use to support Hickman in his work. I became Hickman's assistant, and we got together a number of chemists who were interested not only in making a contribution to the national effort, but also to the physical chemistry of rockets. Among the first was Alexander Kossiakoff, who came as an assistant to me and who has been with me ever since. Later we were joined by Frank T. McClure, Richard B. Kershner, William H. Avery, and others whose names are still familiar.

This effort grew from small beginnings. The Office of Scientific Research and Development, which had taken over the National Defense Research Council, placed a contract with The George Washington University to staff a fairly sizeable rocket effort in the East. A very substantial effort had also begun in the West at the California Institute of Technology under Theodore von Karman. A laboratory was founded near Cumberland, Md., where an ordnance plant, largely designed to produce 50-millimeter caliber cartridges for the Air Force, had completed its mission. Magazines for storing explosives and plenty of grounds were available. Under The George Washington University's direction, it became the Allegany Ballistics Laboratory with the mission to carry on research and practical development of artillery rockets.

When the war came to an end, the professional staff of the Allegany Ballistics Laboratory consisted of some people who had been members of universities and a large number of graduate students. We persuaded the latter to go back to school and finish their work. The Hercules Powder Company was asked to take over the sponsorship of the Allegany Ballistics Laboratory.

Around the latter part of 1945 there was a feeling that, even though hostilities had come to an end, the war-causing ambitions had not. A number of people, both from the proximity fuze development at the Applied Physics Laboratory and the rocket development at ABL, felt that their services were still needed. This concern was supported by some far-seeing military men. The guided missile program at APL was continued. Today this effort would be called a high risk undertaking and possibly one that the government should not indulge in. The intent was to devise a missile that flew at speeds greater than sound, that was launched by large rockets, propelled by ramjets, and guided to follow a radar beam. All of these technologies were still in the future. A great deal of work had to be done, not only in development and engineering, but also in research.

At that time, many things that people thought were established turned out not to be so. Among these was the development of propulsion engines for supersonic speeds. Ramjets were still in the conceptual stage; in fact, their feasibility still had to be demonstrated.

Rockets were another matter. The development of solid fuel rockets, adequate to launch and propel missiles of the speed and size emerging from the APL program, turned out to be much more than a few months' job. In fact, it was many years before a really satisfactory solution was obtained. It is interesting to note that it became a step in the scaling-up of solid fuel rockets, from the small sizes used in the war (weighing ten pounds or so) to rockets in which the propellant charge weighed several tons. It was a step to the large engines that are now used for strategic missiles and for launching spacecraft.

Applied Physics Laboratory, Silver Spring, Md. (1946-1954)

In 1946, administrative difficulties arose in the Applied Physics Laboratory. As in many other organizations that had been set up to aid the war effort, it was divided into two camps. One school of thought insisted that the time had come for those who participated in this work to return to their activities of the 1930's. Those who had come from universities would return to teaching and carry on their research investigations. Those who had come from industry would return to the engineering activities they had left.

The other school of thought, particularly prevalent at APL, was that the Laboratory had undertaken a commitment for the Navy to deliver a working supersonic guided missile. People were willing to devote their lives and their energies to making good on this commitment. Also, the prevailing political climate did not induce a great deal of optimism in those who thought seriously about what was going on in the world.

There were bitter cross currents. APL then was a rather large engineering laboratory, sponsored by The Johns Hopkins University. Questions arose concerning the appropriateness of a large laboratory of this kind, with the attendant financial responsibilities, within the structure of a university.

An experiment in management was tried. The University brought in an industrial company, whose function was to sponsor the engineering activities of the Laboratory (those that were performed on a fairly large scale and that carried with them a large financial responsibility). The University, on the other hand, would sponsor the research activities of a staff of 70 to 100, whose main business would be guided missiles and basic research in related subjects in physics, chemistry, and engineering.

This complex administrative structure was terminated in early 1948, when the University took over the entire responsibility for the operation of the Applied Physics Laboratory. The Laboratory itself became a Division of the University, parallel with the other schools, such as the schools of Arts and Sciences, the Medical School, and the School of Hygiene and Public Health.

One thing everybody agreed to was that if a place like the Applied Physics Laboratory were to continue as an active organization, it should put a great deal of emphasis on research. And, indeed, early in 1946, a high altitude research program utilizing V2 rockets was started by Merle A. Tuve and directed by James A. Van Allen. One question was not in doubt – that whatever we did, we should improve the research effort and spread the research environment that is characteristic of an academic institution. This research effort was strengthened by the establishment of the Research Center in 1947.

The Korean War made a very significant change in the Laboratory's work, in the confidence with which people pursued their endeavors, and in the increased activity of the staff to accelerate the development of missiles to where they could be deployed. The Department of Defense brought in K. T. Keller, who was president of the Chrysler Corporation at that time. His job was to get certain missiles into a form in which they could be used by the appropriate services. He chose, among others, the Terrier (which was in its latter stages of development) for mass production. This involved a large number of unexpected headaches. It not only accelerated the development, but it turned the Laboratory's attention very strongly toward the engineering and even the production engineering aspects of guided missiles. We thought at that time, rather naively, that production engineering know-how was well established in industry. It turned out to be almost as big a research problem as the actual conception and development of the missile itself, to produce a missile that would survive in the atmosphere in which it was expected to operate and to take the stresses of a very rough environment.

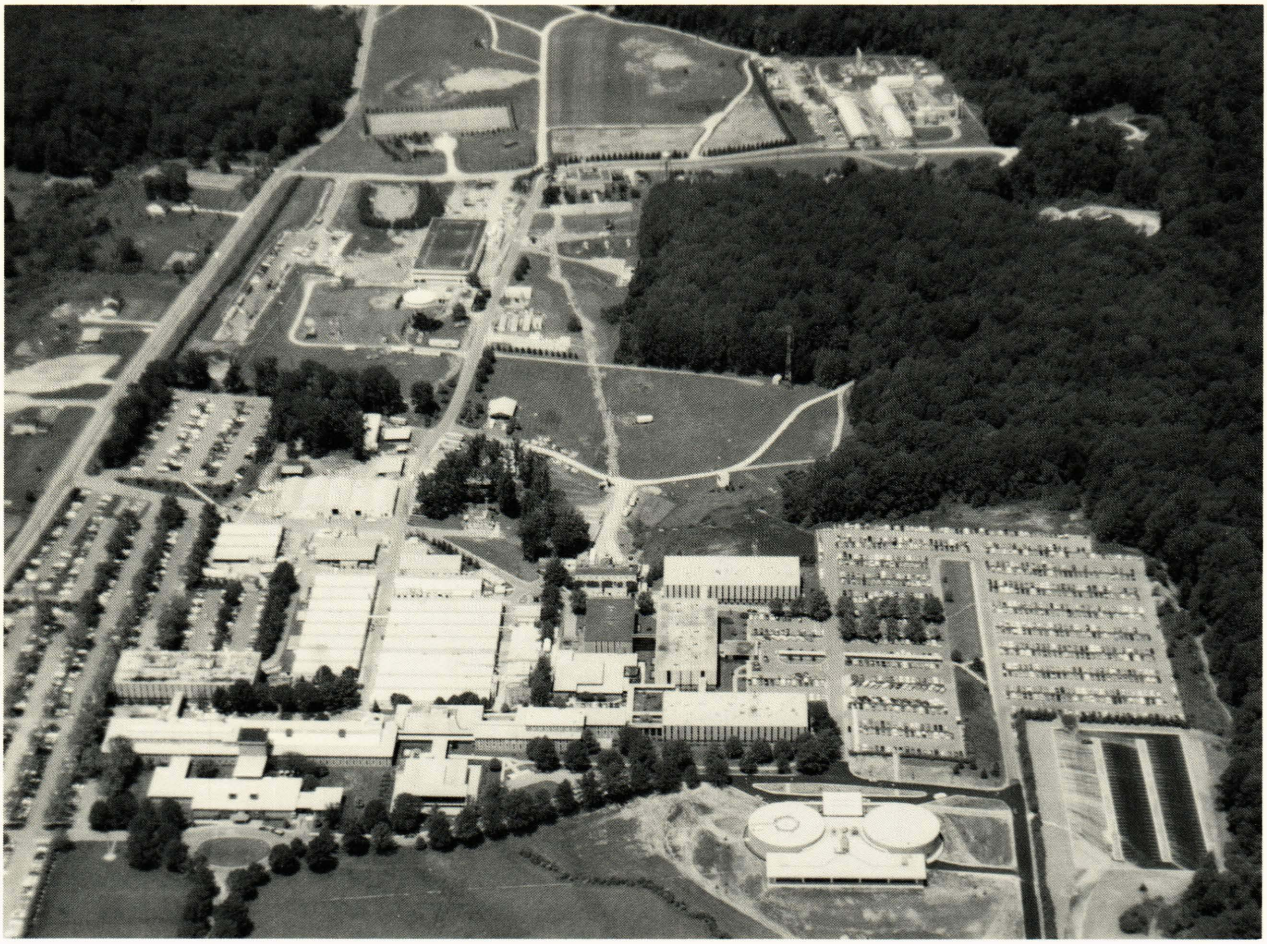
The Laboratory made one big contribution: it designed a missile as an assembly of interchangeable parts in which the parts themselves could be tested under many conditions, and then connected together to form the characteristics of the system of which they were a part.

This increase in activity vindicated somewhat the school of thought that felt that the cessation of hostilities in 1945 had not lessened the values of laboratories such as APL to the government. It also increased the demands on space in the Laboratory. The main laboratory, situated in Silver Spring, Md., could not be expanded. The nearby Forest Grove facility consisted of temporary buildings built during the war to last only a short time. It did not seem profitable to increase the accommodations there. We therefore looked for a site in the country, a site that we could afford and on which we could build a laboratory in which utility and aesthetics were combined in buildings that were pleasant to look at, comfortable to work in, surrounded by the amenities that the countryside could afford.

Three considerations were uppermost in our mind. In view of the deepening relations with The Johns Hopkins University, we wanted to be located between Silver Spring and Baltimore. We wanted to find a tract of 200 to 400 acres on which we could build a laboratory with plenty of room for expansion without fear of disturbing our neighbors. We wanted a tract we could afford. The decision was fairly easy after we found the present site on the farm on what is now called Johns Hopkins Road. It was big enough (about 350 acres) and the price was about one-tenth what it would have been a few miles nearer Silver Spring. The site has fulfilled our expectations, and we never regretted having made the decision. The staff could feel that they not only had a home of their own, living in buildings of the University, but that it was a home that they could be proud of. It had the ingredients of an environment in which good work, good research, good development, and good engineering can be done.

Applied Physics Laboratory, Howard County, Md. (1954-1983)

When we moved in 1954, the Laboratory consisted of about 1200 people. By 1960 it had risen to 2500. Shortly after that we decided that we had grown big



The Johns Hopkins University Applied Physics Laboratory, 1983

enough in numbers and the next thing was to concentrate on growth in the quality and the quantity of our output. This has continued at an accelerating rate, as one may judge, for instance, by the volume of publications from the Laboratory. The quality of these publications and their acceptance by scientific journals of established reputation are a growth of which we can be proud. Another aspect of the growth is the number of patents that were issued, indicating the engineering ingenuity of the staff. Lastly, there is the growth in the number of devices, in short the hardware, that the Laboratory has produced and which have been used all over the world. But our chief product is still people. The Laboratory is growing people of high caliber. Fortunately, it retains many of them, but others go elsewhere, carrying with them what they have learned here.

We were extremely fortunate in our inheritance; we inherited from the Silver Spring laboratory the attitude, the spirit, and many of the people who had come through the fiery furnace of the war in which it was necessary to mobilize the brain and the ingenuity of this country to provide us with weapons and implements to counteract the Nazi menace. Merle Tuve and others had gathered together a group of people who spared nothing to develop the proximity fuze and get it into action. This gave to the staff an attitude of dedication, of keenness to get a job done. They realized that no matter how elegant it might be from the inventor's or from the scientist's point of view, no task was complete until it was actually doing the job for which it was intended. That has left a lasting mark on the Laboratory.

A nucleus of good people is the basis of any organization, people who accept leadership and who have ideas themselves but, more importantly, who are broad enough in their judgments that they can adopt and encourage ideas from below rather than impress them from the top. The essence of a laboratory of this sort is to collect the right people, to encourage their ideas and, from the management point of view, to recognize opportunities; in short, to keep our eyes open for new avenues in which science and technology can be put to the service of the nation.