AUTobiography written at the request of the
"Project on the History of Recent Physics in the United States"
of the American Institute of Physics

1962

A. G. Shenstone

SEP 26 1962
My parents impressed on their six children, of which I was the youngest, their ideals of personal and intellectual integrity. My father, a successful businessman, was himself without university training but our house was full of books and all of us read avidly. My mother, who had been a school teacher before marriage, was very active in Church work of all kinds and was greatly beloved by everyone who came in contact with her. If there was any sibling rivalry between us children we were unaware of it.

One of my brothers, my senior by 12 years, was a brilliant student and later became a very well known surgeon, whom we admired as much for his humanity as for his abilities. Next in age to myself was a sister with whom I was very intimate since she was only a year and a half older than I was. It was she who always was a challenge to me from childhood until she graduated in high standing from Bryn Mawr. My father had urged several of us to go to universities in the States because, as he said, "You will probably be dealing with "United Statesers" later on in life and you should get to know them." We six in the end acquired fourteen degrees at eight different universities in Canada, the United States and England.

My own education started at a small private school at the age of four from which I passed, when six years old and able to read, to public elementary school where I stayed until I was twelve. From twelve to sixteen years of age I attended a collegiate Institute which in Canada is the equivalent of an American High School. I then took the examination for honours admission to Toronto University.

At the Collegiate Institute I had four years of Latin, four years of French and three of German, which was an alternative to Greek by choice of the student. The teaching in history, English and mathematics was exceptionally good. There was a very impressive course in botany and biology, but the smattering of physics and chemistry was almost a waste of time. I remember also a course in drawing, including perspective, which proved very useful in later life.

My choice of colleges lay between Dartmouth, which was being attended by a brother, and Princeton, from which a cousin, F. M. Harris, had
been graduated in 1907. He had gone to Princeton because of a summer friendship with Woodrow Wilson in the Muskoka Lakes. Princeton, with some reservations, accepted my Toronto matriculation examinations, and I entered as a rather young freshman in 1910. At the time I had no idea what I wished my life's work to be and, in fact, I remained doubtful for some years. However, my natural bent led me into physics and mathematics. When I entered Princeton there was still surviving an honours program in Mathematics and Physics in which the courses were taught by the most eminent members of the faculty. I particularly remember a sophomore physics course by W. F. Magie, a mechanics course taught by Oswald Veblen, calculus by Henry B. Fine, electricity and magnetism, using Jeans's book, by E. P. Adams, and mathematical analysis, using Goursat in French, by J. H. M. Wedderburn. This group of courses constituted a very thorough training in classical mathematics and physics, both theoretical and experimental, but the time was not yet ripe for the introduction of such things as the quantum theory into the undergraduate curriculum. In 1914, I was graduated as about the third man in the class, having been elected to Phi Beta Kappa in Junior year. At the time I was in Princeton, the overall standards were not as high as they are today. It was possible, however, for a student with the necessary desire to get as good an education in those days as he can nowadays both in the sciences and in the humanities.

In September 1914 I went to Cambridge University thinking, as many North Americans did, that the war which had started in August 1914 was a matter for professional armies and that it would not last many months in any case. I matriculated as a candidate for the B. A. in research, the only degree then available in Cambridge University. I listened to lectures by J. J. Thomson and did some preliminary experimental work under C. T. R. Wilson. However, there was only a handful of graduate students working in the Cavendish Laboratory and it soon became apparent that it would be necessary for everyone to take part in the war.

I applied for a Commission in the Royal Engineers and in March, 1915 I became a Second Lieutenant and was sent for training to Chatham.
The scarcity of engineer officers was so acute that only seven weeks were spent on our training, which included Infantry Drill, Military Engineering, Riding and Horse Management. There were forty officers in my class and all but one had come from abroad to join up.

In August 1915 I was sent to France and in September joined a famous British Division, the Seventh, just at the end of the Battle of Loos. Soon thereafter, the division was moved to the Somme where it remained throughout the Battle of the Somme and until after the Battle of Arras and the Battle of Vimy Ridge in which it played a flank supporting role. In the summer of 1917 we were moved up to the Ypres Salient where the Third Battle of Ypres was in progress. We took part in that battle during the late summer and early autumn. At that time I managed to get leave of absence for six weeks to go home to Canada and during my absence the division was sent to Italy, because of the grave defeat of our Italian allies.

It may be of interest to learn of some of the duties of an engineer officer in France during 1915 to 1918. We were in general assigned to support the infantry in every way possible. We designed and dug trench systems, sometimes in No Man's Land; we constructed dug-outs both shallow and deep, we put out barbed wire entanglements during battles; occasionally we built bridges with pontoons and trestles which we carried with us; we did demolitions of all kinds.

When I returned from my Canadian leave at Christmas 1917, I was assigned to command a training company at Chatham, but soon became dissatisfied and asked to be sent back to France. I went back to France in January 1918 and was given a job in the construction of rear-defences and on March 21st was working on the Oise on the flank of the great German attack which drove us back nearly to Amiens. For a period at that time every capable man had to put in long hours of work, frequently under fire, to bolster the defenses which were physically almost non-existent and very sparsely manned. The German advance was held in April and continued so until the 6th August, 1918, when the corps to which I was attached took part in the attack which General Ludendorf called "the black day of the
German armies." At that time I was working as a Field Engineer at Corps Headquarters with such new duties as water-supply officer, bridging officer and so on. The Corps was moved in the fall to the Flanders area and took part in the pursuit of the German armies into Belgium. In October, 1918, I was assigned to work at British General Headquarters on maps of the country ahead of us and was so engaged when the Armistice was declared. Then I caught the prevailing influenza and was sent from hospital to recuperate most pleasantly on the French Riviera at Cap Martin.

On return to duty I commanded an engineer company in the Third Division in the Army of Occupation in Germany until July 1919, when the division returned to England. While in Germany I made the first attempt to get back into scholarly work. I wrote to Sir J. J. Thomson and he very kindly sent me several of the most recent scientific books including Richardson's "Emission of Electricity by Hot Bodies", Milliken's "Electron", Richardson's "Electron Theory of Matter" and several others. After four and a half years completely divorced from theoretical study, I found intelligent reading of such books extremely difficult, but I persisted in my determination to at least try to become a physicist or a mathematician. In the Spring of 1919 I wrote to Dean Christian Gauss at Princeton of whom I had had the privilege of becoming a friend in my undergraduate years. I asked him to look into the possibility of my obtaining a fellowship either in physics or mathematics. The result was a fellowship in physics for the academic year 1919-20. My demobilization took place in August 1919, shortly after I had returned to Canada.

Beginning academic life after nearly five years as a soldier was very difficult indeed. One of my first experiences was to discover that examinations on which I had done very well as an undergraduate were now quite meaningless. I was unable even to understand the questions, let alone answer them. The fact is that I have never fully regained much of the knowledge I had as a pre-war student and this lack has always handicapped me.

There were only about four graduate students in Physics in Princeton in 1919, H. D. Smyth being the one whom I knew best then and of whom I
have been a colleague most of my subsequent life. I was considerably older than all of the other graduate students, and because of my age and my war service was even accepted as friend rather than student by several of the professors in the Physics Department, especially by E. P. Adams and H. L. Cooke, both of whom had served in the Royal Engineers in sound-ranging.

The courses in the department still reflected the pre-war classical physics, although electron theory was emphasized. To me the most impressive courses were those given by E. P. Adams in Mechanics and in Electricity. They stand out clearly in my memory. At that time graduate students started to do experimental thesis work from the first year. Professor Adams set me the problem of trying to find out if there was any connection between the conduction electrons and the photo-electrons. He was particularly interested in the Hall effect and its modification known as the Corbino effect. I therefore set up an apparatus to try to find any asymmetry in the photo-electric effect due to the Hall effect. I soon found this was a quite impossible task but I did observe a gradual change in the total photo-emission when a current was passed through the illuminated conductor. This led to my thesis in which I believed I had shown that the effect was real and not connected with heating or the evolution of gas. However, in the light of our present knowledge there can be little doubt that the effect is spurious, its cause still being obscure.

Having in 1914 started towards a degree at Cambridge University, I was determined to continue to that degree. I had an excellent fellowship for 1920-21 and I obtained permission to use it to spend two terms at Cambridge. Actually I went to Cambridge at Christmas, 1920, and returned in August 1921, having spent the most stimulating time of my student years. Rutherford had succeeded J. J. Thomson and had collected around him a brilliant set of young men anxious to test his very advanced ideas on nuclear structure. At that time the group included Chadwick and Ellis, both of whom had spent the whole war as civilian prisoners at Ruhleben in Germany, Blackett, Roberts, Stoner, Appleton. They had already produced the disintegration of nitrogen and were trying to find other similar effects.
Rutherford's mind was ranging far ahead on ideas which could not be tested with any accuracy by the existing experimental techniques of that time. For instance, he had set J. K. Roberts on to the problem of trying to detect the then hypothetical particle, the neutron. He gave me first a routine job of determining the relative numbers of alpha particles emitted by RaC and ThC when compared by their gamma activities. In the course of this work an American chemist, Professor Schlundt, and I counted over 50,000 alpha particles by the scintillation method. For my thesis for the degree (B. A.) I was set the problem of trying to observe short-lived artificial radioactivity, the hoped-for evidence being the emission of protons. The method consisted of bombarding the edge of a rapidly rotating wheel by means of a very strong source of alpha particles and looking for scintillations on a screen exposed to possible radiation from the part of the wheel which had just passed under the source. I obtained a negative result from every element I tried and we know today that this was correct. It may be of interest that the wheel I used had originally been part of the gyroscope on a model mono-rail car. It had been subsequently used by A. H. Compton in an experiment to test whether a high acceleration field would have any effect on the rate of radio-active decay.

On my return to Princeton, I tried to start work in nuclear physics but was ultimately unable to obtain a source of radium or radio-thorium. It is worth remarking that in the United States there was a general feeling that nuclear physics under Rutherford had gone about as far as it could and that it was apt to be a very unproductive field. Having completed my work for the degree of Ph. D., I began thinking of working in the field of critical potentials in which K. T. Compton was particularly interested. In September 1922 I took up a position at Toronto University as a demonstrator which I held for three years. Although the teaching was about fifteen hours a week with much paper-marking in addition, I started work on the critical potentials of copper and silver and carried it on until I realized, as others did at that time, that spectra would give the values much more easily and accurately. Toronto's Physics Department was then under J. C. McLennan and had one of
the most active research laboratories on the continent. It was very strong in spectroscopic research and was particularly productive in atomic spectra and in the elucidation of the auroral spectrum. The first observation of the \( \lambda 5577 \) Oxygen forbidden line, which is the most prominent feature of the night sky, was made in Toronto in the laboratory by G. M. Shrum. Perhaps more important still was the fact that the Toronto laboratory was the first place outside of Leyden where helium was liquified. I was present on the first occasion when success in this was attained and it was very exciting indeed.

While I was at Toronto, Karl Horovitz (later Karl Lark-Horovitz) came to Toronto from Vienna on a Rockefeller fellowship with the hope of doing the crystal structure of solid hydrogen. This he was never allowed to do, it being McLennan's own project. He did, however, do a number of other solidified gases successfully. Horovitz was, of course, later the creator of the first-rate research department of physics at Purdue University. We remained friends throughout his career.

My venture into spectroscopy was made under very difficult circumstances. Not only did I know nothing at all about it, but many difficulties were put in my way. I was forced for a time to work entirely at night when I could surreptitiously borrow the necessary spectrosopes. These difficulties perhaps made me more determined and, in any case, before I left Toronto in 1925 I had already become fairly expert both in experiment and in analysis, without ever having any instruction.

In 1923 I was married in England to Mildred Madeline Chadwick, whom I had met there through a mutual friend in 1914. Her influence on me from then to the present cannot be underestimated, not only in practical matters but much more in matters of the mind and spirit.

In the spring of 1925, Princeton decided to build up the Department of Physics by the appointment of four young men. These were C. T. Zahn, H. D. Smyth, L. A. Turner and myself, perhaps no longer a young man at 32 with a long and brutal war behind me. The University had provided funds for new equipment, my share going mainly into the purchase of a Hilger E.I. quartz spectrograph with which I worked almost exclusively for about six or
seven years, even finding it capable of giving resolved Zeeman effects to assist my analyses of various metallic spectra.

About 1930, K. T. Compton and J. C. Boyce changed their field of research to atomic spectra in the vacuum region. A one-meter instrument with 45° incidence was built and several papers resulted. They then built a two-meter normal incidence instrument arranged to photograph from the central image to 2500Å on a single plate 24" x 3". It is worth remark that it was possible before the second war to buy glass in this size of thickness .7mm, but that it is now impossible. In fact, I myself have such glass recoated for use in my similar instrument.

The Compton-Boyce instrument was never very satisfactory because it was built of flat pieces of $\frac{1}{2}$" brass and it was never possible to keep it vacuum tight for any length of time. It did, however, produce some very useful results after it was moved to M. I. T. about 1933 when K. T. Compton became President of that institution.

Early in the thirties it became apparent that a large grating spectrograph was necessary for the continuation of my work on complex spectra. We obtained a grant from the Carnegie Corporation to convert a room in the Palmer Laboratory into a mounting for a 21' concave grating with 45° incidence. This conversion, including a new concrete floor resting on sand independent of the rest of the building and various concrete pillars on which to mount the optical parts, was carried out for approximately $2,000. The grating which was obtained from R. W. Wood at Johns Hopkins was only moderately good. A peculiar ghost unfortunately prevented me from having the honour of confirming Urey's discovery of heavy hydrogen when he sent me a sample of slightly concentrated material for observation on our instrument.

In the early thirties Professor H. W. Russell and I collaborated on one quite important paper on the description of perturbed series. Such abnormal series had been known for many years and quite extraordinary formulae had been tried for their description. In the same period, I described the effects of auto-ionization, the equivalent in atomic spectra of the Auger Effect in X-rays and predissociation in band spectra. Perturbations of series and
autoionization are closely related effects of configuration mixing and both
can be strikingly large. At this time I also re-observed the spectrum of
Cu II using the hollow cathode at high power as a source. The result was a
very complete description including the identification of all but a few doubt-
ful lines. The observations in the vacuum region were carried out at M.I.T.
on the Compton-Boyce two-meter instrument and the infra-red spectra were ob-
tained at the Bureau of Standards using a sealed off hollow-cathode tube
which I carried there by hand. This material was put into condition for
publication while I was on leave in 1935 at Cambridge University and Ruther-
ford was good enough to put it in the Transactions of the Royal Society.
He was quite interested in the paper, the first one on a spectrum analysis
that he had read, I believe, and was particularly struck by the cooperation
of M.I.T. and the Bureau of Standards which made it possible. The analysis
allowed the accurate calculation of many lines in the vacuum region and for
many years they were used by other spectroscopists as standards.

Up until the Second World War there was no such thing as half-
time teaching. All of us, therefore, were occupied in giving advanced
courses as well as assisting in the elementary teaching. This was very
valuable and stimulating experience since we had to properly master the par-
ticular subjects we were asked to teach. Moreover, the policy of the de-
partment being to change the lecturers in all courses periodically, we all
taught many parts of physics. My own sequence was first Electricity and
Magnetism taught from Starling and later Pidduck, followed by Mechanics from
Lamb's two volumes, followed by Optics from Bruhat's Cours d'Optique in
French. Later in the thirties I took over the large elementary course and
continued it until I left for Canada in 1940. I also lectured in a graduate
course in Spectroscopy in collaboration with L. A. Turner.

My experience leads me to deplore the present insistence on part-
time teaching. It is never possible to know a subject until one has taught
it and we are nowadays bringing up a generation of physicists who will not
have that experience. In my own case, I believe I could have done very
little more research had I had less teaching to do and I would certainly
know less physics. More frequent sabbaticals are more important than less teaching. I also deplore the acceptance of the system, which apparently grew out of the Second World War, that treats the professor as a part time employee and allows the payment of additional salary during the summer. Certainly traditionally the professor was a member of the University for the whole of his time and payment of salary in nine or ten installments instead of twelve was merely a convenience to take account of travel and so on in the summer.

The amount of teaching done by each of us was increased during the depression but it never became excessive. There was, of course, no large financial support for research from government agencies, but we were fortunate in Princeton in having a Scientific Research Fund, raised in the 20's, which allowed us to build a cyclotron and to pay small stipends to a few research assistants. It also enabled me to design and build a twometer vacuum instrument in 1936. This instrument is still in use and has never had to be refocussed. It is probable that it has produced more results than any other instrument of the kind because for some years after 1945 it was the only productive instrument in operation. It has never been described in the literature because I was always more interested in using it than in writing about it. Nevertheless, many of its features have been incorporated in other instruments by physicists who have examined it personally. An important addition to the equipment for spectroscopy was first made in this laboratory by Louis Green when he was a graduate student. It is notoriously difficult to excite Fe II lines in a hollow-cathode tube. In his attempt to obtain higher and higher powers for this purpose, Green hit upon the idea of using a monocyclic network to feed a rectifier set which could deliver up to 4 amperes at 1500 volts. Such a circuit theoretically supplies constant current, whatever the load, and the full power can therefore be applied to the discharge tube. In later research I have used this system exclusively for all D. C. applications not requiring over 4 amperes. It is ideal for operating arcs as well as Schuler tubes.
The Second World War started in 1939 and I tried at the time to get back into my old regiment, The Royal Engineers. The need of engineer officers was not felt until the fall of 1940, by which time I had obtained more suitable employment. In the spring of 1940 I was offered a position as his assistant in war work by Professor R. C. Tolman of California Institute of Technology, but I considered that my Canadian citizenship would militate against my effectiveness and I refused the position. In the summer of 1940, the Tizard Mission came to the United States with carte-blanche to reveal all the important scientific war secrets of Great Britain, including, above all, the 10 cm. magnetron which astounded all the scientists and engineers who saw it. With the Tizard Mission there came Professor R. H. Fowler as a liaison officer between the United Kingdom, Canada and the United States and he needed an assistant. This was the position I was offered and accepted. In November 1940 I moved to Ottawa and was followed early in 1941 by my wife and son, who continued to live there the remainder of the war. The fact that we had a young son precluded the possibility of my wife accompanying me when I later was sent to London. For the period October 1940 to February 1942, I travelled extensively in the United States exchanging information with government agencies and with companies engaged in war research. In particular, I watched the growth of the Radiation Laboratory at M.I.T. which was based entirely on the magnetron.

In February 1942 I was asked to take over the Canadian National Research Council liaison office in London. I spent most of 1942, 1943, 1944 and up to August 1945 in that city, returning to Canada each winter to bring my knowledge of Canadian scientific activities more up to date. In London I had an office in St. James's Square staffed by an assistant and three stenographers. We dealt there with every development from physics to medicine and housing, but our chief interests were in radar, aircraft, aerial photography and explosives.

My friendship with many British scientists and my membership in a Cambridge college made my work of obtaining information of assistance to Canada much easier. I was made a temporary member of the Athenaeum Club.
in London and this was changed to permanent membership at the end of the war.

My residence in London started after the main bombing attacks, but it did include what was known as the "Little Blitz" and the whole of the V.1 and V.2 attacks. The V.1 was a particularly effective weapon because, though it could be heard approaching for a considerable time, its point of impact could not even be guessed at. The V.2, which gave no warning, was not nearly so effective psychologically.

In August 1945 I returned to Canada on a 3500 ton ship from Tilbury to Halifax, Nova Scotia. The voyage took two weeks and the small size of the ship necessitated two sittings for the meals of its twelve passengers.

The purpose of my joining the National Research Council being accomplished, we returned to Princeton in September 1945. Nearly all of the members of the Department of Physics of Princeton University had spent the war in work elsewhere and the department had to be rebuilt anew. During the war some seventy temporary instructors taught physics to service and other students in Princeton under the general direction of Professor H. D. Smyth. The rebuilding of the department was far from easy. The only returning members of the permanent physics faculty were H. D. Smyth, R. Ladenburg, M. G. White, W. Bleakney, J. A. Wheeler, E. P. Wigner and myself. On the advice of M. G. White, our first move was to invite R. H. Dicke and D. R. Hamilton, both former Princeton undergraduates, to join us as assistant professors. They were followed shortly by R. Sherr, R. Hofstadter, H. W. Fulbright, D. Bohm, P. C. Gugelot, A. S. Wightman. E. M. Rogers, who joined the faculty in 1942, was of the greatest value in the rehabilitation of teaching in the department and he has continued his vital role since that time.

In 1949 Professor Smyth, our Chairman, was appointed as an Atomic Energy Commissioner and moved to Washington, D. C. The chairmanship devolved on me and I held it until 1960 when I requested to be allowed to resign in order to have two years of less responsibility before my retirement, which takes place this year. The work of the Chairman has become
more and more demanding with the increase of the faculty and of financial support for physics, so that my successor, Walker Bleakney, finds little time to devote to teaching or research.

The rebuilding of the department involved many serious problems such as the extent to which we should accept federal funds and from whom; the current balance between experimentalists and theorists on the faculty; the ability of the department to train graduate students; the problem of whether or not a university as small as Princeton could afford to have such a large machine as a 3 Bev accelerator (now nearly finished). In addition, there is always the fact that a group of outstanding physicists is certain to include some "prima donnas" in male form who do not make life easy for the chairman.

In Princeton there is an added factor in building up a faculty which is lacking in many institutions. We have always insisted and we continue to insist that all classroom teaching be done by faculty members and that graduate students be used only for the supervision of laboratories under instructors. This has meant a very large number of junior faculty members. That number is in turn nearly doubled by the fact that in recent years nearly all members of the junior faculty are on half-time teaching, the other half of their salary being paid out of project research funds. I believe that the physics faculty of Princeton is larger than that of Harvard, a university which is, of course, several times the size of Princeton.

In 1950 Professor Ladenburg retired and we were faced with the necessity of replacing him in the position of Research Professor of Experimental Physics. We successively invited four well known experimental physicists to join us but in all cases we found that involvement in existing research programs made them averse to a change in institutions. In the end, we accepted the advice of our Advisory Council and asked for the successive appointment for two years each of Walker Bleakney, D. R. Hamilton and R. H. Dicke. At the end of that period the department requested the permanent appointment of R. H. Dicke.
In 1950 I had the honour of being elected a Fellow of the Royal Society of London.

When I came back to Princeton in 1945 I found that the spectroscopic equipment had been prepared for work on uranium. This had been discontinued but it meant that a certain amount of re-arrangement was necessary. My own desire to get back into spectroscopic research was very great, perhaps because it was the one thing that my war work had never included. My first efforts were to complete some pre-war unfinished work and to reestablish contact with spectroscopists. A meeting in Amsterdam in September 1946 was of considerable value in this latter aim. My contacts with Mrs. Sitterly and W. F. Meggers at the National Bureau of Standards had always been close, and with the encouragement that spectroscopy got after the appointment of E. U. Condon as Director those contacts became even closer. As a result I took up the analysis of certain spectra that had been previously neglected, chiefly third spectra in the long periods for which my vacuum instrument was particularly suitable. I developed new methods of observation of such spectra, the chief innovation being due to the ease with which we could obtain pure helium in quantity. This method involved the mounting of a spark chamber into which helium at nearly atmospheric pressure could be fed. From there the helium leaked into the spectrograph through the slit and was pumped away. The special design of the slit allowed a pressure ratio of about 75 to be maintained. This method makes possible the photography of spectra down to and even past the resonance line of helium at 58μA° without the great admixture of lines due to highly ionized atoms, which are a feature of vacuum sparks. For spectra down to the limit of Li F at about 1100μA° the method is simplified by the use of a window and an attachment which ensures an optical path of pure helium at atmospheric pressure between the source and the slit.

During the period from 1949 to 1960 when I was Chairman of the department I was able to continue my research in spite of the many added responsibilities mainly owing to the fact that I had acquired a certain facility in working on it in any small amount of time available between other duties.
I have never liked working with assistants and this probably made it possible for me to continue my intimate interest in spectroscopy. In point of fact, I have realized more and more that a spectrum photograph is to me a thing of great beauty. I am fascinated with the information one can obtain from it.

During my incumbency of the chairmanship the department grew very rapidly and it has continued to do so. In 1949 the number of the faculty in physics was 25, in 1960 47 and in 1962 57. These numbers do not include graduate student assistants of whom about 15 to 18 were employed each year.

After the war when H. D. Smyth was still Chairman, he realized that the growing use of Government research funds would eventually lead to the Chairman becoming nothing but an administrator. He, therefore, obtained permission from the University to appoint an Executive Assistant to take from his shoulders a large part of the detailed work involved in the running of the non-academic side of the department. While I was Chairman the duties of the assistant grew with the rapid increase in the size and complexity of the department. We would now consider it impossible to do without an Executive Assistant. The present holder of the position is R. A. Winters, a Ph. D. in economics.

Some general remarks about my life's work are in order. It should be obvious from what I have written that I have always avoided publicity or involvement in any activity which could take me away from the academic environment. I have been assisted in the latter by the fact that I have some private means, though I do not think that that has been a major factor. Inside the University I have always spoken my mind, and it seems I have, in fact, acquired some reputation as a guardian of faculty rights and of University standards. I find Americans averse to voicing objections in public, frequently for fear of being disliked. There would be no United States today had their ancestors shown similar timidity.

With regard to avocations and amusements, I played ice hockey for Princeton as an undergraduate and during the years up to 1940 I was an enthusiastic though mediocre tennis player, but the latter activity ceased
abruptly in 1942 when I developed an uncertain heart. From 1903 onwards my family spent their summers near Digby in Nova Scotia, and I now own the cottage my father built there in 1907. Whenever we have been in Nova Scotia in the summer months it has been possible to catch up on scientific and general reading, but my chief joy has been sailing, which in those parts is rendered very difficult by the 28 feet tides. My attachment to Europe started with a trip in 1913 which took us to a few places in England, France, Switzerland and Germany. Those were the days of apparent stability when passports were not required and one travelled with gold in one’s pocket. The English-American exchange never varied from $4.86 2/3 to the pound. The war now known as World War I strengthened my feeling of identity with Great Britain and my marriage to an Englishwoman confirmed it.