25 1927–1931. Years of Growth and Progress

We stayed a few days with my sister on Pleasant Street, in Belmont, where there had been a blizzard just before we arrived. I remember how soothing to me was the muted sound of the slapping of heavy chains on the snow. Then we started house hunting. We found an apartment just across the Arlington line. I began to try to adapt myself to a life of handyman domesticity, for which I had no particular qualifications. I made good after a fashion as a furnace tender and furniture varnisher, but this was never my métier.

Dirk Jan Struik, a Dutch mathematician whom I had met at Göttingen and who was now a new appointee at M.I.T., had already come over some months before and was immersed in his work. He fitted into the familylike environment of our department very well, and I began to study his field of differential geometry. We started work together on an attempt to apply his ideas to differential equations and, in particular, to the Schrödinger equation of quantum theory. We were off the main line of progress, but we did obtain some interesting theorems. Our work was not the sort which makes a big splash in the literature, but it was the sort which is rediscovered years later, and which has a permanent but limited interest.

The Struiks and ourselves spent the summer in our beloved town of Sandwich, in New Hampshire. We used as our headquarters a boarding house which had long been known to us. Margaret was pregnant and terribly uncomfortable with poison ivy rash, so she could not accompany Struik and me on our hikes. The two of us, however, ranged the nearby mountains and made a larger expedition together to the Presidentials. From this we returned bearded like the pard. Struik promptly shaved off his beard, which had made him look as if he had been painted by Rembrandt, and Margaret started to trim mine down by degrees, until it attained the exiguous proportions of the beard which I continue to wear to the present day.

We had many friends among our country neighbors. These included Clare George, a well-to-do, eccentric, mannish spinster, who affected trousers at a period when these were not yet in vogue for women, and who used to puff a furtive cigarette when she was alone with my wife. We saw her often at the home of our friends, the Corlisses. Louis Corliss is a Cornell engineer who had worked for the Sperry Gyroscope people in the early days, until a series of deaths in the family and ill health due to overwork had led him to choose the life of a farmer on his family farm rather than the confusion of modem engineering and industrial life. He was a widower, living with his grandmother and his mother. The whole family was charming to us and has continued to be among our close friends. Grandma Corliss died some twenty years ago, and Corliss married the nurse who attended to her in her last days. Their daughter, Janet Corliss, has become my trusted secretary during the summer months and has assisted me with the preparation of this manuscript.

Clare George and Louis Corliss's mother were well aware that I loved the Sandwich region and that Margaret was coming to love it. We felt that we wanted our prospective children to have the advantages of country life that had been granted in one way or another to each of us. Our friends made themselves busy scouring the neighborhood for a suitable summer home. They found one on a knoll on the Bear Camp Pond road. The house was uninhabited. It had been only recently inhabited, however, and was in good condition. When Margaret and I made the circuit of its weed-grown lawns and peered through its cobweb-covered windows into its graciously proportioned rooms, we knew we had found what we wanted.

We sought out the names of the caretaker and of the lawyer who was entrusted with the property. The region had been going downhill from the Civil War to that time, and real estate prices were at a dead bottom. The price named, if not within our immediate reach, was not outside what we could hope to pay within a few years. We agreed that it was the summer place for us.

Soon after that, my parents came up to visit us at our boarding house. We showed them the country place we were thinking of buying, and they were enthusiastic about it and helped us to purchase it. Ever since, our vacations in the White Mountains have meant for us the relaxation we need from the strenuous life of M.I.T., and also a chance to give our children the experience of the country and the freedom of living which we feel to be the birthright of every child.

As a matter of fact, Margaret and I needed the place as much as we hoped our prospective children would need it. Teaching at a university is a very strenuous job, but teaching combined with research is a full load for any man. Much of my research depended on free exchange of ideas with other men; but there always comes a time when this preliminary work is done and when I have had to spend my main attention on writing up the work in compact and acceptable form. This writing can be done best when there are no distractions and when my life is a simple alternation between concentrated intellectual effort and the completely non-intellectual pleasures of roaming the countryside, meeting my non-professional friends, and swimming and basking on the beach.

There are many people who believe that the summer vacation of a college professor is a very special sort of junket, a pleasure granted to the intellectual in return for his smaller salary and his unexalted position in the American scale of social values. Nothing could be further from the case. Severe work of a research nature drains one dry, and without an ample opportunity to rest as intensely as one has worked the quality of ones' research must go down and down.

I do not mean to make the claim that only intellectuals need long vacations. I am quite certain that the continuous pressure of industrial work and the fragmentary vacations given as a surcease for this work are responsible for the early aging of many of our best minds in industry. This condition has become particularly acute since the war, for we have acted on the hypothesis that in times of stress it is a sort of treason to relax. I am convinced that our policy of continued tension is foolish and that it fails to serve the end of the best use of our human resources.

My older daughter, Barbara, was born during the next academic year, 1927–28, and I began as a very clumsy pupil in the art of baby sitting and of hanging out a long signal hoist of diapers.

The next summer, we settled on our new estate with our new baby. My father had given me his superannuated Model T beach wagon, and he had made several trips with me to bring up the necessary furniture. In those days we were without a telephone, without electricity, and even without a stove. We prepared Barbara's formula in the fireplace and did our rudimentary cooking there until such a time as we could get a second-hand two-burner oil stove. To the present day we remain without running water, although we have found a very satisfactory substitute for this in the form of a force pump and gravity tank.

We used to take the baby down to the beach of Bear Camp Pond, where she, like our second daughter, Peggy, practically grew up in the water. The beach was semipublic and was frequented by a large number of neighbors with children of all ages. I initiated these children into the habit of taking long mountain hikes, and now when I go to the beach, I see there the children of these children.

We had the Klines as our guests that summer. They were charmed by the region, and ultimately decided first to rent and then to buy a summer cottage there. I have said that the region was about at its lowest economic point at the time that we bought our house. From the agricultural standpoint, it has perhaps lost further ground, but it has become a popular summer home for a very congenial middle-class group, among them a number of university people. Some of these, indeed, have since retired, and spend a large part of the year in those restful surroundings. At present it would be easy to recruit the faculty of a very fair university from our neighbors without bringing anyone in especially for the purpose.

The improvement in my scientific status went on apace. My new personality as a married man made it possible to allay some part of the hostility with which I had been received in mathematical circles. Nevertheless, the many barriers against me were still up. Birkhoff had made his prejudice a matter of principle, and saw to it that many academic offers which otherwise might have been open to me were diverted elsewhere. As this was the period during which the status of the college professor was improved greatly over the country as a whole, and in which most of my colleagues were preparing to save for their old age, this deprivation was serious for me. Tech indeed kept on advancing me steadily, notwithstanding the absence of outside offers, but it is an unquestioned fact that the existence of such outside offers would have improved my position in one way or another.

In default of American offers for an improved position coming through the normal channels, I began to look around and to see if I could not do something for myself elsewhere. The British universities and the universities of the British colonies operate under the legal provision that if any vacancy occurs it must be advertised and the applications of all candidates must be considered at least in a formal way. This requirement is not taken too seriously, and in many cases a decision has already been made for all practical purposes at the time the vacancy has been advertised. These advertisements appear on the back pages of *Nature* and other British intellectual publications. I sent in my name for one vacancy at Kings College in London and for one in Australia, but of course nothing happened. Nevertheless, the practical evidence that I was trying to better myself was of considerable benefit to me as far as my M.I.T. advancement went.

It was about this time that there was a summer meeting of the American Mathematical Society at Amherst. Margaret went with me, and we both thoroughly enjoyed the occasion. The important matter for me was that

I saw there a great deal of my friend, J. D. Tamarkin, another Gottingen acquaintance, and that he seemed favorably impressed with my research. He became my enthusiastic and sincere backer, and it was more through him than through anyone else that my American recognition began to take serious proportions.

Tamarkin was a brilliant mathematician whose origins were in those days before the First World War, when life in Russia was very good for those above middle-class standing. In America he attempted to carry out the open-handed lavish hospitality which belonged to prewar Russia. He had escaped from Russia at the risk of his life and had won an enthusiastic reception from Professor R. G. D. Richardson, of Brown University. Tamarkin's mathematical standards were of the highest, and he welcomed my work at a time when those of a purely American tradition did not think very much of it.

During these years, Hardy made a series of visits to the United States. He also thought well of my work, and between him and Tamarkin I began to be heard of in this country, but I was never able to forget that the people to whom I owed the greatest part of my recognition were not Americans.

The years after I came back from my trip of 1926 and before my trip of 1931–32, were of course the years of Coolidge prosperity and of the depression. Even in our relatively protected academic life we felt the strong impact of both phases of national and world existence. As I have said, Harvard salaries had been raised during the boom years to a very considerable extent; and although Tech salaries lagged, there was still the expectation on the part of many that they might gradually climb, if not to Harvard levels, to a reasonable imitation thereof. As a consequence, many of my colleagues at both institutions were talking stock market and behaving like capitalists. You could not get a group of five college professors together without hearing a comparative evaluation of the popular stocks of the day. One or two of my younger colleagues devoted more attention to the course of their investments than to their academic work.

I never fully believed in the boom, though I was quite well aware of its consequences on our lives. Too many of the values were paper values which, as I even then saw, could vanish over night. The farmers went in for those silver-fox farms which even the slightest slump would deprive of a market. Some of my colleagues tried to supplement their incomes by breeding fancy kinds of dogs and Siamese cats, and these were subject to a similar disadvantage. In the same category of will-o'-the-wisp prosperity were the land boom in Florida and the vogue of Steuben glass and antique furniture. We never had enough money to go in for any of these things, and, frankly, I never felt the temptation. Thus, I was prepared for the boom to end in a crash in which the loss of a set of paper values was bound to bring as a consequence the loss of an entire structure, which had flourished like the green bay tree.

The paper values of a monetary nature involved a whole series of paper moral values. I was astonished and shocked at the way in which a great national magazine gave its columns over to a panegyric of the Swedish match king, Ivar Kreuger. What I was less prepared for was the acceptance of the same moral values in academic circles. I hoped and prayed that the slump would come early before it could build up into the complete failure that generally follows the burst of a promotional bubble. I talked this over with my good friend Phillips and was surprised to find that, for all his native skepticism and personal shrewdness, he was hopeful that the boom was here to stay. Behind this feeling on his part was a long experience as a youth in the ruined South after the Civil War and a fear that we might be heading for another such period. To a large extent, his optimism was a whistling to allay his own fears, but at any rate he did not share with me the hope that a mild depression might turn us away from the fleshpots of Egypt and to a greater evaluation of moral and intellectual matters. When the slump came, I saw that he had been justified in his fear that a collapse would not only destroy commercial values, but moral values as well.

The slump was bound to affect everyone, but we academic people had the best of it, for a while. To some extent, prices went down, and while our expectation of rapid advances in salaries blew up in our faces, many of us, like myself, had tenure; and those of us who did not have it had a moral expectation of it which was not easily canceled. At any rate, we did not suffer from the epidemic of suicides which took place among businessmen, and a high window was no particular moral hazard for us.

A college professor who is really interested in his work is considerably insulated from the vicissitudes of the world about him. At the present day, when science is an object of general attack, and when many of us have serious doubts whether our civilization really is viable, the protection of our isolated position has largely vanished.

In those days—the late twenties and early thirties—however, while we may have doubted many things, we did not have fundamental doubts of the long-time recovery possibilities of the world in which we lived. Thus the Sacco-Vanzetti case, the phony boom, and the almost equally phony depression that followed it, drove us more and more into ourselves and into our real function, academic work.

My work, as I found it at this time, was research and the initiation of research students into their proper activity. As my work and reputation developed, I began to get graduate students. There was an M.I.T. professor whose son was doing research in the Harvard mathematics department for the doctor's degree and who wanted to work with me. We found it possible to make an arrangement between the two departments by which his degree should be at Harvard while I should be the director of his thesis.

My first M.I.T. doctoral candidate was a young man by the name of Carl Muckenhoupt. I gave him a thesis topic in Harald Bohr's theory of almost-periodic functions. These functions had been studied by Bohr as purely abstract mathematical entities, but I saw how they could be used as effective tools in the qualitative and even the computational study of vibration problems. The Muckenhoupt thesis represented another link in the synthesis between pure and applied mathematics which I conceived it was my function to make.

Important as the Muckenhoupt work was in my development, there were two pieces of work that came along slightly later which turned out to be even more important. These represent doctoral theses which, by a peculiar coincidence, were done under my supervision by two students from the Far East. Their names were Yuk Wing Lee, from China, and Shikao Ikehara, from Japan.

The way in which I came across Lee is interesting. My Dutch friend, Struik, had found some summer work with the Bell Telephone Laboratories in connection with the analysis of electrical circuits. This immediately led me to consider whether I might not have a different approach to the same field by the use of Fourier series. My idea continued to look good to me under further examination, and I asked Vannevar Bush whether he could lend me a good student in electrical engineering to do a thesis under me. He was only too glad to do so, and suggested Lee, who was then living at the parish house of one of the Boston churches. Lee readily accepted the offer and we went to work together.

Lee and I have been scientific colleagues now for about a quarter of a century. From the beginning, his steadiness and judgment have furnished exactly the balance wheel I have needed. My first idea of an adjustable corrective network would have worked, but at the cost of a great wastefulness of parts. It was Lee who saw how the same part could be used to perform several simultaneous functions and who in that way reduced a great, sprawling piece of apparatus into a well-designed, economical network.

It was also Lee who found a possible purchaser for our invention, in the form of a research corporation allied to the moving picture industry. It was

Lee, above all, who went down to Long Island City and spent many months of patient work in developing our apparatus, computing the size of the parts, constructing a good working model which functioned the first time up to the degree to which we had predicted its functioning, and selling the idea to our clients.

Unquestionably those were thin times for the electrical end of the moving-picture industry, which recently had been created to take care of the problems of the talking film. Our rights reverted to ourselves, and we had to look for a permanent purchaser. It was Lee who found such a purchaser in the Bell Telephone Laboratories and who saw the work through the tedious stages of patenting.

Here I must remark that the public that is interested in inventions but has had no direct experience of the Patent Office can have no adequate idea of the utter boredom of seeing an invention through the necessary stages of search and documentation. In the first place, it means nothing merely to patent an invention. Any teeth that the patent may have—and they are few enough when the patent is held by a private individual with no means—depend on a detailed legalism of the phrasing of claims and specifications, which have very little to do with the actual merits of the invention. Here the patent lawyer can be of great help, but he can be of very limited help unless he is backed by the peculiar understanding of the invention which only the inventor himself may be expected to have.

The result is that the inventor proceeds without any transitional stages from a game of ideas to a game of words. The more he loves his invention for its own sake and the more that he wishes to develop it, the more he finds himself frustrated by the unreal world of the Patent Office, in which he is forced to live for a term of months or years.

At the end of these months or years, Lee and I found ourselves possessed of a salable invention—indeed, of an invention which we had actually sold before the final stages of its patenting were completed. But then we met the further frustration that all this effort we had made went into a paper patent—that is, into a patent which the Bell people never intended to use but simply to hold *in terrorem* against competitors.

The Bell people never did use our invention, from the time it was on the books of the Patent Office to the time its patent expired. Nevertheless, as our document was near the end of the seventeen years, which are to a patent what seventy years are to a man, we found that certain radio and television firms began to show a great curiosity concerning the new invention, as if in fact they were about to incorporate it into the sets they made. Since our rights have expired, we have never shown the curiosity to find out whether these engineers have in fact followed our ideas to the point of execution or our baby was stillborn. This does not prevent us from having a shrewd idea that, whether or not the invention was ever made for purposes of sale in the form in which we wrote it up, it is still exercising an influence on the philosophy of the art.

I made every effort to place Dr. Lee in the American electrical-engineering industry, where he would have been a valuable man. At that time, the engineer from Eastern countries did already exist in the United States, but he was a much rarer bird than he is today. The sales resistance I had to meet was more than we could overcome, and Lee went back to China to seek a job, first in industry and then in academic life. I shall have much more to say about him in later chapters.

At the same time at which one Oriental student, Lee, was developing the consequences of some of my engineering ideas, another, Ikehara, was perfecting my methods in prime-number theory. Landau, the chief German exponent of prime-number theory, was at first hesitant to accept our results, but ultimately he and his colleague, Heilbronn, wrote papers taking this work still further. The result was to remove a difficult branch of mathematics from the latter years of graduate work and to make it available even in an advanced undergraduate course. In recent years, the Scandinavian school of mathemations has gone even beyond where we left our work and has made prime number theory elementary in a certain very technical sense.

I have already mentioned Vannevar Bush as a great inventor of electromechanical devices, and his chief work was along the line of the development of high-speed computing machines for solving problems in the field known as technique of differential equations. Differential equations are concerned with the relations between various measurable physical quantities and their rates of change in time and space. These physical quantities can be currents or voltages or the angles of rotation of shafts or quantities of still different sorts. Bush's device now tried one set of quantities as their bases and now another, but the form to which his apparatus gravitated was a sort of meccano set, the differential analyzer, in which various quantities could be represented, let us say, by the rotation of a shaft, and in which these quantities could be added, multiplied, divided, and operated on in still other ways. Above all, where the total sum of a quantity was wanted, these quantities could be read off by the device known as an integrating disk.

None of the individual parts of the Bush machine were completely new in conception, but the technique by which they could be combined, and in particular by which the power to move the apparatus could be put in locally in such a way that the machine would not stick, represented an improvement in technique going far beyond anything conceived before. Bush's machine was successful where Babbage's earlier machine had failed, precisely because of a brilliant use of engineering facilities and engineering ideas not accessible at the time of Babbage.

In Bush's machine numbers were represented as measured quantities rather than as sequences of digits. This is what we mean by calling the Bush machine an analogy machine and not a digital machine. The former measures. The latter counts. The physical quantities involved in the problems which the machine was to solve were replaced in the machine by other physical quantities of a different nature but with the same quantitative interrelations.

At that time the digital machine, which is an improved and automatized abacus, was confined to various forms of the desk computing machine.

It was an essential part of Bush's machine that the variable in which all changes were to take place was time. This became exceedingly significant when Bush asked me for advice on how to make his machine take care of partial-differential equations, in which time rates of change and the space rates of change are united by equations.

When Bush asked me this question, I realized that the main problem of the partial differential equation was that of the representation of quantities varying in two or more space dimensions, such as, for example, the density of a photographic negative, which varies up and down and right and left.

Once the problem of representation of functions of several variables was clearly stated, it became desirable to represent these too as something changing in time rather than in space. Here it appeared to me that the new and developing art of television gave the necessary clue. In television, a picture is conveyed not by pieces of silver of various opacities placed simultaneously on a film but rather by a dot of light running over the various rows of a grid point by point, and the whole grid row by row. This process, called scanning, is now familiar to anyone who has the least curiosity as to how his home television set works.

In fact, I was convinced that the scanning technique would prove socially more important in computing machines and their close relatives than in the television industry itself. The future development of computing machines and control machines has, I believe, borne me out in this opinion.

In representing a quantity as a single locus in a television screen, we may follow two different techniques, one derived from the analogy machine and one from the digital machine. Each spot on the television screen may determine either a quantity of light which is measured by its intensity or a sequence of digits such as we use in writing down a number. The combination of these quantities to represent the situation present in a partial-differential equation may accordingly be either a combination of intensities or a combination of the digits of several numbers. It seemed to me even at that time that the latter method of representation would be more suitable for partial-differential-equation machines, because of the fact that, with electronic apparatus, digits may be combined more accurately and more expeditiously than amounts of light. I need only say that the actual development of the computing-machine technique has proved the correctness of my surmise and that the high-speed computing machines of the present day follow very closely along the lines which I then suggested to Bush.

The emphasis which I then put and still put on speed in mechanical or electrical computers has fully justified itself. We can put more distinguishable numbers on a square grid than we can on any one line of the grid, and the number of operations through which we must go to represent the process of solving a partial-differential equation is simply enormous. Without stepping up the speed to a tremendous extent, the partial-differential equation machine would have been so slow that it would have become useless. In general, the computing machine is a competitor of the human computer; and when all is said and done, its advantage over the human computer lies primarily in its speed. This subject, to which I shall revert later in the present book, represents the first step toward the origin of the extremely high-speed computing machine of today as well as toward the closely related machines of the automatic factory.

At about the same time, Bush was engaged in the writing of a book on electrical-circuit theory. Here some of the work which I had done on generalized harmonic analysis became of considerable practical value. He called me into consultation for advice concerning many of the chapters and also asked for a supplementary chapter on Fourier methods. We enjoyed the collaboration greatly, and both of us have spoken often of the fun we had in working together. Bush soon left his theoretical work for an administrative career. This was a step in the formation of a new academic setup which had long been overdue.

When I arrived at M.I.T., in 1919, Richard McLaurin was president. His was a name to conjure with, and he had added enormously to the position of the Institute at home as well as overseas. However, he died within a term of my arrival and left incomplete much of what we had hoped he would achieve. In particular, the science departments, including mathematics, and the cultural departments, such as those of English and history, were still conceived as service departments for the main center of life in the Institute, which was engineering.

After McLaurin's death we went into the doldrums for eleven years. For part of that time we were ruled by committees of the faculty, which could do very little because of their avowed temporary character, and for another part by President Ernest Nichols, who, however, was already in ill health when he came, and who retired and died before he could leave much of his impress on the school.

Finally, President Wesley Stratton was appointed, on the strength of the good works he had done as the head of the Bureau of Standards. Like Nichols, however, he came at a period when his best work was over, and he did very little but prolong the interregnum. Until 1930, there was no ruler of the Institute with a sure touch, a clear policy, and an unquestioned vigor and understanding.

Then Karl Taylor Compton was appointed president. He had been a distinguished professor of physics at Princeton, and he combined complete integrity, a long view for the future of the Institute, and a still untapped health and strength. He inspected the physics department on behalf of the Corporation and saw clearly what none of his predecessors had had the opportunity to realize, that a strong engineering school must be at the same time a great school of science.

The role of mathematics at the Institute had changed greatly since the days after the First World War, when I came to M.I.T. At that time, mathematics was something which was chiefly needed to educate our students up to the point where they could handle the engineering which was their main object in life. Physics and chemistry, too, had not yet emerged completely from their status as service departments, whose main purpose was ancillary to the chief task of the Institute, which was to train engineers. When a branch of physics or chemistry had reached a sufficient importance in its own right for a specific course in it to be made a new branch of engineering, this course was set up as an independent engineering course. This had been the history of our electrical-engineering department and of our chemical-engineering department. Now for the first time the Institute had begun to be aware that direct research in mathematics and the sciences was of engineering importance in its own right and that we should devote ourselves explicitly to the training of scientists in these fields as well as to the training of engineers.

In the mathematics department in particular, this removed a great blight under which we had been suffering. Our research began to be recognized as an essential part of our function at the Institute rather than merely as a way to keep us on our toes so that our routine teaching could be fresher and more authoritative. The Institute began to follow the example of the great universities in recognizing us as mathematicians rather than solely as mathematical-routine teachers. This does not mean that we gave up or that we could give up the service work so necessary at an engineering school, but it did mean that we had begun to come into our own, on a status comparable with that of the members of the engineering departments.

President Compton was accessible, modest, sincere, and lovable. With his appointment, the Institute was once more in strong hands, and the line of progress, which had been interrupted by McLaurin's death, was resumed.

Bush's advancement was part of the same movement that brought Compton to the Institute. He was a splendid administrator, taking as his particular field the laboratories of the Institute rather than the personnel. He relieved Compton from much of the detail inseparable from the running of a great school.

One part of Compton's policies was to bring faculty salaries to a level comparable with those already reached at Harvard, Princeton, and the other major universities. Later on, the war and the vicissitudes of the postwar period prevented us from catching up as completely as we might have wished with other schools of the same level. However, the intention was there, and much was done to realize it.

I was a personal beneficiary of the new regime, both as far as salary was concerned and as far as the opportunity to see my hopes for a research mathematics department come into being. I received my promotion to the rank of associate professor, and from that time on my status was assured.

It was thus at the time of M.I.T.'s new impetus of life and Bush's greatest development as an electrical engineer that he had offered me Lee as a graduate student. This was one of the finest things Bush has ever done for me, and I am eternally grateful that he turned Lee in my direction.

About this time we began to find ourselves pleasantly occupied with the visits of a number of scientific colleagues from Europe. In connection with circuit analysis and the electrical-engineering aspects of my work, I saw a good deal of Richard Cauer and his wife, who had come over from Berlin. However, the scientist with whom I had the most interesting and profitable contacts was Eberhard Hopf. He had come over from Germany to Harvard, largely to study with G. D. Birkhoff.

Hopf's interests had been in celestial mechanics, and the new work of Birkhoff. The ergodic theorem, which finally gave the proper form to the ideas of Willard Gibbs, was exactly along the line of Hopf's interests. This piece of work, by the way, was a remarkable tour de force, as Birkhoff had gone into the subject cold, with no particular previous knowledge or interest in the Lebesgue integral. However, he had managed, by his own powers, to extract one of the leading theorems which has dominated the theory of Lebesgue integration ever since.

I was much interested in Lebesgue integration and probability theory, so Hopf and I had a great deal to talk about. However, the best of the work which he and I undertook together concerned a differential equation occurring in the study of the radiation equilibrium of the stars. Inside a star there is a region where electrons and atomic nuclei coexist with light quanta, the material of which radiation is made. Outside the star we have radiation alone, or at least radiation accompanied by a much more diluted form of matter. The various types of particles which form light and matter exist in a sort of balance with one another, which changes abruptly when we pass beyond the surface of the star. It is easy to set up the equations for this equilibrium, but it is not easy to find a general method for the solution of these equations.

The equations for radiation equilibrium in the stars belong to a type now known by Eberhard Hopf's name and mine. They are closely related to other equations which arise when two different physical regimes are joined across a sharp edge or a boundary, as for example in the atomic bomb, which is essentially the model of a star in which the surface of the bomb marks the change between an inner regime and an outer regime; and, accordingly, various important problems concerning the bomb receive their natural expression in Hopf-Wiener equations. The question of the bursting size of the bomb turns out to be one of these.

From my point of view, the most striking use of Hopf-Wiener equations is to be found where the boundary between the two regimes is in time and not in space. One regime represents the state of the world up to a given time and the other regime the state after that time. This is the precisely appropriate tool for certain aspects of the theory of prediction, in which a knowledge of the past is used to determine the future. There are however many more general problems of instrumentation which can be solved by the same technique operating in time. Among these is the wave-filter problem, which consists in taking a message which has been corrupted by a simultaneous noise and reconstructing the pure message to the best of our ability.

Both prediction problems and filtering problems were of importance in the last war and remain of importance in the new technology which has followed it. Prediction problems came up in the control of anti-aircraft fire, for an anti-aircraft gunner must shoot ahead of his plane as does a duck shooter. Filter problems were of repeated use in radar design, and both filter and prediction problems are important in the modern statistical techniques of meteorology.

In the fall of 1929, I received an invitation to lecture on my own research at Brown University. Dean Richardson invited me, but the spirit behind the invitation was Tamarkin whom I have already mentioned. Richardson was a dry, friendly Scotsman from the Maritime Provinces who gave Tamarkin a home in the United States, much to the advantage of Brown University. I traveled once a week to Brown, where I had found a most cordial reception. Tamarkin, together with Mrs. Tamarkin, who had by now come to join her husband, was my principal host.

The Tamarkins had carried their expansive style of living into an America where the habits of the country and the difficulties of the servant problem made this sort of a life almost impossible. Mrs. Tamarkin struggled courageously in the restrictive environment of Providence to feed her husband's habitual need for good food and drink, but in the course of this effort she wrecked her health by overwork while her husband continued to overload his damaged heart. Mrs. Tamarkin died ultimately of an attack of phlebitis, but her husband attributed it to overwork and reproached himself. Before her death he had been the soul of jollity and good cheer, and he continued to offer unstintingly to the younger mathematicians from the store of his great knowledge and sympathy. Yet he never was quite the same man again, and a few years later he too succumbed to the strains which he had imposed on his heart.

Peggy, my second and last child, was born in that year, and I went directly from my vigil at the hospital to my Brown lecture. With two babies in the family, I became much more of a family man and Margaret much more occupied with family duties.

I was frightfully busy at the time working up my definitive paper on generalized harmonic analysis. This appeared in *Acta Mathematica,* a Swedish journal of great international prestige.

The paper was practically a small book It was Tamarkin who urged me to write up this work in definitive form, and it was he who criticized every stage of my manuscript and proof, to its great advantage. I think my papers satisfied Tamarkin to some degree, and it was certainly as a result of his backing that I soon received an invitation to write for the American journal, *Annals of Mathematics,* a paper of similar comprehensiveness concerning Tauberian theorems.

These papers assumed the proportions of small books. As to the *Annals of Mathematics* paper, it was actually published as a separate memoir. In my later writing I have often wished that I had the continued advantage of Tamarkin's selfless criticism.

My research at this time received a ready reception in Russia and was in close relation with the work of some of the Russian mathematicians. I had long had a peculiar sort of contact with the leading Russian mathematicians, although I had never met any of them nor, I believe, ever been in correspondence with them. Khintchine and Kolmogoroff, the two chief Russian exponents of the theory of probability, have long been involved in the same field in which I was working. For more than twenty years, we have been on one another's heels; either they had proved a theorem which I was about to prove, or I had been ahead of them by the narrowest of margins. This contact between our work came not from any definite program on my part nor, I believe, from any on theirs but was due to the fact that we had come into our greatest activity at about the same time, with about the same intellectual equipment.

Four and a half years without travel abroad had put me in a mood for renewed foreign contacts. Since the International Mathematical Congress at Strasbourg, two more Congresses had passed in which I had not participated. The Congress of 1924 took place at Toronto, but, as I have said, I had devoted that summer to my trip abroad with my sister Bertha. That of 1928 occurred too soon after my Göttingen trip to make my attendance possible, particularly as it was the year of the birth of my elder daughter, Barbara.

By 1932 I found the urge to attend another Congress too strong to resist. I planned to spend the academic year of 1931–32 studying at Cambridge and to participate in the Zurich Congress the following summer. I received generous assistance, in the matters of both leave and finances, from the Massachusetts Institute of Technology, so that Margaret and I found it possible to venture a European trip together with our young family of two.

Norbert Wiener—A Life in Cybernetics

Ex-Prodigy: My Childhood and Youth and I Am a Mathematician: The Later Life of a Prodigy

By: Norbert Wiener

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