Norbert Wiener

A Life in Cybernetics

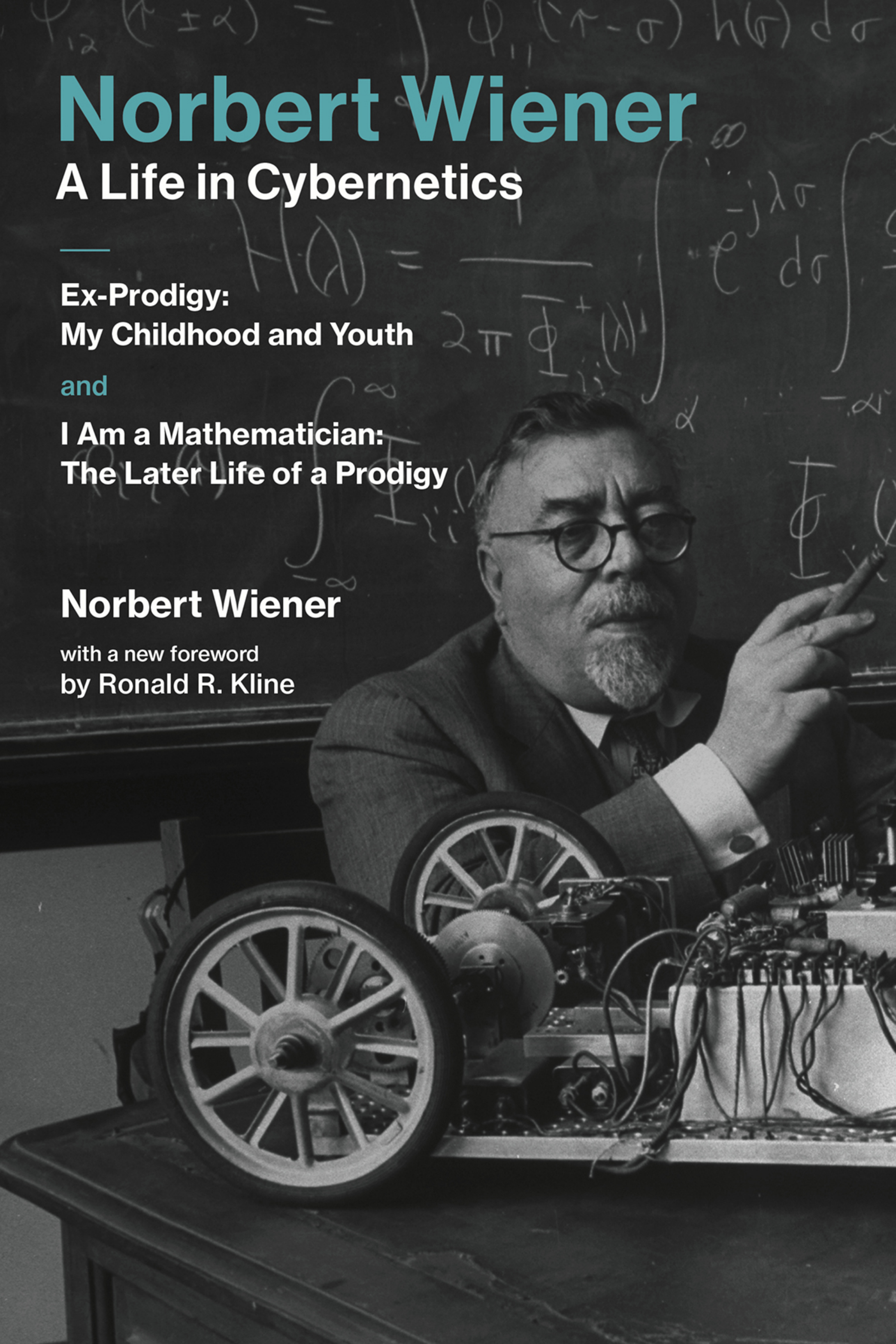
Ex-Prodigy:
My Childhood and Youth

and

I Am a Mathematician:
The Later Life of a Prodigy

Norbert Wiener

with a new foreword
by Ronald R. Kline



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The MIT Press
Cambridge, Massachusetts
London, England

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Funding for the open access edition was provided by the MIT Libraries Open Monograph Fund.

This book was set in ITC Stone Sans Std and ITC Stone Serif Std by Toppan Best-set Premedia Limited.

Library of Congress Cataloging-in-Publication Data

Names: Wiener, Norbert, 1894-1964, author. | Kline, Ronald R., writer of foreword. | Container of (work): Wiener, Norbert, 1894-1964. Ex-prodigy. | Container of (work): Wiener, Norbert, 1894-1964. I am a mathematician.
Title: Norbert Wiener—a life in cybernetics : Ex-prodigy : my childhood and youth, and I am a mathematician : the later life of a prodigy / Norbert Wiener ; with a new foreword by Ronald R. Kline.
Description: Cambridge, MA : The MIT Press, 2018. | Includes bibliographical references and index.
Identifiers: LCCN 2017042823 | ISBN 9780262535441 (pbk. : alk. paper)
Subjects: LCSH: Wiener, Norbert, 1894-1964. | Mathematicians—United States—Biography.
Classification: LCC QA29.W497 A25 2018 | DDC 510.92 [B] —dc23 LC record available at https://lccn.loc.gov/2017042823

10 9 8 7 6 5 4 3 2 1

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Foreword

When the two-volume autobiography of Norbert Wiener was published in the 1950s, newspapers and magazines in the United States reviewed it widely because the author, a distinguished mathematician at the Massachusetts Institute of Technology, had become a public figure. Following the surprising success of his runaway scientific bestseller, *Cybernetics*, published in 1948, journalists ignored its arcane mathematics to interview Wiener about the philosophical and social issues raised in the book. They asked the polymath professor whether the newly invented electronic digital computers, dubbed "electronic brains" by the press, could think, the extent of the unemployment that computerized automatic factories would cause in what Wiener called the second industrial revolution, and what the field of cybernetics, coming out of the scientific laboratories of World War II, had to say about the new relationships between humans and machines being forged in the post-war order.

Wiener's next book, *The Human Use of Human Beings*, published in 1950, reached a wider and even larger audience, securing his place as a pubic intellectual. Wiener drew on his cybernetic vision of the world to analyze such wide-ranging issues as the nature of law, education, and language, the harmful effects of lavish government funding and military secrecy on science, and the Cold War anxieties surrounding McCarthyism and the nuclear threat of the Soviet Union. He thought the mathematical science of cybernetics could shed light on the atomic age because it was based on the idea that the principles of control and communication engineering could explain all self-regulating organisms, living and nonliving, from the level of the cell to that of society. Wiener and his followers touted cybernetics as a universal science, one that could model humans as machines in the social sciences, build human-like machines in the new field of artificial intelligence, and create prosthetic cyborgs (short for "cybernetic organisms") by merging humans and machines. Wiener defined cybernetics as a theory of the messages that held together all organisms, including society. It was thus a science of the future, where "messages between man and machines, between machine and man, and between machine and machine, are destined to play an ever-increasing part."[1]

At the height of the enthusiasm for cybernetics—-when it was taken up by scientists, engineers, humanists, science-fiction writers, artists, and musicians—-Wiener published his two-volume autobiography. *Ex-Prodigy* appeared in 1953, followed by *I Am a Mathematician* in 1956. *Ex-Prodigy* received the most publicity. Wiener promoted the book on radio and television shows in New York City, including the Today Show on NBC. Newspapers and magazines followed up the stories they had carried about Wiener, cybernetics, and the automatic factory with rave reviews of *Ex-Prodigy*. High-brow and middle-brow outlets—including the *New York Times*, the *New York Post*, the *Chicago Tribune*, and the *San Francisco Chronicle*, as well as the *New Yorker*, *Saturday Review*, and *Time* magazines—praised Wiener for baring his soul about how he overcame the traumas of being raised as a child prodigy by his overbearing father, Leo Wiener, who became a professor of Slavic languages at Harvard University, to become the eminent mathematician who founded cybernetics. Among those reviewing the book were the liberal public intellectuals journalist Max Lerner, historian Henry Steele Commager, and anthropologist Margaret Mead. Mead knew Wiener well, having participated with him in the Josiah Macy, Jr., Foundation conference series on cybernetics. Held from 1946 to 1953, the interdisciplinary meetings did much to introduce Wiener's ideas into biology and the social sciences. Writing in the scholarly *Virginia Quarterly Review*, Mead observed that the "story is shot through with pain," but Wiener "measures his words, uses them sparingly and with precision, and achieves an almost Olympian objectivity and gentleness."[2] It was Wiener's favorite review; he thought she understood him the best. The sequel, *I Am a Mathematician*, subtitled "The Later Life of a Prodigy," did not receive as much press, even though editor Jason Epstein at Doubleday succeeded in prodding Wiener to explain his advanced mathematics in lay terms.

Although the MIT Press has kept the two volumes in print since Wiener's death in 1964, this is an excellent time to republish them as one volume. The past few years have witnessed a renaissance of interest in Wiener and cybernetics in the humanities and social sciences, systems science, and the biological sciences.

The two volumes cover what seem at first glance to be two tenuously related eras of Wiener's life. *Ex-Prodigy* recounts the period from his birth to an immigrant Russian-Jewish father and a German-American mother in the university town of Columbia, Missouri, in 1894, through his trials as a child prodigy—when he entered Tufts college at the age of eleven, obtained his bachelor's degree at fourteen, and his doctorate from Harvard at eighteen—to his emancipation from his father upon his appointment to MIT in 1919 and his marriage to Margaret Engemann in 1926. *I Am a Mathematician* takes up the story from there. It relates how he became a world-renowned mathematician at MIT by doing research in pure and applied mathematics in the areas of Brownian motion, generalized harmonic analysis, probability theory, and the statistical theory of communication, which forms a basis for cybernetics.

Yet each volume informs the other. We gain a much better understanding of why Wiener viewed mathematics as an aesthetic, creative art and how he helped establish the interdiscipline of cybernetics, discussed in *I Am a Mathematician*, by reading about his unusual education and his varied life experiences in *Ex-Prodigy*. Before joining MIT, he worked as a journalist and encyclopedia writer, an engineering apprentice at General Electric, and a human "computer" of gunnery tables in World War I. Likewise, we understand more fully why he viewed his appointment at MIT as marking the end of his career as a prodigy, thus becoming an "ex-prodigy," by reading about how he blossomed as a mathematician in the second volume. Although cybernetics only makes its appearance at the end of the autobiography, the title chosen for the combined volumes, *A Life in Cybernetics*, is apt. It refers to the fact that Wiener drew on his entire life experience, as recounted in both volumes, to create the science of cybernetics, which social scientists and humanists are still engaging with today.

A Life in Cybernetics takes up many themes of interest then and now. First and foremost is the new relationship between humans and machines that became evident with the fast-paced development of modern computing after the war. Just as readers in the 1950s turned to Wiener to understand cybernetics, the science associated with the invention of room-sized electronic computers, many readers today turn to books on cybernetics to understand the long history of the information revolution embodied in smart phones, the Internet, and all things "cyber."

The cybernetics breakthrough is the climax of the autobiography. Near the end of *I Am a Mathematician*, Wiener explains that the "whole background of my ideas on cybernetics lies in the record of my early work. Because I was interested in the theory of communication, I was forced to consider the theory of information and, above all, that partial information which our knowledge of one part of a system gives us of the rest of it. Because I had studied harmonic analysis and had been aware that the problem of continuous spectra drives us back on the consideration of functions and curves too irregular to belong to the classical repertory of analysis, I formed a new respect for the irregular and a new concept of the essential irregularity of the universe. Because I had worked in the closest possible way with physicists and engineers, I knew that our data can never be precise. Because I had some contact with the complicated mechanism of the nervous system, I knew that the world around us is accessible only through a nervous system, and that our information concerning it is confined to what limited information the nervous system can transmit." Above all, he concluded, it "became clear to me almost at the very beginning that these new concepts of communication and control involved a new interpretation of man, of man's knowledge of the universe, and of society."[3]

In this remarkable passage, Wiener draws together the two threads that made his vision of cybernetics possible: his mathematical research—on harmonic analysis before World War II, prediction theory during the war, and information theory and the nervous system after the war—and his collaboration with mathematicians, scientists, and engineers—at MIT and throughout the world.

Collaboration is a second major theme in the autobiography. Early on, Wiener lets us know that the traditional path for an aspiring mathematician in the early twentieth century was to work with an accomplished mathematician on a specific problem. After finishing his Ph.D. in Philosophy, with a thesis on the system of logic in Alfred North Whitehead's and Bertrand Russell's masterwork, *Principia Mathematica* (1910–1913), Wiener studied with Russell on a fellowship at the University of Cambridge in England. Russell convinced him to learn more about the mathematics he philosophized about. Wiener learned what it meant to be a research mathematician from attending G. H. Hardy's lectures at Cambridge. Thereafter he sought out senior mathematicians, including Maurice Fréchet, with whom he worked on topology in France for a week before attending his first International Mathematical Congress, held in Strasbourg in 1920. After gaining some renown as a mathematician in the 1920s, Wiener used his facility with languages—learned under the strict discipline of his father, a philologist—to collaborate with several mathematicians and engineers in Europe, Asia, and the United States. In the interwar years at MIT, he worked closely with mathematician Eberhard Hopf and electrical engineer Vannevar Bush. Bush requested his help in putting the Heaviside Operational Calculus, used to design telecommunications circuits, on a firm mathematical foundation. During World War II, Bush directed the National Defense Research Committee in Washington, D.C. In addition to funding the development of such major weapons as the atomic bomb and microwave radar, the NDRC supported numerous small projects such as Wiener's research on an anti-aircraft gun director at MIT. The mathematical theory of prediction that came out of this project, which used autocorrelation to separate signals from noise in a time series, forms the mathematical basis of Wiener's vision of cybernetics. The biological aspect of his vision—that both animals and machines could be modeled using the engineering theories of control and communication—came from collaborating with Mexican physiologist Arturo Rosenblueth, first in Cambridge, when Rosenblueth was at Harvard, and then at Rosenblueth's laboratory in Mexico, on a grant from the Rockefeller Foundation to do research on mathematical biology. Wiener wrote *Cybernetics* while on leave at this laboratory. He dedicated the book to "Arturo Rosenblueth, for many years my companion in science."

Wiener discusses his competition with several mathematicians. Among these is Claude Shannon, who studied under him at MIT. Shannon later founded what came to be known as information theory, based on his classified work on cryptography during the war at AT&T's Bell Telephone Laboratories, then located in New York City. In 1948 Wiener and Shannon independently published their similar definitions of the amount of information as being mathematically equivalent to the physical concept of entropy. Although the identity was known as the Wiener-Shannon or Shannon-Wiener definition of information for several years, Shannon is credited today for creating information theory as a discipline because he proved numerous coding theorems based on that definition.

We also hear about Wiener's high opinion of the émigré Hungarian mathematician John von Neumann, after whom the architecture that structures today's computers is named. Von Neumann hired Julian Bigelow, who was Wiener's engineer on the war-time anti-aircraft project, to be his chief engineer in building a pioneering digital computer at Princeton. But Wiener does not mention that he participated in the Macy conferences on cybernetics with von Neumann and Bigelow.

There are some significant collaborations that Wiener leaves out of his account. He does not mention by name three prominent cybernetics researchers at MIT, with whom he had a falling-out over a misunderstood family matter.[4] From 1952 to the end of his life, Wiener did not speak to Warren McCulloch, the eminent neurophysiologist who chaired the Macy conference series, mathematical prodigy Walter Pitts, McCulloch's collaborator on neural net modeling who studied with Wiener as a graduate student, and Jerry Lettvin, a mainstay of McCulloch's laboratory at MIT.

A third theme, overcoming the trauma of being a child prodigy, is indicated by Wiener's original title for *Ex-Prodigy*, "The Bent Twig." Because of how frankly scholarly journals and newspapers covered his father's educational experiment with him, and also how the *New Yorker* magazine depicted the unsuccessful career of W. J. Sidis, one the four prodigies who entered Harvard with him in 1909, Wiener feared that he would become a failed prodigy. He worried that he had not won the fame as a mathematician that was expected of him as a prodigy, that he had not been recognized as such by Harvard mathematicians and the rest of the American mathematical community. These worries continued, even though European mathematicians accepted him early in his career, the American Mathematical Society honored him, and several mathematical terms—such as the Wiener measure, Wiener Tauberian theorem, and Hopf-Wiener integral equation—bear his name. This insecurity is evident in the autobiography, from beginning to end, and informs the choice of the titles *Ex-Prodigy* and *I Am a Mathematician*. The lifelong insecurity drove him to produce a voluminous body of research up to the time of his death at the age of seventy. But it also led to psychoanalysis, to the extent that he listed his psychiatrist, Dr. Janet Rioch of New York City, in the acknowledgments to *Ex-Prodigy*. Wiener admitted in print that writing the volume had proved therapeutic for him.

A fourth theme, which resonates as powerfully today as it did sixty years ago, is the ethical responsibility of scientists and engineers. Wiener's title for his chapter on this topic, "Moral Problems of a Scientist. The Atomic Bomb. 1942," reflects his belief that this issue would persist for years to come, which it has. In the chapter, Wiener explains his decision in 1947 to no longer accept military funding for his research because of the horrors of Hiroshima and Nagasaki. He also addresses another moral issue: his decision to include in *Cybernetics* a warning about the technological unemployment that the new science he had helped to create could cause with the advent of the automatic factory. Newspapers and magazines extensively covered Wiener's views on these subjects before the publication of his autobiography. The *Bulletin of Atomic Scientists*, the organ of the scientists' movement against nuclear proliferation, thought the chapter was valuable enough to reprint, almost in its entirety, under the title "Moral Reflections of a Mathematician" in 1956.

One of the pleasures of reading *A Life in Cybernetics* is to vicariously experience the politics, history, and culture of the times. We travel with Wiener by steamship to Europe and China, and then by air to India to spend weeks at a time abroad, as a tourist and a collaborator with mathematicians in their homelands. That was necessary to gain a reputation in mathematics early in the twentieth century when Europe outranked the United States in all sciences. It was less so after the influx of scientific émigrés to the U.S. following Hitler's rise to power. Wiener helped several Jewish mathematicians land jobs in the U.S. in the 1930s, and also lobbied for relief efforts for Chinese scholars at this time. After the war, he brought Yuk-Wing Lee from China to MIT as a professor of electrical engineering. Wiener had advised Lee on his Ph.D. at MIT, had spent a sabbatical year with him in China in 1935, and had taken out a patent with him on filters for telephone systems. Lee thrived at MIT, where he became the chief expositor of Wiener's statistical theory of communications.

Wiener opens a few windows on his private life. We watch him as a boy enjoy spending summers with his parents, and then as an adult with his wife and daughters in a farm house they restored, in his beloved New Hampshire countryside. There, the portly mathematician relaxes from an intense year of teaching at MIT by swimming in nearby Bear Camp Pond and hiking the mountain trails he climbed as a youth. We feel the angst of Wiener discovering at the age of fifteen that he is Jewish—seven-eighths Jewish by his calculation—a fact his parents had kept from him until his father accidentally revealed that they were descended from the legendary medieval Jewish philosopher Moses Maimonides. Although Wiener was proud of his heritage, he was extremely conflicted by this revelation, especially because, when growing up, he had regularly heard anti-semitic remarks from his mother, whose father was a German-Jewish immigrant! The feelings that Wiener allows us to glimpse are palpable and help us understand why he dedicated the first volume of his autobiography to his wife Margaret and the second one to MIT. The dedications honor their roles in helping him overcome his inner turmoils to become an ex-prodigy, a distinguished mathematician, and the founder of a science whose history sheds light on the digital utopianism of our time.

Ronald R. Kline

Notes

1. Wiener, *Human Use of Human Beings*, 9.

2. Mead, "Analyzing a Prodigy," 438.

3. Wiener, *I Am a Mathematician*, 323–324, 325.

4. For details, see Conway and Siegelman, *Dark Hero*, chapter 11.

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I Ex-Prodigy: My Childhood and Youth

To my wife
Under whose gentle tutelage
I first knew freedom

Foreword to *Ex-Prodigy: My Childhood and Youth*

The author wishes to acknowledge the help he has received from many sides in the writing of this book. First of all, the greater part of the first draft of the manuscript was dictated to my wife in Europe and Mexico in 1951, in such divers places as Madrid, Saint-Jean de Luz, Paris, Thonon-les-Bains, Cuernavaca, and Mexico City. In Mexico, Miss Concepcion Romero of the Instituto Nacional de Cardiologia helped me in the retyping of a revised version of the manuscript. And finally at the Massachusetts Institute of Technology, my secretary, Mrs. George Baldwin, has borne with me through the long work of revision and selection which has been necessary for the production of the finished manuscript. I have been greatly assisted in the final typing of the manuscript by Miss Margaret FitzGibbon, Miss Sally Starck, and Miss Katharine Tyler of the Massachusetts Institute of Technology. As this book has been done almost entirely by dictation, the secretarial assistance I have received from these various sources is a vital and constructive contribution to the production of the book.

I have submitted my manuscript for inspection to many friends, and I wish to thank them for their detailed criticism and their positive and negative suggestions. Besides my wife, who has collaborated with me through the entire book, I wish to mention Dr. Marcel Monnier of Geneva; Mr. F. V. Morley and Sir Stanley Unwin of London; Dr. Arturo Rosenblueth of the Instituto Nacional de Cardiologia in Mexico City, Dr. Moritz Chafetz and Dr. William Osher, also temporarily at the Instituto; Dr. Dana L. Farnsworth, Dean F. G. Fassett, Jr., Professors Georgio de Santillana, Karl Deutsch, Arthur Mann, and Elting E. Morison of the Massachusetts Institute of Technology; Professors Oscar Handlin and Harry Wolfson of Harvard University; and Dr. Janet Rioch of New York. Of all these, I should like particularly to single out Professor Deutsch, who has gone into my work with an amount of detailed criticism which is altogether beyond what I could expect of a friend who has volunteered to read the book.

In the firm of Simon and Schuster, my publishers, Mr. Henry W. Simon has had the job of seeing this book through the press. I wish to express particularly my gratitude for his sensitive criticism and his understanding comments.

Norbert Wiener
Cambridge, Massachusetts
June, 1952

Introduction

As this book will show, at one period I was an infant prodigy in the full sense of the word, for I entered college before the age of twelve, obtained my bachelor's degree before fifteen, and my doctorate before nineteen. Yet any man who has reached the age of fifty-seven is certainly no longer an infant prodigy; and if he has accomplished anything in life, whatever temporary conspicuousness he may have had as a prodigy has lost all importance in view of the much greater issues of success or failure in his later life.

But the present book does not attempt to be an evaluation of my whole life for better or for worse. It is rather the study of a period in which I underwent a rather unusual and early course of education, and of the subsequent period in which the unevenness and irregularity thus accentuated in my nature had an opportunity to work themselves out in such a way that I could consider myself launched on an active career both as a scholar and as a citizen of the world.

The infant prodigy or *Wunderkind* is a child who has achieved an appreciable measure of adult intellectual standing before he is out of the years usually devoted to a secondary school education. The word "prodigy" cannot be interpreted as either a boast of success or a jeremiad of failure.

The prodigies whom we generally call to mind are either people like John Stuart Mill and Blaise Pascal, who had proceeded from a precocious youth to an effective adult career, or their antitheses, who have found the transition between early precocity and later effectiveness one which they have become too specialized to make. Yet there is nothing in the word itself which restricts us to these two opposite cases. It is perfectly conceivable that after an especially early start, a child may find a place in life in which he has a good, modest measure of success without having stormed Olympus.

The reason infant prodigies are generally adjudged in terms of immense failure or immense success is that they are somewhat rare phenomena, known by hearsay to the public; and, therefore, the only ones the public ever hears of are those who "point a moral or adorn a tale." There is a tragedy in the failure of a promising lad which makes his fate interesting reading; and the charm of the success story is known to all of us. Per contra, the account of a moderate success following a sensationally promising childhood is an anticlimax and not worth general attention.

I consider this attitude of extremes toward the infant prodigy false and unjustified. In addition to being unjustified, it is in fact unjust; for this feeling of anticlimax which the story of the moderately successful prodigy excites in the reader leads the prodigy to a self-distrust which may be disastrous. It takes an extremely solid character to step down gracefully from the dais of the prodigy onto the more modest platform of the routine teacher or the laboratory floor of the adequate but secondary research man. Thus the child prodigy who is not in fact a prodigy of moral strength as well must make a career success on a large scale, for want of which he is likely to consider himself a failure, and actually to become one.

The sentimentality with which the adult regards the experiences of a child is no genuine part of a child's attitude toward himself. To the grownup, it is lovable and right for the child to be confused, to be at a loss in the world of adults around him; but it is far from a pleasant experience for the child. To be immersed in a world which he cannot understand is to suffer from an inferiority which has no charms whatever for him. It may be ingratiating and amusing to his seniors to observe his struggles in a half-understood world, but it is no more pleasant for him to realize that his environment is too much for him to cope with than it would be for an adult under the same circumstances.

Our present age is sundered from that of the Victorian by many changes, not least among which is the fact that Sigmund Freud has lived, and that no one can write a book nowadays without being aware of his ideas. There is a great temptation to write an autobiography in the Freudian jargon, more especially when a large part of it is devoted to the very Freudian theme of father-and-son conflict. Nevertheless, I shall avoid the use of this terminology. I do not consider that the work of Freud is so final that we should freeze our ideas by adopting the technical language of what is certainly nothing but the contemporary phase of a rapidly growing subject. Yet I cannot deny that Freud has turned over the stone of the human mind and has shown a great population of pale and emotionally photophobic creatures scuttling back into their holes. However, I do not accept all Freudian dogmas as unquestionable truth. I do not consider that the present vogue of the emotional strip-tease is a wholly good thing. But, let my reader make no mistake: the resemblance between many of the ideas of this book and certain of the notions of Freud is not purely coincidental; and if he finds that he can carry out the exercise of translating my statements into Freudian jargon, he should consider that I am quite aware of this possibility and have rejected it deliberately.

1 A Russian Irishman in Kansas City

My impression of the intellectual environment in the first decade of this century is intense and actual, and I have learned much of it as a child sitting in the kneehole of my father's desk while he discussed with his friends the vicissitudes of those times and the facts of all times. Child as I was, I absorbed a real understanding of many things, and my childish point of view is not totally devoid of significance. Those of us who make scholarship our career are often able to take the dispersed and uncorrelated memories of our childhood, covering much which we had not understood at the time they were received, and to build them into an organized and cogent structure.

Today all of us have grown up into an age which, while it may be an age of losses and of decline, has also been an age of new beginnings. In these beginnings the scientist and even the mathematician have had a large share. I have been both a witness of and a participant in them. Thus I may speak of them, not only with the understanding of the participant, but I hope also with something of the normative judgment of the objective critic.

That part of my work which appears to have excited the greatest public interest and curiosity concerns what I have called Cybernetics, or the theory of communication and control, wherever it may be found, whether in the machine or in the living being. I have had the good fortune to say something about these matters. This has not been merely the *aperçu* of a moment. It has its deep roots both in my personal development and in the history of science. Historically it stems from Leibnitz, from Babbage, from Maxwell, and from Gibbs. Within me, it stems from the little I know of these masters and from the way this knowledge has fermented in my mind. Therefore, perhaps, an account of the origins of my predisposition toward these ideas, and of how I came to take them as significant, may be of interest to the others who are as yet to tread my road.

As far as I know, I am about seven-eighths Jewish in ancestry, with one possible German Lutheran great-grandparent on my mother's side. Because I am myself overwhelmingly of Jewish origin, I shall have more than one occasion to refer to Jews and Judaism. Since neither I myself nor my father nor, so far as I know, his father has been a follower of the Jewish religion, I must explain the sense in which I tend to use the word "Jew" and all related words such as "Judaism" and "Gentile" which are given a definition in terms of the master word.

The Jews seem to me primarily a community and a social entity although most of them have been members of a religion as well. Nevertheless, when this religion has begun to offer a less impenetrable barrier to the surrounding community, and when the surrounding community has begun to offer a less impenetrable barrier toward it, there are many factors in the life of those who had adhered to the religion which have continued to follow more or less the original religious patterns. The Jewish family structure is somewhat closer than the average European family structure, and much closer than that of America. Whether the Jews have had to meet a religious prejudice or a racial prejudice or simply a minority prejudice, they have had to meet a hostile prejudice, and even though this may be disappearing in many cases, the Jews are well aware of it, and it has modified their psychology and their attitude toward life. When I speak of Jews and of myself as a Jew, I am merely stating the historical fact that I am descended from those belonging to a community which has had a certain tradition and body of attitudes, both religious and secular, and that I should be aware of the ways in which I myself and those around me have been conditioned by the very existence of this body of attitudes. I am saying nothing about race, for it is obvious that the Jews have sprung from a mixture of races, and in many cases are being absorbed again into another mixture. I am saying nothing about Zionism and other forms of Jewish nationalism, for the Jews are much older than any movements of this sort which have amounted to more than literary and ritual conventions, and might well continue to exist even though the new state of Israel succumbs or gives way to other manifestations of nationalism. I do not pretend to assign a normative value either to language or religion or race or nationalism, and least of all to mores. What I assert is that I myself and many of those about me come from an environment in which our knowledge of the fact of our Jewish origins is significant for our own understanding of what we are, and for our proper orientation in the world about us.

On the side of my father, Leo Wiener, the documents are scanty and perhaps largely irrecoverable. This is all the more so since the Nazi sack of the White Russian city of Byelostok during the Second World War. There my father was born. My grandfather is said to have lost the family genealogy in the burning of a house in which he lived, although indeed from what I have heard of him he was capable of losing documents in the most calm and sober of times. As I shall point out later, there is a tradition that we are descended from Moses Maimonides, the Jewish philosopher of Cordova, and the body physician of the Vizier of King Saladin of Egypt. Even though a man is but a distant kinsman of his own ancestors of seven hundred years ago, I should like to imagine that this tradition is true, because Maimonides, the philosopher, the codifier of the Talmudic law, the physician, the man of affairs, is a much more pleasing ancestor to me than most of his contemporaries. It would scarcely be respectable to claim to be the descendant of a medieval monk—the only type of intellectual then existing in Western Christendom. But I am afraid that after so much passage of time our supposed ancestry is a very shaky legend, and perhaps is based on no more than a dash of Sephardic blood which has leaked into our veins at some epoch.

The next outstanding figure in our ancestry is a much more certain one, even though I find him far less attractive. He is Aqiba Eger, Grand Rabbi of Posen from 1815 to 1837. Like Maimonides, he was recognized as one of the greatest Talmudic authorities, but unlike Maimonides, he was opposed to secular learning, which was coming into Judaism through such men as Mendelssohn. On the whole, I feel quite content that I did not live in his times and that he does not live in mine.

My father has told me that one thread of our ancestry leads down through a family of publishers of the Jerusalem Talmud which appeared in Krotoschin in 1866. I do not know their precise relationship to my grandfather, Solomon Wiener. I only saw my grandfather once in New York when I was a small boy, and he made no particular impression on me. I gather that he was a scholarly journalist and a most irresponsible sort of person, unable to hold his family together. He was born in Krotoschin, but married and settled down in Byelostok, where my father was born in 1862. One thing he did has had a great though indirect influence on my life: he sought to replace the Yiddish of his environment by literary German. In doing so, he made certain that German should be my father's native language.

My father's mother came from a family of Jewish tanners in Byelostok. I am told that they had been honorary citizens of Russia in the old days. For a Jew this amounted to a minor patent of nobility. For example, when the Tsar came to Byelostok, it was the house of my grandmother's family that was selected as his residence. Thus, their tradition was somewhat different from the tradition of learning of my grandfather. I suspect that it was his solid, business-like habits that gave my father a firm footing in life; and although he was an enthusiast and an idealist, he had his feet well planted on the ground and was always a good custodian of the family responsibility.

Let me insert here a word or two about the Jewish family structure which is not irrelevant to the Jewish tradition of learning. At all times, the young learned man, and especially the rabbi, whether or not he had an ounce of practical judgment and was able to make a good career for himself in life, was always a match for the daughter of the rich merchant. Biologically this led to a situation in sharp contrast to that of the Christians of earlier times. The Western Christian learned man was absorbed in the church, and whether he had children or not, he was certainly not supposed to have them, and actually tended to be less fertile than the community around him. On the other hand, the Jewish scholar was very often in a position to have a large family. Thus the biological habits of the Christians tended to breed out of the race whatever hereditary qualities make for learning, whereas the biological habits of the Jew tended to breed these qualities in. To what extent this genetic difference supplemented the cultural trend for learning among the Jews is difficult to say. But there is no reason to believe that the genetic factor was negligible. I have talked this matter over with my friend, Professor J. B. S. Haldane, and he certainly is of the same opinion. Indeed, it is quite possible that in giving this opinion I am merely presenting an idea which I have borrowed from Professor Haldane.

To return to my grandmother, it is certain, as I have said, that she received very little aid from my grandfather at any time, and the young family had to be brought up to earn its own living. The age of thirteen is a rather critical one in the Jewish tradition, for it represents the admission of the boy into participation in the religious community. In general the prolongation of youth into the period of high school and college which belongs to our Western culture is foreign to Judaism. From the beginning of adolescence, the Jewish boy is given both the dignity and the responsibility of a man. My father, who was an intellectually precocious child, had begun to support himself at the age of thirteen by tutoring his fellow students. At that time, he already spoke several languages. German was the language of the family, and Russian that of the State. The role of German in his life was reinforced by the fact that because of the German bias of my grandfather my father went to a Lutheran school. He learned French as the language of educated society; and in Eastern Europe, especially in Poland, there were still those who adhered to the Renaissance tradition and used Italian as another language of polite conversation. Moreover, my father soon left the Minsk Gymnasium for that of Warsaw, where the classes were also conducted in Russian, although Polish was the language that he spoke with his playmates.

Father always felt very close to his Polish schoolmates. He told me that so far as he knew he was the only non-Pole at that time to be identified with the underground Polish resistance movement and to be privy to its secrets. As a Warsaw Gymnasium student he was a contemporary of Zamenhof, the inventor of Esperanto, and although the two were students in different Gymnasia, my father was one of the first to study the new artificial language.

This gives more force to his later rejection of its claims, and indeed of those of all artificial languages. He asserted, and I believe rightly, that by the time an artificial language would have developed a sufficient tradition to be used with the intellectual precision and emotional content of the existing natural languages, it would also have developed a burden of idiom structure equal to that of its competitors. Father's fundamental idea was that to a very considerable extent the difficulty of a language reflects the amount of thought that has come to make up its tradition, and that the English language is as dependent on its idioms for expressing complicated ideas as written Japanese (which can express every word by its phonetic notation) is dependent on Chinese characters for terseness. Father always considered Basic English less basic than debased. No language with idioms adequate to express complex ideas concisely, he said, would be able to serve as an easy vehicle of neutrality between competing cultures.

From the Gymnasium my father went on to the medical school of the University of Warsaw. I dare say that at least part of his motive was one so common in Jewish families, which generally desire to have at least one son a professional man and if possible a doctor. The motive is strong and easily understandable in a group which has long been undervalued in the community. Lord only knows how many tongue-tied rabbis, discontented lawyers, and physicians without a practice this motive has produced.

At any rate, my father soon found out that he had not the stomach to be a doctor. The work of dissection, as well as, I surmise, the self-defensive coarseness of his fellow students, sickened him. At any rate, he soon left Warsaw to enroll in the Polytechnicum which was then in Berlin, although it has now been situated for many years in Charlottenburg.

Father came to Berlin with an excellent secondary training. The Gymnasium, as opposed to the Realgymnasium and the Oberrealschule, stresses the classics above all else, and my father was an excellent Latin and Greek scholar. However, the Gymnasium does not scamp its mathematical training. Indeed, Father continued to be an amateur mathematician all his life, and to contribute now and then to obscure popular American mathematical journals, so that it was not until I had begun work which was already of late college or early graduate-school standard that I came to feel that I had outdistanced him.

I do not know that my father was much more successful as a budding engineer than as a budding doctor. He has told me very little of this time of his life save that it included the frugal beer, cigars, and meat cookery of the impecunious Jewish student. I do know that he worked in the drafting room between a Serb and a Greek, and that he added Serbian and modern Greek to his linguistic repertory.

My father had wealthy relatives in Berlin. They were bankers, connected with the Mendelssohn bank, with traditions going back to Moses Mendelssohn and the eighteenth century. They tried to persuade my father to join them as bankers, but he did not like the confined life, and was still hungry for adventure.

One day he happened to go to a student meeting of humanitarian nature. The speeches reinforced a vein of Tolstoyanism which had long been in him, and he decided to forswear drink, tobacco, and the eating of meat for the rest of his life. This decision certainly had important consequences for my future. In the first place, without this decision my father would never have come to the United States, he would never have met my mother, and this book would never have been written. However, assuming for the sake of argument that all these events had occurred in their due sequence, I should still not have been brought up as a vegetarian, should not have lived in a house in which I was surrounded by horrible and hair-raising vegetarian tracts concerning cruelty to animals, and should not have been subjected to the overwhelming precept and example of my father in such matters.

All this is speculation. The fact is that Father did join up with a student colleague in a wild undertaking to found a vegetarian-humanitarian-socialist community in Central America. His companion reneged, and Father found himself alone on board a ship bound for Hartlepool, after showing a bewildered official his Russian school certificate in lieu of the German military papers which he should have possessed. After crossing England to Liverpool, he sailed again for Havana and New Orleans. This was a two weeks' trip, during which my father learned the essentials both of Spanish and English. I am told that he took his English very largely out of the plays of Shakespeare. The combination of his linguistic fluency with his archaic vocabulary must have left quite a strange impression on the people he met on the levee at New Orleans. The harebrained scheme of a Central American colony had already exploded, and Father was left with his career to make in the United States.

I have before me as I write a copy of a series of articles entitled "Stray Leaves from My Life," written by my father in the spring of 1910 for the Boston *Transcript*—the dear, stodgy, civilized old *Transcript!* It is a shock for me to realize that they were written by my father when he was ten years younger than I am now. They cover his youth and education in Europe, his trip to America, and his life here until he had arrived at a successful academic career at the University of Missouri. They are written with the full romantic joy in living and the adventurous indifference to poverty and hardships that belong to a vigorous young man, more especially to one who has just escaped from the rigid discipline of a European secondary education. *Dans un grenier, qu'on est bien à vingt ans!*

The expatriate American who consciously seeks the titillations of Bohemia on the Left Bank is generally ill-prepared for this experience, and is not aware of its real significance to the young European. He has never been subject to the strict discipline which belongs alike to the French *lycée,* the German Gymnasium, and the English public school. He does not feel the utter hunger for a period of growth and freedom between his escape from this servitude and the stricter servitude of earning his own living in a hard competitive world. For him Bohemia is nothing but an extra period of laxity superimposed on the lax and undemanding education he has already known. Even worse, it is a laxity in which he has freed himself from the demands and standards of American society without assuming those of the country in which he finds himself. He is lucky if he does not give himself over completely to drink, lust, and unproductive sloth.

The European boy, on the other hand, and especially the European boy of the last century, had to burst the hard cocoon of an education, effective, severe, and traditional, and to try his wings. It mattered little whether he did this among the cloistered pleasures of Oxford or Cambridge, in the beer-and-song *Burschenleben* of the German university, or in a garret in the *Quartier Latin.* The supreme assertion of youth and freedom was to wander in new lands at a time when the United States was itself emphatically a new land.

Thus Father's artless tale is written in the genuine spirit of the purely American counterpart found in Mark Twain and Bret Harte. It breathes the quintessence of youth, courage, and adventure, seen through rose-colored glasses. One senses the dust of the Southern roads and the new-turned furrow of the Kansas farm, the clamor of the raw Western city, and the keen wind off the peaks of the Sierras. All through it goes the slight, active, bespectacled figure of my father, alert to everything strange and striking, living the new life to the full, losing a job and taking a job without thought of the morrow, and having a glorious time through it all.

He was a small man, about five feet two or three inches in height, very quick in his movements, and a person who made a sharp and definite impression on everyone who saw him. He had the chest and shoulders of an athlete, with narrow hips and slender legs, and in those days he had the lean alertness of the athlete as well. His eyes were dark and flashing and beamed quick and penetrating intelligence behind the heavy glasses of the myope. His hair and mustache were black and remained black until his late middle age, and his face was ascetic. An enthusiastic walker and cyclist, he used to lead a crowd of young people on excursions into the country; and I still remember a photograph of a group of these in which he was standing beside a high-wheeled bicycle of early vintage.

He had a sharp and decisive voice and an excellent command of English, as of every other language which he spoke. I am told that he had a strong foreign accent, but through habit my ear was early blunted to this, and my impression of his English is that it showed its foreignness more in an excessive precision of diction and of vocabulary than in any other way.

He was a combative and fascinating conversationalist, although with his great intellectual power and aggressiveness it was difficult for him to limit his share in a discussion. Many times what he said would be a series of brilliant dicta rather than the give and take which best brings the other person out. He was impatient of fools, and I am afraid that to his keen intelligence very many people seemed to be fools. He was kind and beloved by his students, but he could be overwhelming through the very impact of his personality, and he was constitutionally incapable of allowing for his own forcefulness.

He was an enthusiastic farmer and outdoor man and a tireless walker. He tended to impose his amusements and preferences on those about him without fully realizing that many of them might have come to a fuller participation in a life together with him if this participation had not been so obviously enforced. One of his particular hobbies was the collection, cooking, and eating of such fungi of the region as were reasonably safe. Perhaps the infinitesimal chance of a catastrophe from eating poisonous mushrooms lent a certain savor to this sport.

He was eighteen when he arrived in New Orleans in 1880, with fifty cents in his pocket. Most of this went into his first few meals of bananas, and he had to look around for work. His first job was in a factory where cotton was baled by a hydraulic press. However, when a comrade fell into the press and was badly mangled, Father lost his interest in the work. Then he got a job as a water boy on a railroad being built across Lake Pontchartrain. He lost the job through some maladroit blunder of a boy unfamiliar with manual labor. There followed a period of aimless tramping through the deep South with a comrade or two, and after that a further period of farming in Florida and in Kansas. No farmer can be more enthusiastic than the Jew when he decides to turn his hand to the plow. To my father's dying day, he was more pleased by raising a better crop than his professional farmer-neighbors than he would have been by the greatest philological discovery.

At one point in his farming career, Father ran into the remains of an old Fourierist community in Missouri. It had gone to seed, and all the efficient people had left it, while all the rogues and footless, incompetent idealists remained. Father soon had his fill of this, and while he continued to be a Tolstoyan all his life, he never afterward had much use for those whose idealism was not mixed with a certain practical sense.

I don't know just how Father came to Kansas City in the first instance, nor exactly what he did there. There was a period during which he worked as a peddler. On another occasion he pushed a broom through a Kansas City store. By this time the fun of the new American adventure was wearing rather thin. Father had begun to be a bit envious of the well-clad customers. He had decided that he was entitled to some of the pleasures and amenities of the life about him. It must have been during this time that he passed a Catholic church on which there was a sign: GAELIC LESSONS GIVEN. This was too much for Father's philological curiosity to bear. He joined the class; and because he was a far more gifted linguist than the others, he rapidly became the teacher of the class and soon the head of the local Gaelic society.

The fame of the "Russian Irishman," as he was called, was widespread about Kansas City. It had for some time been notorious in the public library that the humble immigrant peddler was calling for books that no one else could read, and was reading them.

At length my father decided to end this anomalous existence and to go back into that intellectual work for which he was cut out. He ventured to ask the Kansas City superintendent of schools for a job; and after a trial period in a wild sort of country school in Odessa, Missouri, he was taken on in the Kansas City high school. Here he showed himself a brilliant teacher, a great friend of the students, and a reformer who left his mark on the Kansas City school system. When Father was teaching (but not always when he was teaching me), he tried to draw out his students' interests rather than to compel them to think in preassigned directions. He aimed at exciting their independent thought, not their involuntary obedience. He took part in their sports and their excursions, and managed to transfer to them some of his love for the out-of-doors.

During the period of his teaching at the Kansas City high school, my father took a trip to California with some of his friends. He used to delight in telling me of the romantic city of San Francisco, of his tramp through the Yosemite Valley, and of his initiation into mountain climbing in the high Sierras. He told me that on the occasion of his climbs, he met a lady tourist, who was greatly interested in the young man's romantic love of nature and adventure. This lady was Miss Annie Peck, who later became one of the most distinguished mountaineers of her generation, and who made notable climbs in the Andes, among them the ascent of Chimborazo and Cotopaxi. Later on Miss Peck wrote to Father, saying that her alpinism was first stimulated by his enthusiasm.

One of my father's amusements during his Kansas City period was to attend spiritualistic seances and to try to discover the sleight-of-hand technique of the mediums. I don't think that Father was very much excited with the ideas of spiritualism either pro or con, but the chance to do a little detective work appealed to his adventurous spirit and to his intellectual curiosity. He became firmly convinced that if there were anything whatever in spiritualism, it was not to be found among the mediums he had investigated.

The budding culture of the Middle West of this period was attracted to the anfractuous style and the puzzling allusions of Browning's poetry. Of course, to a man of my father's breadth of cultural background neither the style nor the allusions offered much difficulty. Father became rather a lion to roar at the meetings of ladies' Browning clubs, and I believe it was there that he met my mother. Be that as it may, they certainly enjoyed reading together *The Ring and the Book* and *On a Balcony.* My own name and that of my sister Constance represent characters from *On a Balcony,* and we are thus unwilling fossils of a bygone intellectual era. I must think that my parents' indifference to the consequences of giving me such a recondite and unusual name was part and parcel of the decision which they had already made to direct and to channel my life in every detail.

My father chose to become a teacher of languages. He might almost as readily have become a teacher of mathematics, for he had both talent and interest in the field. Indeed, throughout my college training I learned the large part of my mathematics from him. There are times when I think that it would have been more fortunate for my father if he had taken mathematics for his field rather than philology. The advantage is that mathematics is a field in which one's blunders tend to show very clearly and can be corrected or erased with a stroke of the pencil. It is a field which has often been compared with chess, but differs from the latter in that it is only one's best moments that count and not one's worst. A single inattention may lose a chess game, whereas a single successful approach to a problem, among many which have been relegated to the wastebasket, will make a mathematician's reputation.

Now, philology is definitely a field dependent on the careful assessment of a number of small considerations rather than on a mechanical ride in a train of logic. For a man with intuitions and imagination, philology is a field in which he may easily go wrong and in which, if he does go wrong, he may never find it out. The mathematician who makes serious mistakes and never finds them out is no mathematician, but an imaginative philologist may go very far wrong before a demonstrable error pulls him up sharp. My father's success in philology was unquestioned, but his sanguine temperament would have benefited under the discipline of a field in which discipline is automatic.

This, then, was the strange young man who became my father and my teacher. In 1893 he married my mother, who had been Miss Bertha Kahn, the daughter of Henry Kahn, a department store owner, of St. Joseph, Missouri. Let me say something of my mother and her background.

2 The Proper Missourians

Henry Kahn, my mother's father, was a German Jewish immigrant from the Rhineland, and a department store owner of St. Joseph, Missouri. His wife belonged to a family named Ellinger, which had been settled in the United States for at least two generations previously. I gather that my grandmother's mother was not Jewish. This seems to have introduced a peculiar pattern of marriages into the Ellinger family, so that the girls of that generation tended to marry Jews like their father, and the boys Gentiles like their mother. At any rate, even a hundred years ago the family was hovering in a state of unstable equilibrium between its Jewish background and an absorption into the general community.

This tendency for the marriage pattern of the boys to differ from that of the girls is something of which I have heard under quite different circumstances. I have been told of a family in New York in which the wife was Dutch and the husband a Chinese Protestant clergyman. All the boys of that marriage seem to have lost themselves in the general American background, taking American wives and putting behind them the Oriental part of their ancestry. The girls, on the other hand, all married Chinese and returned to China. In that case, the motive for the differentiation seems to have been the extreme demand on the part of young Chinese men for wives with a Western education and background, which gave the daughters unusually favorable opportunities for Chinese marriages as against American marriages, conditioned at least in part by the restrictions of race prejudice. Whether motives paralleling these to at least some extent existed as well in the Ellinger family, I do not know; but it is interesting to observe the phenomenon cropping up in such different situations. The Sword follows the Sword, the Distaff the Distaff.

The Ellinger family appears to have had its American origins in Missouri and perhaps even further to the south. Its members combined a proper Southern gentility of outlook with a high degree of unpredictableness.

More than one of the men ended a career of impeccable propriety by suddenly leaving his family and taking to the great open spaces. There is a legend that one of the Ellinger family finally became a Western bandit and was shot down while resisting capture.

Even apart from such radical manifestations of individuality, the Ellingers were and are a centrifugal clan. It is only in the later history of the family that the gradual dilution of these idiosyncrasies has permitted them to take the full place in the community that their abilities have always justified.

We are now used to the fact that almost all of us are the descendants of immigrants. This was not true in the middle of the last century. Today the melting pot not merely melts but alloys. It does this much more easily because it is not overloaded with strange and refractory metals which have been thrown into it cold. The immigrant family which has already begun to lose itself in the general American picture is no longer faced by the even more recent immigrant who has just stepped out of the steerage. No longer is our American of Continental ancestry confronted with a hierarchy of greenhorns, established immigrants, and old Americans, forming a stable ladder of social ascent in which each man has his rigid place.

In many ways the early immigrants who had the easiest emotional time of it were the Eastern European serfs, who had almost literally nothing to lose but their chains. For the immigrants of higher caste in the European system, their Americanization and subsequent ascent in the social hierarchy was preceded by a disinheritance and a leveling downward.

All this was inevitable and perhaps even an essential part of the discipline which the foreigner needed to fit him to take a place in a community very different from the one into which he was born. Today, however, the immigrant is not only the beneficiary but a benefactor to the country to which he has immigrated. His native culture often has a richness which should not be degraded and lost in the general mist of a tradition averaged thinly over a continent. There are facets of his art and his thought, of his folklore and his music, which are well worth resetting in the regalia of America. Yet in the presence of the overwhelming invasion of greenhorns from the old country, these heirlooms of the immigrant were difficult to evaluate and to appreciate. He was only too likely to accept without protest the inferior position which was allotted to him, and to protect his ego by a similar depreciation of the even more recent immigrant.

In such a community and such a period, respectability is a pearl beyond price. Schmidt becomes Smith, and Israel Levin becomes Irving Le Vine. The evangelical (and, incidentally, also the rabbinical) religious injunction to avoid even the *appearance* of evil is interpreted as an injunction to avoid the appearance of evil and vulgarity far more than evil and vulgarity themselves. A strong character may indeed defy such a society and live according to his own values. It is much easier for the less forceful character to accept these values, and to fall on his knees before Mrs. Grundy. Only a man like my father, who was ready to defy the almighty Jehovah himself, was not likely to fall into the orthodoxy of Grundy worship.

The specific frustrations of the immigrant and Jewish families of the seventies and eighties and nineties were reinforced by the general moral backwater of the Gilded Age. It was an age in which the Grant of the Whiskey Ring had replaced the Grant of the Civil War and in which Lincoln was dead and the Daniel Drews and the Commodore Vanderbilts were very much alive. The enthusiasms and devotions of Civil War times had run out and the enthusiasms and devotions of the twentieth century had not yet appeared above the horizon. There was a general slackness and letdown. This letdown must have been most intense in the defeated South, and in the Missouri which was almost an anteroom to the South.

Besides these stresses and strains belonging to the social group and to the time, my mother's family was also subject to more personal dissensions. Her family was split by an alienation between her parents. My mother's mother was a person of good general culture and vigorous and uninhibited emotions. She had a strong, undirected vitality which enabled her to live to a great age, and she was simply too much for her quieter and less energetic husband.

Then, too, one of my mother's older sisters had ambitions to be the woman intellectual of the family, and rather looked down on her sisters. This led to an ultimate rift, in which my mother and father stood on one side and most of her family on the other. At least one cause of this rift was the traditional friction between the German Jew and the Russian Jew and their differences in social status. This was supplemented by my father's downrightness and naïveté in social matters.

In any case, my mother, with my father's support, gradually broke with her family. Although he must often have been a puzzle to her, she was deeply in love with my father, and admired him greatly. Nevertheless, it was not an easy step for my mother to take. She had been brought up with the indulgence often extended to the belle of the family. I remember one photograph of her made when I was about four years old. She looked extremely handsome in the short sealskin jacket of that period. I had great pride in that picture and in her beauty. She was a small woman, healthy, vigorous, and vivacious, as she has indeed remained to this present day. She still carries herself like a woman in her prime.

In the family of divided roots and Southern gentility into which she was born, etiquette played a perhaps disproportionately large part, and trespassed on much of the ground which might be claimed by principle. It is small wonder, then, that my mother had, and conceived that she had, a very heavy task in reducing my brilliant and absent-minded father, with his enthusiasms and his hot temper, to an acceptable measure of social conformity.

The permissions and demands of our society in this matter differ widely for the man and the woman. For the man, a certain degree of shaggy unconformity may be permitted in return for character and genius. But the woman is expected to be the custodian of the more conformist and conventional virtues, which indeed need someone to cultivate them. A man may have a hot temper without reproach, but a woman must be smooth and suave. When I was born there was added to my father's downrightness the problem of a child with somewhat similar ability, the same hot temper, and the same resistance to taming; and it is not surprising that my mother sometimes must have felt at her wits' end. Later, when my father's temper and mine came into head-on collision, mother could do little but act as a general peacemaker, without indulging in any too definite opinions or convictions of her own on which to base this peacemaking. This made it hard for me to understand her. In my collisions with my father, dramatic as they were, I could generally recognize a principle which I had to respect, even when I was suffering from my father's interpretation of it. My mother was scarcely able to afford such luxuries. When the husband is a zealot, the wife must be a conformist. How many unworldly scholars, whether Jews or Christians, must have depended for their very existence on their conformist wives!

When my parents married, my father was already Professor of Modern Languages at the University of Missouri in the town of Columbia. He taught both French and German, and my parents shared in the simple social life of a small college town. They lived in a boardinghouse together with many members of the faculty, and there I was born on November 26, 1894.

Of course, I have no memories of the town which I left as a babe in arms, but there were family stories which go back to that boardinghouse, and to Father's friend of those days, W. Benjamin Smith (who later taught mathematics at Tulane University). He was a particular crony of my father's and a great practical joker. One time Smith came back to the boardinghouse and found that the colored waiter of ample dimensions had been replaced by a wizened little fellow. "Sam!" roared Professor Smith, "how you have shrunk!" The puzzled waiter left the room on the run, never to return.

I mention a story concerning Smith and a Negro since it was on this racial issue that his friendship with my father broke up many years later. Smith, who was an unreconstructed rebel, had published a pseudolearned book on the inferiority of the Negro, and that was too much for my father's liberalism, and for his respect for facts.

As little more than an infant, I accompanied my parents on a one-way trip that ended in Boston. The motive power behind this trip was a power rooted deeply in Missouri. A Missouri politician had his eye on my father's job for kinsman or henchman. My father had met with such success that it was no longer possible to run Modern Languages at the University of Missouri as a one-man show. It was decided to put a man in charge of each of the departments of French and German. When offered his choice, my father chose German. Unfortunately, there was a protégé or connection of the politician's family who also had his eye on this chair, and Father was left out when the department was split. Father had no connections in the academic life of the rest of the country. He came to Boston on a pure speculation, for he thought that it was best to look for jobs where the jobs were.

He soon attracted the attention of Professor Francis Child, the learned editor of *Scottish Ballads*. Child traced the Scottish ballads and their parallels through the various languages of Europe and Asia, and needed help in collating sources for many of them. Father was given the southern Slavic languages as his assignment. He made himself so useful to Child that Child helped him find a position around Boston. Father's first teaching jobs were at Boston University and the New England Conservatory of Music, and he did some work in the cataloguing department of the Boston Public Library. Finally, Child obtained for him an instructorship in Slavic Languages at Harvard, the first of its kind at Harvard and, I believe, in the country. This led to a career of gradual promotion through the successive ranks of assistant professor and professor until my father retired in 1930.

Nevertheless, for many years he had to reinforce his salary with outside jobs. Although living expenses were low, salaries were also low. Father continued his work at the New England Conservatory and at Boston University for several years, and did occasional jobs for the Boston Public Library. He also did a considerable amount of important etymological work for several editions of the Merriam-Webster *Dictionary*; in this work he was associated with Professor Schofield, also of Harvard. In later years, Father's main source of academic pin money was Radcliffe College, which had served Harvard professors for so many years as a supplement to salary.

Professor Child was a remarkable and most democratic person, and a sincere friend of my father. One day Father saw another short, nearsighted, vigorous young man leaving Child's house. When Father came in, Child told him that he had just missed meeting Rudyard Kipling. Apparently Kipling had got off to a bad start with Child, who had been out in his garden in his old clothes watering his roses. Mr. Kipling had mistaken him for the hired gardener. "Ah," said Child, "a workman looked over my fence yesterday and was drinking in the odor of my flowers. *He* was my brother."

3 First Remembered Patterns

1894–1901

It is a disservice of some Freudians (and I do not mean Freud himself) to have reduced the infant to a homunculus possessing little mental life outside of a rudimentary sexuality. Many a Freudian looks askance at all other recollections from infancy and very early childhood. I will not for a moment deny that infantile sexuality exists and is important. But it is far from an exhaustive description of the child's early mental life, both emotional and intellectual.

My conscious memory goes back to a time when I was about two years old, when we lived in a second-story apartment on Leonard Avenue, in a rather obscure and not too desirable region on the boundaries of Cambridge and Somerville. I remember the staircase leading to our quarters: that it seemed to rise for what was for me an interminable distance. We must have had a nursemaid even at that remote period, for I recollect going out with her to make purchases at one of the little shops which I was told was in Somerville. The whole region is a confusion of streets belonging to the unconformable systems of the two cities, and I distinctly remember the acute angle at which these streets intersected before our particular grocery shop.

Around the corner was a rather grim and terrifying building which, I learned, was a hospital for incurables. It still stands, and is the Hospital of the Holy Ghost. I am quite certain that I had at that time no clear idea of what a hospital was, and even less idea of an incurable, but the tone in which my mother or our nursemaid mentioned the place was enough to fill me with gloom and foreboding.

This is all that I can truly remember about Leonard Avenue. I was later told that my mother had a second child there who died on the day of its birth. When I was told this, I was a child of thirteen; the news shocked me to the extreme, for I was afraid of death and had comfortingly believed that our own family circle had never yet been broken. I have no direct recollection of the existence of this child, and I still do not know whether it was a boy or a girl.

We spent the summer of 1897, when I was two and a half years old, in a hotel in Jaffrey, New Hampshire. There was a pond nearby with rowboats on it, and a path leading up a mountain whose name I understood to be Monadnock. My parents climbed the mountain, naturally without me, and they also took me to a neighboring village where for some reason or other they visited the blacksmith shop. The blacksmith had had a toe crushed from a horse stepping on it, and I was frightened to hear of it, for even at that time I had a very lively terror of injury and mutilation.

The academic year of 1897–98 found us on Hilliard Street in Cambridge. I have a dim recollection of seeing the moving van which transferred our goods from Leonard Avenue. From this point on, my recollections come thick and fast. I remember my third birthday, and my two playmates, Hermann Howard and Dora Kittredge, the children of Harvard professors who lived on the same street. I am sorry to say that my first recollection of Hermann is quarreling with him at his own birthday party, when he was five and I was three.

I am told by my parents that while we lived on Hilliard Street I was taught French by Josephine, a French maid who worked for us. I have no recollection of Josephine herself, but I do remember the children's textbook which she used, with the names and pictures of a spoon, a fork, a knife, and a napkin ring. What French I learned at this time I must have unlearned with equal rapidity, for when I studied French again in college at the age of twelve, no obvious trace of my former knowledge of the language remained.

It must have been Josephine who took me for walks on Brattle Street and about the Radcliffe grounds. The darkness of what I now feel as the pleasant shade of the trees on Brattle Street terrified me at the time; the geography of the neighboring street left me in utter confusion. There was a house on the corner of Hilliard and Brattle Streets which had a closed-off bay window that frightened me very much because it looked as if it were a blind eye. I had the same feeling of terror and claustrophobia when my parents had the carpenter close up some sort of serving hatch connecting the dining room of our house with the butler's pantry.

Not far from our house was an old school building, but whether it was used or deserted at the time I do not remember. Mount Auburn Street was only a few houses away, and around the corner was a blacksmith shop, with a driveway lined with white painted cobblestones. I tried to lift one of them once and take it away, and I was duly reproved. An alley by the side of our house led back to a little garden where an old gentleman by the name of Mr. Rose—at least he seemed old to me—would take the air and smoke his pipe. Behind that was another house where there lived two older boys who took me under their wing. I remember that they were Catholics and had in their house an image of the crucified Christ with the wounds and a crown of thorns which struck me as the image of a victim of cruelty and injustice. They also had a potted plant which they called Wandering Jew, and to explain its name they told me a legend which I did not understand but found very painful.

Over all this early period of my life, I have very little recollection of my father. My mother figures largely in my early memories, but my father was an austere and aloof figure whom I saw only occasionally in his library, working at his great desk. I used to play under that desk. I have no recollection of any coldness or harshness on his part, but the low timbre of the male voice was in itself enough to scare me. To the very young child, the only parent is the mother, with her solicitude and tenderness.

Mother used to read to me in the garden. I know now that the yard was a mere three or four feet of grass outside the front steps, but then it seemed to me enormous. The book from which she most enjoyed reading was Kipling's *Jungle Book,* and her favorite story was "Rikki-Tikki-Tavi." I myself was beginning to read at the time, but I was only three and a half years old, and there were many words that caused me difficulty. My books were not particularly adapted to my years. My father had an old friend, a lawyer by the name of Hall, who was blind in one eye and deaf in one ear, and quite abstracted from human society and ignorant of the needs of a small child. He gave me for my birthday the volume on mammals of the old *Wood's Natural History.* It was in small type, and an nth reprint at that, with the type and the woodcuts blurred and clogged with ink. My parents lost the original copy, but in order not to disappoint the old gentleman they promptly got another, and even before I could read it with any ease, I used to finger through the pictures.

Another book which I received as a present about the same time is rather a puzzle to me. I know that it was a children's book on elementary science, and I know that among other things it discussed the planetary system and the nature of light. I know that it was a translation from French, and that at least some of the woodcuts represented Paris. However, I do not know the name of the book, and I do not believe that I have seen it since I was five years old. Perhaps it was a translation of a work by Camille Flammarion. If any of my readers can identify the work and I can lay my hand upon it, I can certainly check up from my memory of the pictures whether it is the book to which I am referring. As I have made my career in science, and as the book was my first introduction to science, I should very much like to see the point from which I started.

I cannot remember many of my toys of that time. One, however, which I can remember with absolute clarity, was a little model of a battleship which I pulled along on a string. It was the time of the Spanish-American War, and toy battleships were all the rage among the youngsters. Even now I can recall the white paint and the straight masts of the battleship of that transitional period before the days of the dreadnaught, with the small deck turrets of the secondary battery, and only a few turrets containing guns of the larger caliber.

My nursery was a room at the back of the house, separated from other rooms on the second floor by a step or two. One day I stumbled and fell up this little flight of steps, gashing my chin and leaving a scar which I bear to this present day and which is one of the reasons for my wearing a beard. I also cut my hands on the metal fins of the little iron cot in which I slept. I can still recall just how uncomfortable it felt.

I remember the songs with which my parents sang me to sleep. My mother was a great lover of *The Mikado,* and its arias are among my very early recollections. Some music-hall songs also played a role in my childhood, and among these were "Ta-ra-ra-boom-de-ay," and "Hush, Hush, Hush! Here Comes the Bogey Man." My father preferred the "Lorelei," and a Russian revolutionary song which I never understood but whose syllables I remember to the present day.

My sister Constance was born in the early spring of 1898. The midwife, a genial Irishwoman by the name of Rose Duffy, was a particular friend of mine, and I named a rag doll after her. She lived on Concord Avenue with a sister, Miss Mary Duffy, who did her housekeeping. When I visited them I had the run of their box of gingersnaps and molasses cookies.

I am told that the arrival of my sister disconcerted me very much. Certainly some years after, when she was old enough to be an individual, I did begin to quarrel with her in a most reprehensible way, but this was succeeded by many years of companionship and good feeling. To have a baby in the house taught me much I did not forget later about the mysteries of bottles and diapers.

During that summer, Father traveled in Europe. I was delighted with his post cards from strange cities, with the text printed out for me in consideration of my childish inability to read handwriting. Also during that summer I began to read a certain natural history magazine which had pictures of birds. I can even recall the queer, old-fashioned advertisements on the pages of this magazine, but its name has escaped me.

Already Father had much contact with the staff of the Boston Public Library. One of his friends there, a Mr. Lee, had a wife who was an illustrator and author of children's books, and a little daughter of my own age. They lived in Jamaica Plain, within a stone's throw of Franklin Park. I remember reading Mrs. Lee's books and playing with the little girl in the stone grottoes of that part of the park. I remember the streetcar trip by way of Central Square and the Cottage Farm Bridge, across a part of Boston which has completely changed character since that time. I used to read the Lee girl's copy of *The Arabian Nights*. Some years later she fell victim to diabetes, which was a death sentence to a youngster in those days before insulin. Mr. Lee gave me the book, together with some other belongings of his daughter, but it was a sad pleasure for me to read it.

One of the other books which I read at the time was *Alice in Wonderland,* but it took me years to get the full flavor of Lewis Carroll's humor, and the metamorphosis of Alice in the book had a sort of terror for me. Indeed, when I saw a copy of *Through the Looking Glass* I lost all sense of humor and flatly regarded the book as superstitious.

I was an easily frightened child. One time when my parents took me to the old Keith's vaudeville theater for want of a baby-sitter, I saw a pair of slapstick comedians hitting one another about. After a sudden blow, one of them appeared in a glaring red wig, and the whole thing scared me so much that I burst out weeping and had to be taken out of the theater.

The next year after Father had returned from Europe, we continued to live in the same house on Hilliard Street. I was sent to a kindergarten on Concord Avenue, opposite the Harvard Observatory. I have not forgotten the woolies and long leggings which I put on, or the conducted games with the other children, or the webs of paper which we had to weave. I met my first sweetheart there, a dear little girl whose voice charmed me, and in whose neighborhood it was good to be. I recall the delightful visit we kindergarten children made to a nearby garden with its crocuses and tulips and lilies of the valley under the broad spreading fir trees.

We spent the summer of 1899 at Alexandria, New Hampshire. At four and a half, I was old enough to look out of the train window and to watch the landscape flash backward. I had already begun to be interested in the technique of the railroad. By this time I must have had a toy "puff-puff" of my own to add to my interest.

From that time to about 1933 I had no occasion to revisit Alexandria. When I first revisited it I found that my memories of the geography of the place exactly fitted the scene before me: Bristol, with its Civil War monument and its old-fashioned mortar in the center of the village square;

Newfound Lake, the boardinghouse where we stayed, the little house opposite it where a colleague of my father's had lived and with whose son I had played. All were just as I had pictured them from memory. I found the village of Alexandria itself unchanged, and Bear Hill, where I had taken a walk with my parents through the pine woods with their Indian pipes, and had had to be carried back on my father's shoulder. It all was just as I had fancied it. I well remember the textile mill at Bristol, with its whirring looms, where my father had taken me as a boy.

We spent the next winter—that of 1899 to 1900—in one half of a double house on Oxford Street in Cambridge. My parents already had plans to put me in school, and they took me with them to visit Miss Baldwin, the principal of the Agassiz School, which was only two doors from us. No final arrangement was made to put me in school. Miss Baldwin, an extremely distinguished schoolteacher and a woman of great dignity, was a Negro. She had entered the Cambridge school system in the 1880s at a time when the humanitarian impulse of the abolitionists had not entirely died out and before New England snobbery had surrendered to Southern gentility at the beginning of the twentieth century.

I received the *St. Nicholas Magazine* as a birthday present while I was living on Oxford Street. I remember very well the day on which the postman brought me one back number and one current number dated in 1899, and from then on, the new century had begun and I was in 1900. *St. Nicholas* was a revelation to me and constituted much of my most pleasant reading in my childhood. It is scarcely possible for me to conceive how the present generation of children gets along without it or its equivalent. *St. Nicholas* always assumed that the child, for all his few years, was an essentially civilized individual, and disdained to put before him intellectual pabulum which in essence would not be worthy of an adult. How the present generation of children has been able to substitute for this the blatant and inane fare of the funnies on the one hand and the highly artistic picture book without literary content on the other is a profound mystery to me. The children of my day would consider that the children of the present day are letting themselves be shortchanged.

The autumn marked the triumphant return of Admiral Dewey to Boston after the Spanish-American War. My parents took me to see the parade that marked this occasion; but I did not and could not have had a sense of the historic importance of the occasion, for a war meant to me certain military toys that went bang-bang rather than anything that had to do with real people losing real lives.

Another clear recollection of this winter is of Christmas. I woke up in the morning well before dawn to look into my stocking and to discover what Santa Claus had put in it. At this time I did not know that Santa Claus was my father, but I appreciated the sweets and toys and little written jests that I found with the tangerine and the nuts and candy in my stocking. The larger presents were under the tree, and my sister and I should have waited until morning to see them, but we interpreted morning in a very loose sense, and made our way downstairs around four o'clock.

The other pictures I carry of the year are completely isolated. Our next-door neighbor, a militant Irishwoman, the wife of a policeman, valiantly drove off from her fortress some naughty boys who were invading it. I believe she used a broom. I rode up and down the sidewalk on my tricycle, and used to meet on my daily rides boring grown-up friends of my father. There was a one-legged boy in the neighborhood who puzzled me by appearing alternately with and without an artificial leg; he used to pass our house on his way to school on a bicycle.

It is strange how pictures of suffering and mutilation recur among my very early impressions. I doubt whether much of my interest in these matters was humanitarian or proceeded from a genuine compassion for the suffering. Part of my interest was the cruel, staring curiosity of the child, and another part was the genuine fear of catastrophe as something which had occurred to people whom I could see and which might conceivably occur to me. I had had a minor surgical operation for tonsils and adenoids about this time and was terrified by the harsh swirl of confusion that followed the administration of the anesthetic. But I perceived no relation between my own minor surgical operation and my terror at the fact of mutilation. All this is a repetition of an observation amply familiar to the Freudians, and efficiently explained by them.

My father was very proud of his early record as a farmer and had long aspired to be a landed proprietor. In this there was a mixture of Tolstoyanism and a pride in having overcome one of the traditional limitations of the Jew. In the spring of 1900 he bought the farm of his dreams in Foxboro. The house was set well back from a road, lined with a row of catalpa trees which gave the place its name: Catalpa Farm.

I do not remember what Father raised on the place, although I cannot imagine him not farming. I am sure that that summer contributed greatly to my knowledge of country life and of the New England trees and plants. The country children on the next farm took the usual advantage of me which they considered themselves entitled to take of a city boy, and filled my mouth with the filth of the road. I found a few more suitable playmates in the village, or at least a few playmates more willing to accept me. They acquainted me with the existence of earthworms, and surprised me by exhibiting the very moderate inconvenience which the worm suffers by being cut in two. The cruelty of this process gave me only some minor twinges of conscience.

I remember very little of Foxboro as it was at that time, although I am sure that the most exciting thing about it for us was the gossip concerning a church which had recently been founded there by the Holy Rollers. I also remember an older boy who took me to a baseball game between Foxboro and Attleboro. The mysteries of baseball were too much for me, and it was only much later that I was able to take an interest in the game.

The early summer was marked by the arrival of my Grandmother Wiener from New York, accompanied by my cousin Olga. I remember Grandmother even then as an old lady, although she cannot have been much older than I am at the present time. She was always dressed in the dark clothes of an elderly European, and she had definitely foreign mannerisms, exhibited in the way she would gesture with her forefinger and would shake her head. She was a tiny, active person, with the air of having suffered much; and from everything that I have heard about my grandfather, he was not a man with whom it was possible to live without suffering, if only because of his temper and improvidence. In Europe Grandmother had been thrown on her own devices for earning her living; and now that my father's example had been followed by the other Wieners and they had come to America too, she was passed on from child to child according to their financial ability to support her.

Grandmother always spoke with a strong accent and was never able to distinguish between the words "kitchen" and "kitten." She read her own newspaper, printed in a foreign type-face which I later found out to be Yiddish. She always signaled her coming to visit us by bringing us dainties and toys from New York, but we would have loved her even without this. My mother, who looked rather askance on my father's New York kinsfolk, from the vantage point of having been in the land for one more generation, could not help loving Grandmother, or *Grossmutter,* as we always used to call her to distinguish her from Grandmother Kahn.

Cousin Olga was a sharp young person of nine years, four years older than I. Her mother, my Aunt Charlotte, had been left alone in the world by a runaway husband, and it was very important for Olga to have a chance for a healthy summer vacation in the country. She and my mother were always at swords' points. The life of the New York back streets tends to make one worldly-wise before one's time and this was hard on my mother.

Olga and I often quarreled. On one occasion, she and I got into a quarrel—I do not know about what. Olga told me that God knew everything, and would not approve of my behavior. I then and there made the statement that I did not believe in God. Seeing no lightning descending from heaven to smite me dead on the spot, I persisted in my godlessness and told my parents about it. I found enough sympathy in my father's attitude to encourage me to persist in this point of view.

I have never made up my childish quarrel with Jehovah, and a skeptic I have remained to the present day, although I look askance at those skeptics who make their skepticism into a positive religion, and are Bezbozhnik in the same spirit in which they might be churchmen.

There were lilac bushes under the catalpa trees, and in them I found a little nest with blue eggs. Olga told me that because I had touched them, the mother bird would leave them alone and never come again, and that the eggs would not hatch or the nestlings would die. For a child of five, that made me as good as a murderer in my own mind, and the sense of guilt that it gave troubled me for a long time afterward.

Father took me on several excursions about the neighborhood, some of them for the sake of the tramp and the chance to engage in his favorite sport of collecting mushrooms, and some of them to contribute to my education. For example, he took me to a foundry and machine shop in the neighborhood. The blast furnace was fed with scrap, not ore; and I saw the metal run into the molds for pigs and the more elaborately shaped molds for parts to be machined. The machine shop worked in brass as well as in steel. It was a delight to see the white and yellow shavings turning up into curls under the pressure of the tool.

Father often tried to secure my education at school, but he found some obstacles in his way, though I don't quite know what the difficulties were. I fancy I was just too young for the standards of the school board. I was vaccinated for the village school, and went to it for a few days, and then Father transferred me to a little red schoolhouse in the country, where children of all ages studied together under one teacher. All that I remember of it is that there was a pond outside the schoolhouse, and that it was winter, and that children were sliding and skating on it.

Some time in the spring of 1901, when I was six years old, we took rooms in a boardinghouse on Concord Avenue opposite the Harvard Observatory in Cambridge. We had returned to a Cambridge boardinghouse because we were contemplating a summer trip to Europe. My parents were busy making the necessary purchases of equipment for the trip, and buying toys and other amusements to occupy my sister and me on the ship. I have not many recollections of this time except that I again visited my playmate Hermann Howard, and that an older girl, Renée Metivier, who was lodging in the same boardinghouse, took me under her wing. She taught me how to make and to fly a kite, and I remember going down with her to Church Street to get the material. Church Street at that time was even more the artisan's section of Cambridge than it is at the present day. While I was in kindergarten the teacher had taken us down there to see the delightful mysteries of its forges and wheelwright shops and carpenters' yards.

There is one point which I would append to the discussion of my early reminiscences. It is probably of considerable interest to the reader to know how the very early intellectual development of the prodigy differs from that of other children. It is, however, impossible for the child, whether he be prodigy or not, to compare the earlier stages of his intellectual development with those of other children until he has reached a level of social consciousness which does not begin until late childhood. To say that one is a prodigy is not a statement which concerns the child in question alone. It is a statement which concerns the relative rate of his intellectual development with that of others. And it is a thing which his parents and teachers can observe far earlier than he can himself. In one's earlier stages of learning, one is one's own norm, and if one is confused, the only possible answer is that of the Indian, "Me not lost, wigwam lost."

I was well along in childhood, probably seven or eight, before I knew enough about the intellectual development of other children to comment in my own mind on their relative speed of learning and my own. By this time the earlier stages of the process of learning how to read and even the simpler aspects of learning my arithmetic had receded into the past almost as thoroughly as the average child's consciousness of learning how to speak. For this reason what I shall have to say about these matters will scarcely be distinguishable from the history of any other child, except on the basis of the precise year and month in my life at which I passed various stages of development.

For mark this well: all early learning is a miracle, even on the part of the child whom we later consider to be somewhat dull. When a child begins to speak, it has already learned its first foreign language. Between birth and the age of two years there is a blossoming out of new intellectual acquisitions which can never be paralleled in later life, and this whether the child is a genius or a moron. This is a development of doing rather than of reflection about doing; a spontaneous burgeoning of new talents and not the work of the child as his own self-conscious schoolmaster. It is a fact that in my case the beginnings of reading go back to an age not twice that of the beginnings of speech with many children, and that this is obscured by the fact that I was learning to read, not learning to think about reading. Later, when I went through my earlier schoolbooks (at home under my parents' guidance), I learned some of the puzzling distinctions between capitals and small letters and script. I have memories only of the obstacles in my way, and not of the greater part of the task, which was accomplished spontaneously and unconsciously. I remember that the similarity between *i* and *j* puzzled me, and that in old-fashioned books there was a long *s* which looked curiously like an *f*. I remember the mechanical difficulty of writing, and that my best handwriting was and long remained below the acceptable standard of the class. As to arithmetic, I counted on my fingers and continued that long after it was regarded as unpermissible by the standards of my school classes. I was puzzled by such things as the axiom that *a* times *b* equals *b* times *a,* and I tried to clear this up by drawing a rectangle of points and turning them through a right angle. I was not particularly fast in learning my multiplication tables or, in fact, anything else that had to be learned by rote, although I had a good understanding of the principles of fairly complicated operations from a very early period in my early childhood. I remember the old Wentworth *Arithmetic,* in which I read ahead into the discussion of fractions and decimals without any great difficulty. In general the two things that held me up were at the opposite ends of the game: the technique of adding and multiplying rapidly and precisely and the understanding of why the various laws of arithmetic, the commutative and the associative and the distributive, were true. On the one hand, my understanding of the subject was too fast for my manipulation, and on the other hand, my demands in the nature of fundamentals went too far for the explanations of a book devoted to manipulation. But if we go beyond that to the very first beginnings of my arithmetic, they are nearly as hard for me to recall as the beginnings of my reading or speaking.

This relegation of the difficult and the truly intellectual part of my work to a level below full consciousness is not merely a matter of my childhood, but something that has continued to the present day. I do not fully know how I get new ideas or how I resolve the apparent contradictions between those already in my mind. I do know that when I think, my ideas are my masters rather than my servants, and that if they resolve themselves at all into a usable and understandable pattern, they do that at a level of consciousness so low that much of it happens in my sleep. I shall have to speak of this elsewhere, but I cannot find in my own intellectual history any brusque change between the striving of childhood after childish knowledge and the power and the striving of my grown life after the new and the unknown. I know more and I have better tools, but it would often be hard for me to say just when and how I have acquired these tools and this new knowledge.

One thing that I share with my father is an excellent memory. By this I do not mean that we cannot and have not been perfect examples of the absent-minded professor, and that our capacity to forget in matters of daily life has not been ample. But I mean that when we have acquired a range of ideas or a way of looking at things, this has become a part of us not to be lost through any vicissitudes whatever. I remember the last stages of my father's life when he was on his death bed with apoplexy, and when his fine intelligence no longer enabled him even to recognize the loved ones about him. And I remember that he spoke as though he had the gift of tongues, in English, in German, in French, in Russian, in Spanish. Confused as to what he saw about him, his languages nevertheless were clear, grammatical, and idiomatic. The pattern went through the fabric, and neither wear nor attrition could efface it.

4 Cambridge to Cambridge, via New York and Vienna

June–September, 1901

G. K. Chesterton has said somewhere that the best way to see London is to make a voyage from London to London around the world. We cannot appreciate an experience until and unless we have other experiences which differ from it to serve us as points of orientation. I am certain that I could never have learned to understand New England if I had not at some period in my life got far enough away from it to see the great lines of its spiritual character spread before me as a map.

In the late spring of 1901, when I was six and a half and my sister was about three, our family took the Fall River boat to New York where we were to stay with my father's relatives. They were living somewhere in the East Sixties between Third and Fourth Avenues. Upper Fourth Avenue had not at that time gained its present prestige as Park Avenue; the region was a slightly superior East Side slum. The typical old-fashioned apartment in which my relatives lived was at the top of a long flight of outside steps leading in from the street. It was dark, overcrowded, and stuffy. The only windows were at the very front and at the very back, and the apartment was already more than full before it had to take the burden of four more visitors from Boston. But we were near enough to Central Park to be able to reach it by a short walk past the palaces then lining Fifth Avenue, and the Central Park Zoo was always a delightful goal for us.

My Uncle Jake Wiener was the only man of the establishment. He was a journeyman job printer and very good at his trade. His main amusement was gymnastics. At one time he had been number three man in the American rating on the parallel bars. Certainly, if ever a man was built for the sport it was Uncle Jake. I have said that my father had powerful shoulders, but his brother Jake's were enormous, and he was muscled like a wrestler. He was even shorter than my father, who was also a short man; and his legs were thin and spindling. He had the drawn-in muscular abdomen of the athlete. His face was twisted to one side by some early injury which had caused the necrosis of one side of his lower jaw. He was very kind to us children, and I remember his showing us a fool's cap with bells which he had worn at some lodge entertainment. He was scarcely thirty at that time, and single, though he later married and had a family.

My aunts had had better cultural opportunities than Uncle Jake. They had retained a good deal of Russian culture, though they later found that French culture had a greater commercial value for them in the garment trades. Aunt Charlotte, the mother of Olga, had a husband who had left her. She was divorced from him and was destined to know a similar misfortune again. Like her sister Augusta, who never married, she was in the needle trades. Both of them were exceptionally intelligent women; and with half an opportunity in life, they would have had careers comparable with that of my father. They spoke several languages fluently; and later on, when they had spent some years in Paris, they both became valuable assistants in the business of a New York couturier, where they passed themselves off as Frenchwomen.

Aunt Charlotte continued to work to a very advanced age and died only relatively recently from an accident. She was a very definitely Jewish-looking type, resembling some of those French Jewish women drawn with such zest by Du Maurier. Aunt Augusta rather resembled her, though she was much the better looking of the two. Like her sister, she also lived to an advanced age. There was an Aunt Adele as well, who later married and was a neighbor of ours for a brief time in the country, and still later moved out to the Pacific coast; but of her I have very little recollection.

There had been another brother, Moritz, older than my father, who had disappeared from the family view for many years. The last place from which he had been heard was Colon, or Aspinwall as it was then called, and the time had been one of a notorious outbreak of yellow fever. My grandmother always spoke of him as if he were still alive and might turn up at any time; but in her heart she knew that he had been long dead. Still, although it is rather late in the day, when my daughters wish to indulge in daydreams of sudden fortune, there is always the remote possibility that a very old gentleman may come to our house from a distant place, say in Australia, where he had made his fortune, and leave it to us in a burst of family sentiment.

As long as my grandmother lived, a visit to New York meant an unending series of courtesy visits to third and fourth cousins and their friends. I now know that this is an established part of the Jewish family structure, but at the time I did not even know that our family was Jewish. Of course, it was necessary for my mother to have some phrase in which to describe those qualities of my father's relatives which she did not want us to imitate; but the words “New York,” spoken with a properly contemptuous intonation, was quite adequate to the purpose.

A child, however, is mostly interested in childish things, and the one of the family who was chiefly interesting to me was Olga. She showed me a few of the city child’s tricks, such as putting pins on the streetcar tracks to be flattened out by the passing cars, and I believe that we used to play cassino together. Uncle Jake would show me card tricks, and how to build little houses out of old packs of cards. There were miniature children’s cards that Olga used to buy in nearby stationery shops; the packs were always incomplete and there wasn’t much to be done with them except to build houses.

The puffy little steam trains that ran over the Third Avenue Elevated delighted me. We used to travel downtown on it to make purchases in the big shops. One of my unpleasant memories is that of chasing around after my mother on these shopping errands, although there was no other way to outfit me for the ocean voyage to come, and many of the purchases were toys for the amusement of my sister Constance and me. I remember a little sailboat which I tried to sail on the lakes of Central Park. Neither my father nor I had the knowledge or experience to enable us to manage it.

Other presents for the voyage were sets of scientific experiments for children, entitled “Fun with Electricity,” “Fun with Magnetism,” and “Fun with Soapbubbles.” I wonder whether the present generation, under the stimulus of Charles Addams, has extended the scope of these sets to cover “Fun with Atomic Physics,” “Fun with Toxicology,” and “Fun with Psychoanalysis.” Whether they have or not, the sets of my childhood were thoroughly delightful, and even today I can remember the details of the experiments they contained.

At length we crossed the harbor by ferry to Hoboken, to embark in a ship of the Holland-America Line. We traveled second class, which represented the cheapest travel decently possible for a family with children in those days, when third class still meant steerage. Even in my early westbound trips after the First World War, I can remember looking down from the second-class deck onto the varied assemblage of steerage passengers, often still in their native costume, who herded together in a discomfort suggestive of the days of the Middle Passage.

A ship is a delightful place for a young child. There were a number of other children on board with whom I could play; and seasickness is chiefly a disease of the adult. I got into the proper amount of mischief, being duly chased out of the ship’s working alleyway. I delighted in looking down from the deck on the ever-changing marbling foam. The ship’s notices in English, German, and Dutch excited my interest, and I already knew enough German—I do not know how I learned it—to make out the similarity between the stateroom notices in German and Dutch. I unquestionably made a nuisance of myself to the passengers in the steamer chairs, as children always do on board ship, but I got properly punished for my sins when one passenger held me under his chair and tickled me past the point of all bearing. Finally, one morning I awoke to find the ship's engines stopped and a view of Rotterdam facing me through the porthole.

We took a compartmented European train to Cologne. I can still picture the railway station, the hotel at which we stayed, and the cathedral. The penny-in-the-slot machines of Germany were much bigger and finer than anything I had known at home, and the burnt almonds they sold were a new treat. Finally we went out to a suburb of Cologne where a cousin of my father lived.

I have said that I already had some fragmentary ideas of German before I left for Europe, but I doubt whether they would have added up to enough to allow my father to say that I knew any German at all. My father was a perfectionist in languages, as befitted a man to whom languages came easily, and who had penetrated very deeply into them. His desire for the utmost finish and correctness was not always easy on his students, and was even harder on his family. My mother probably had a better than average ability to speak foreign languages and a fairly good acquaintance with German. Nevertheless, she was tongue-tied before Father's overwhelming proficiency. She admired his skill in languages, and let herself become unnecessarily dependent on it. As for me, it was not until I had left my home and married, and had come under the milder guidance of my wife, that I ever ventured to use a foreign language without a sense of guilt that led me to hesitate and to stammer each word.

To visit Europe with my father was to see it with the eyes of the European. Strictly speaking, I never went through that period of the tourist in which every door and every wall seems a fortress against him. For during this first visit my inadequacies as a stranger in the land were completely dwarfed by the different and greater inadequacies of the child. By the time of my second visit to Europe, when I was a young man, the memory of my first visit, my studies and reading, together with the continued presence of my father, had made Europe nearly as familiar to me as the United States. At no time could I thus contrast an unknown Europe with an America I knew well.

I will not say that I never went through any of the stages of brashness and bluster which belong to the innocent abroad. But the disease was brief, and it was greatly alleviated by my earlier inoculation with Europe. It has always seemed to me that Henry Adams, in his late attack of tourist frustration, was like a man who is first exposed to the mumps in his twenties. Adams remained allergic to modern Europe all his life. As for me, my early visit was perhaps the very best of all possible trainings, for the scientist must be a citizen of the world.

From Cologne we went up the Rhine on a steamer. We left the boat at Mainz, and made our way to Vienna. Vienna was our headquarters for a considerable period, and it is the part on our trip that has left the greatest impression on me. It is the little things that impress a child, and of these it is perhaps the smells which stay longest in one's memory. The smell of the alcohol lamp over which my parents prepared my sister's warm evening meals, the smell of the rich European chocolate with whipped cream, the smell of the hotel and the restaurant and *café*—all these are still sharp in my nostrils. I can remember the vegetarian restaurants at which we ate, which were generally up a flight or two in some obscure part of the city, and the skin on the boiled milk which I could scarcely bear to swallow. In Frankfurt we had tried a glass of *Apfelmost,* which had even worse consequences for me.

It was new for me to see the newspapers mounted on their wooden frames in the cafés. While Father would read his for the news, I would look over an English paper which contained a children's story as a *feuilleton.* My reading was not yet very fluent, and it was not easy for me to reconstruct the story out of the straggling portions which appeared from day to day and on which I could lay my hands, but I have a dim impression that the story was Kipling's "Puck of Pook's Hill." Certainly the dates are about right, since Kipling's story was written for his children when they were a trifle older than I was then, and they must have been born during his stay in Brattleboro slightly before my own birth.

One of my father's purposes in going to Vienna was to see a journalist by the name of Karl Kraus. I do not know what they discussed, though they probably concerned themselves with Jewish matters and quite possibly with the problem of the translation into literary German of the Yiddish poems of Moritz Rosenfeld, the New York garment worker-poet, whom father had "discovered." I remember being taken into Kraus's apartment in an old-fashioned Vienna apartment house, and there I remember what seemed to me a confusion and disorder which I have never seen equaled elsewhere.

Vienna was hot and uncomfortable, and the bedbugs bit us children unmercifully. My parents did not know what had affected us, nor was the prominent dermatologist whom we called in of much use in the matter. He diagnosed our affliction as the itch, whose appellation of the "seven-year itch" did nothing to quiet the alarms of my parents.

When the true nature of the disease became manifest, we got no sympathy from our landlady. She informed us that in Vienna, that old city of stucco and crumbling plaster, no one was immune from them—not even the Emperor in his palace. They may have been old to the Emperor, but they were new to us; and they served a fully adequate notice on us to leave the city for a more salubrious place.

We found lodgings with a cobbler in the little Wienerwald town of Kaltenleutgeben. The house opened directly on the village street with scarcely any pavement in between. Behind it, the hill rose abruptly, and a little flight of steps led to a pleasant garden arbor. As in the case of the farmhouse where we had stayed in Alexandria, my early experience in Kaltenleutgeben was enough to fix the geography of the place sufficiently in mind so that I was able to recognize the house when I visited it more than thirty years later.

There were several boys in the cobbler's family, and they were my playmates. How we communicated with one another is a difficult question to settle now, for they certainly spoke no English, and my parents have assured me that I spoke no German—at least none by the standards of my father. That we did come to an understanding is clear, for we participated in more pranks together than did Max and Moritz, the young rascals whose history has been given us by Wilhelm Busch. When we were not examining the fat slugs and snails which peopled the back garden, we were playing forbidden games with the balls of the bowling alley attached to a neighboring restaurant, or decidedly not benefiting the cobbler's machinery by the liberties we took with it. We were to be found in the not too clean environs of the nearby open-air bath or buying little imitation baby bottles of colored water at the local fair.

Eventually, after a slow trip across Germany and Holland, we reached London. We found a vegetarian hotel in Maida Vale, which consisted of two large houses thrown into one, and behind it was a garden reserved for the abutting householders. I played with some of the children there, who, if I remember right, were the younger brothers and sisters of the famous pianist Mark Hambourg. The Boer War was still being fought at the time; and as my father was politically liberal, I echoed his opinions to my playmates by calling myself a pro-Boer, though I did not have the slightest idea what the conflict was all about. The English children retaliated by piling three-deep on top of me.

Not far from Maida Vale was the house of Israel Zangwill, with whom my father had corresponded about Zionist matters. Zangwill was one of the most eloquent British Zionists. My father foresaw the difficulties which have arisen since then from the superimposition of a Jewish colony upon a Moslem background. He was an assimilationist in a quite genuine sense, for he felt that the future of the Jews in the newer countries lay in their identifying their interests with those of the country, not in opening the wound of a separate new nationalism.

We visited Zangwill at his house near Maida Vale, which had a pretty little garden in front of it. He carried me upstairs on his shoulders. I remember his face: very Jewish, strongly lined, and not handsome but interesting and sensitive. I was to see him again on my next visit to Europe, which did not take place until I was eighteen years old.

He was not the only literary figure whom we visited in England. There was also Kropotkin, the great geographer and a genuine Russian prince of the imperial blood, who had turned anarchist as a young man, had tried to assassinate his cousin the Tsar, and had been obliged to flee the country. He had visited Boston about a year before and had been shown around by my father. He had given me as a present a little cardboard cabinet of minerals. One evening after he had been dined and wined by Mrs. Jack Gardner at her Fenway Palace, he turned up at our house, angry and inarticulate. “Wiener,” he said to my father, “I have been insulted!” When Father had reduced him to comparative calm, the story came out. A Boston society lady had asked him, “Oh, Prince Kropotkin, how is your *dear* cousin the Tsar?”

We visited Kropotkin in his little house at Bromley, Kent. It was a workingman’s home, with the usual depressing similarity to every house up and down the street. The back garden was a pleasant place, however, where his two daughters served us tea.

We saw the usual sights of London, such as the Houses of Parliament and Westminster Abbey. Sometimes we ate in a vegetarian restaurant on Holborn, and sometimes at an A.B.C. tearoom. We traveled mostly on busses, the upper deck delighting me; sometimes we took the new tube railway which in those remote days was known as the “tuppenny tube.” The hansom cab still had no real competitor, and the London streets had what I later learned to recognize as the full, rich flavor of the Sherlock Holmes stories.

We left for Liverpool on the last lap of our journey, and of course the return voyage seemed as dull and uneventful as all such voyages, without the prospect of new adventure abroad to enliven them.

5 In the Sweat of My Brow

Cambridge, September, 1901–September, 1903

On Avon Street in Cambridge we took a moderately old but pleasant house, with an ell behind and a little below the level of the main house. It had ground- and cut-glass front doors, a library and living room in front, and a small but adequate study for my father. The upstairs rooms were large and sunny, and the little upper story of the ell housed our nursery. There was a fairly large back yard for my sister and me to play in.

Some two houses from us lived Professor Bôcher, who, we later learned, was a great mathematician. He was the son of a former French professor of modern languages at Harvard, and I believe he had a family of two children about my own age. On the Easter of 1903, I joined his children to look for the Easter eggs which had been planted for their benefit. A little beyond him, well back from the road, was the house of Professor Otto Folin, the distinguished physiological chemist. Of Swedish peasant origin, he was married to an old-stock Western American woman, one of my mother's closest friends from her Missouri days. I had the run of their house and used to read their books. Both my mother and Mrs. Folin are still alive, and are still close friends.

The geneticist Castle and the physiologist Walter Cannon were two other friends of my father and of them I asked childish questions about science. My father and I went to see Cannon in his laboratory at the Harvard Medical School of that day, which was then behind the Boston Public Library, in a building now used by Boston University. I was particularly interested in the pictures Dr. Cannon showed us of the Canadian backwoodsman, Alexis St. Martin, who had accidentally shot a hole in his stomach, and of the American Army doctor, Beaumont, who had used him as a guinea pig for the study of digestion. Cannon himself told us the fascinating story of this partnership.

I was also interested in Dr. Cannon's X-ray machine, which, if I remember correctly, was excited by some sort of electrostatic generator. Cannon was perhaps the very first man to use the new ray of Röntgen in the study of the softer tissues, such as the heart and the stomach, and thus to continue the early work which St. Martin's ghastly fistula had made possible. He was also a pioneer in the use of lead screens for the protection of the X-ray operator. It was because of this precaution that he seemed for many years to have gone scatheless from these dangerous beams, while the majority of his early colleagues had crumbled to pieces by bits, submitting to amputation after amputation. Yet while he lived well into his seventies, his early X-ray burns killed him in the end.

These men I saw only occasionally. A much commoner visitor to our house was Father's friend, the Assyriologist Muss-Arnoldt. Muss-Arnoldt was, I believe, an Austrian Jew, and he had almost exactly the face and expression of his own Assyrian winged bulls. He was black-bearded and rather burly, a great scholar, and a man with an irascible disposition. He taught me occasionally when he was staying at our house, and my father was otherwise occupied; and he was a strict but unskillful disciplinarian. One day a few years later, after a Latin lesson which particularly rankled in me, I was watering the lawn, and obeying a sudden and irrational impulse, I turned the hose on him. I was duly punished by my parents and Muss-Arnoldt looked askance at me ever after.

To a person who has seen the intervening stages of its development and decadence, it is difficult to compare the American Cambridge of today with the Cambridge of the beginning of the century. It is only by imperceptible steps that the houses have become grimier, that the traffic has become heavier, that the vacant lots have vanished, and that a community which in 1900 preserved much of the atmosphere of the country town has grown into a great, dirty, commercial city.

When I was a child, there were those who still spoke of Massachusetts Avenue by its old name, North Avenue; and it was lined by the inartistic but attractive and comfortable mansions of well-to-do businessmen. They are still standing, but fallen from glory. Their porte-cocheres shelter no coaches, and the elaborate wood carving of their porches is rotting away. They were inhabited by families with four or five children, and were ruled from the kitchen by a competent and masterful servant girl. The children had ample yards to play in, and the trees which shaded them had not yet been reduced to sickly pallor by the smoke of the East Cambridge factories.

The vacant lots of Cambridge bloomed with dandelions in the spring, buttercups in the summer, and the bluish blossom of chicory in the fall. The streets were, for the most part, unpaved; and when it rained, they were deeply rutted by the wheels of the horse-drawn delivery trucks. In the season of snow, the wagons were replaced by sleighs and sledges, and it was a favorite pastime of the youngsters to tie their sleds behind the delivery sleighs then known as pungs. On the hilly streets there was coasting, not only on the small sleds which one rode belly-bump but on large double-runners made up of two such sleds, a plank, and a steering wheel. There was an abundance of frozen puddles on which one could skate, and it was always possible to go to Jarvis Field and watch the Harvard hockey team at practice.

As I have said, my father was an enthusiastic amateur mycologist; and under his guidance, I toured vacant lots in search of morels in the spring and field agarics in the fall. The morels were confined to a few well-known spots, and the Harvard mycologists considered that they had duly staked their claims on these spots. It was a frequent cause of bad feeling when one of them stole a march on a colleague and reaped the little clump that the latter had considered his private property. Stands of field agarics were less subject to this test of ownership, and coprinus was too common to be considered a property at all.

These additions to our kitchen were supplemented by an occasional lepiota or a batch of elm mushroom. Every now and again we would find a clump of clavaria or hydnum, and even a few rarer delicacies; but these were mostly reserved for our summer vacations. Part of the fun was the fact that one might just possibly confuse these edible fungi with an amanita, or at least an emetic russula; and the knowledge that one would have to wait some twelve hours before the symptoms became obvious was a source of more than one sleepless night to my parents and to myself.

I have botanical memories beyond these stray fruits of the field. I can never forget the little maple keys taking root in the soil, nor the tiny trees which started from them. The smell of fresh earth, of maple bark, of the gum of cherry trees, and of newly mowed grass all belonged to my youth, with the drawl of the lawn mower and the pattering of water from the spray which kept our grass green. In the fall it was always delightful to trudge through the crisp heaps of fallen leaves in the gutter or to smell their aromatic smoke as they burned. In my childhood recollection, these are supplemented by the resinous perfume of freshly cut pinewood and the various builders' smells of linseed oil and new cement.

The whole frame of our lives has changed between that day and this. Wood then was so cheap that we used to knock up for firewood the boxes in which our groceries were delivered, and our butter came in wooden tubs or in neatly dovetailed wooden boxes with sliding lids. The chief token of those ampler days was, however, the ease with which one could secure servant girls. Mother never had less than two, a cook and a children's maid, together with the services of a laundress, and yet my father was only an impecunious instructor or assistant professor, with no promise of tenure for some time to come. For a large part of our time on Avon Street I nearly worshipped our maid, Hildreth Maloney, an intelligent, loyal and competent young woman, who was later to improve her position in the world. I do not remember our cook, but our laundress was a faithful and hard-working woman by the name of Maggy, to which we added the soubriquet, "The Button-breaker."

I was brought up in a house of learning. My father was the author of several books, and ever since I can remember, the sound of the typewriter and the smell of the paste pot have been familiar to me. But it was not the efforts of the literary scholar that first seized my imagination. By now I could read freely. I had full liberty to roam in what was the very catholic and miscellaneous library of my father. At one period or other the scientific interests of my father had covered most of the imaginable subjects of study. Somewhere in our bookcases there was a Chinese dictionary, there were grammars of unusual and exotic languages, there were charlatanlike books on the occult, there were accounts of the excavations of Troy and Tiryns, and there were a series of the English scientific primers of late Victorian times. Above all, there was a compilation of papers on psychiatry, electrical experiments, and travels of naturalists in the wilder parts of the world, that went by the name of the Humboldt Library. There were two odd volumes of the excellent *Natural History* of Kingsley, together with the far less scholarly and more anecdotic book of Wood which Mr. Hall had given me years before.

I was an omnivorous reader, and by the time I was eight I had overstrained a pair of rather inefficient eyes in consuming whatever books came my way. The learned works of my father's library shared my attention with the books of Dickens which my mother read to me, Stevenson's *Treasure Island, The Arabian Nights,* and the writings of Mayne Reid. To me they were all books of high adventure; yet the tale of Long John Silver and the stories in the *St. Nicholas Magazine* were pale to me beside the true accounts of the adventures of those naturalists who had found new beasts and birds and plants in the somber darkness of the rain forest, and had heard the raucous calls of the macaws and the parakeets.

Thus I longed to be a naturalist as other boys long to be policemen and locomotive engineers. I was only dimly aware of the way in which the age of the great naturalists and explorers was running out, leaving mere tasks of gleaning to the next generation. Yet even if I had been fully aware of this, my allegiance in science was already mixed. My father had brought me from the Harvard library a book devoted to the various branches of the study of light and electricity, which included a stillborn theory of television, frustrated by the inadequacies of the selenium cell. This book had attracted my fancy. I followed it up by further reading in physics and chemistry. When I was about seven years old, Father recognized this interest by inviting a chemical student, who had shown an interest in Russian and had attended his classes, to set up a little laboratory in the nursery and to show me some simple experiments.

Of course, I was particularly interested in the smellier of the experiments, and learned the trick of making a sulphide by heating scraps of metal with sulfur, and then of generating hydrogen sulphide by exposing this sulphide to the action of an acid such as vinegar. Mr. Wyman, my instructor, continued to teach me over the period of a few months later when I was forbidden to read because of my rapidly advancing myopia. Not long after this, I heard of his early death in an automobile accident not far from where my M.I.T. office is today. I believe that this was one of the earliest automobile fatalities in Cambridge.

Even in zoology and botany, it was the diagrams of complicated structure and the problems of growth and organization which excited my interest fully as much as the tales of adventure and discovery. Once I had been sensitized to an interest in the scientific—and various toys of scientific content played almost as great a role in this as my reading—I became aware of stimulating material all about me. I used to haunt the Agassiz Museum until there was more than one exhibit which I knew almost by heart. I read one scientific article that has had a direct influence on my present work, but I am unable to recollect where I saw it. It is confused in my memory with an article by Dan Beard which appeared in the *St. Nicholas Magazine* and was called "The Jointed Stick." It contained some material on the analogies and homologies of the skeleton of the vertebrates. The deeper article which my memory has long confused with this must have been written by some professional physiologist. It contained a very sound account of the progress of a nerve impulse along a nerve fiber, as a consecutive process of breakdown, analogous to the consecutive fall of a train of blocks rather than to a continuous electrical phenomenon. I remember that the article excited in me the desire to devise quasi-living automata, and that the notions I acquired from it survived in my mind for many years until they were supplemented in my adult life by a more formal study of modern neurophysiology.

Behind these books, which I read freely, there were a number that caused me a very real pain, yet a pain in whose titillations I was ashamed to observe elements of pleasure. No one had forbidden them to me, but I had forbidden them to myself, and yet when I turned past the fearful pages I could not refrain from giving them a stray glance. Much of *Struwwelpeter* came under this heading and a good deal of *Max und Moritz*. In *The Arabian Nights* there is a terrible "Tale of the Greek Physician," and there is Grimm's fairy tale of "The Boy Who Did Not Know Fear." There were certain parts of the scientific books to which I had access that excited this baser mixture of emotions, and I remember in particular terrifying but fascinating passages in the Humboldt Library which were devoted to an account of execution by electricity and of fashion in deformity. I had an early interest in medical books which was partly legitimate and scientific but which also contained not a little element of "looking bogy in the face." I was quite aware of the mixture of emotions with which I read these, and I was not able for any length of time to pretend that my interest was altogether innocent. These books aroused or recalled emotions of pain and horror, yet showed these emotions to be related to those of pleasure. I knew this then, long before Freud's work had come to my attention and helped me to understand these tangled emotions.

Probably much of my early reading was over my head at the time. It is not essential for the value of education that every idea be understood at the time of its accession. Any person with a genuine intellectual interest and a wealth of intellectual content acquires much that he only gradually comes to understand fully in the light of its correlation with other related ideas. The person who must have the explicit connection of his ideas fed to him by his teacher is lacking in the most vital characteristic that belongs to the scholar. Scholarship is a progressive process, and it is the art of so connecting and recombining individual items of learning by the forces of one's whole character and experience that nothing is left in isolation, and each idea becomes a commentary on many others.

This unusual reading history of mine made me difficult to place in school. At seven, my reading was far in advance of my handwriting, which was awkward and ugly. My arithmetic was adequate but unorthodox, in that I preferred to use such shortcuts as to add nine by adding ten and subtracting one. I still was inclined to do sums on my fingers, and was not yet very sure of the later parts of my multiplication tables. I had the beginnings of a familiarity with German, and I devoured every scientific book on which I could lay my hands.

After a certain amount of looking around, it was decided to put me in the third grade of the Peabody School on Avon Street. The teacher was kind and intelligent, as well as very tolerant of my infantile maladroitness. I do not know how long it was before my parents and my teachers came to the conclusion that I should be shifted to the fourth grade. I do not believe that they waited all year to make this decision. I still could scarcely have been much more than seven years old at the time. At any rate, the fourth grade teacher was less sympathetic with my shortcomings, and in one way or another I did not click.

My chief deficiency was in arithmetic. Here my understanding was far beyond my manipulation, which was definitely poor. My father saw quite correctly that one of my chief difficulties was that manipulative drill bored me. He decided to take me out of school and to put me on algebra instead of arithmetic, with the purpose of offering a greater challenge and stimulus to my imagination. From this time until I went to the Ayer High School at the age of nearly ten and even later, all my teaching was in my father's hands, whether directly or indirectly.

I do not think that his original purpose had been to push me. However, he had himself started his intellectual career very young, and I think that he was a little surprised by his own success with me. What had started as a makeshift was thus continued into a definite plan of education. In this plan, mathematics and languages (especially Latin and German) were central.

Algebra was never hard for me, although my father's way of teaching it was scarcely conducive to peace of mind. Every mistake had to be corrected as it was made. He would begin the discussion in an easy, conversational tone. This lasted exactly until I made the first mathematical mistake. Then the gentle and loving father was replaced by the avenger of the blood. The first warning he gave me of my unconscious delinquency was a very sharp and aspirated "What!" and if I did not follow this by coming to heel at once, he would admonish me, "Now do this again!" By this time I was weeping and terrified. Almost inevitably I persisted in sin, or what was worse, corrected an admissible statement into a blunder. Then the last shreds of my father's temper were torn, and he addressed me in a phraseology which seemed to me even more violent than it was because I was not aware that it was a free translation from the German. *Rindvieh* is not exactly a complimentary word, but it is certainly less severe than "brute"; and *Esel* has been used by so many generations of German schoolteachers that it has almost become a term of endearment. This cannot be said of the English word "Ass!" or of its equivalents, "Fool! Donkey!"

I became accustomed to these scoldings quite rapidly; and in view of the fact that my lessons never lasted many hours, they were emotional hurdles which I could take in my stride. However, they never ceased to be genuine hurdles. The schoolmaster everywhere can summon to his aid the absurdity of his pupil. The very tone of my father's voice was calculated to bring me to a high pitch of emotion, and when this was combined with irony and sarcasm, it became a knout with many lashes. My lessons often ended in a family scene. Father was raging, I was weeping, and my mother did her best to defend me, although hers was a losing battle. She suggested at times that the noise was disturbing the neighbors and that they had come to the door to complain, and this may have put a measure of restraint on my father without comforting me in the least. There were times for many years when I was afraid that the unity of the family might not be able to stand these stresses, and it is just in this unity that all of a child's security lies.

But much more serious for me were the secondary consequences of my father's discipline. I used to hear my juvenile ineptitudes repeated at the dinner table and before company until I was morally raw all over. On top of this, I was made well aware of the shortcomings of my father's father, and it was borne in upon me that his worst traits were latent in my makeup, and only waiting for a few years to be brought out.

When I now read John Stuart Mill's account of his father, it seems on the surface to have represented a completely virtuous relationship on both sides. I know better, and when I read his few words about his father's irascibility I know just how to interpret these statements. I am certain that even if that irascibility had been more decorous than that of my father, it had probably been no less unremitting. There is passage after passage in Mill which could well be the statement by a proper Victorian of a course of training which had been very close to that I had experienced.

My own education had both remarkable similarities to that of Mill and important differences from it. Mill's education was predominatingly classical at a time when there was no other basis for a sound training. Hence Mill covered a wider range of the classics than I did, and at an earlier age; but he began mathematics rather later, and his father was a less authoritative preceptor in these matters. My father had shown from his youth a rather outstanding mathematical ability, which he imparted to me from my seventh year on. Moreover, by the time I was seven, my own reading had penetrated into branches of biology and physics which were even beyond my father's own scope, and which must have gone far beyond the rather pedantically classificatory natural history available to the boy Mill during his tramping excursions.

In one respect my father resembled James Mill: both were ardent walkers and loved the countryside. I gather, however, that the elder Mill did not have the green thumb of which my father was so proud, and that the boy was not under the same pressure to work in the garden and in the field. With Mill as with myself, walks with our fathers seemed to be a fruitful source not only of outdoor pleasure but of the moral stimulus derived from contact with men of learning and of character.

Both the Mills seem to have centered their lives around questions of ethics. They were of a Scottish family, and it is every Scotsman's birthright to be a philosopher and a moralist. It is likewise the birthright of every Jew. And yet the more impulsive character of the Mediterranean gives to his philosophizings and moralizings a different appearance from that which belongs to the man of the north.

The Mills rank as two of the great humanitarians of history. My father's career shows an almost equal depth of humanitarian motivation. Yet the roots of his humanitarianism were different from the Mills', as different as Jeremy Bentham and Leo Tolstoy. The Mills' passion for mankind was an intellectual passion, full of nobility and righteousness, but perhaps rather arid in its lack of an emotional participation with the oppressed. The roots of my father were in the deep human sympathy of Tolstoy, which itself has much of the compassion and self-abnegation of the Hindu Holy Man. In short, the Mills were classicists partaking of the sympathies of a romantic period, whereas my father, although educated in the classical tradition, was a romanticist of the romanticists.

I cannot imagine my father or myself being greatly moved as the Mills were by the icy glitter of Pope's translation of Homer. The poetry that most moved my father, as it has most moved me, was that of Heine, with its aspiration for the beautiful and the bitter revulsion which comes as the poet sees far too clearly the horrible contrast of that which is with that which he would like to believe. I cannot imagine Mill regarding Heine as more than an impertinent upstart, although there well may be hidden references to Heine in Mill's books which give me the lie.

In the details of Mill's experience and of my own as well as in their larger lines, there is much that is parallel. It is clear that both of our teachers wished to prevent us from taking ourselves too seriously by a policy of enforced modesty, which at times amounted to systematic belittling. It is clear that both children combined a profound respect for their fathers with a certain degree of an inner feeling of deprivation and resentment. Yet the conflict of son and father has come to show itself in very different ways. There seems to have been in both Mills an aversion to any display of emotion, which was certainly not present in my father. Yet it is quite clear from Mill's account of his training that strong emotions were there and that they were not weakened in any way by the impassive façade which both father and son maintained.

I doubt whether the older Mill had any of the possibilities of explosiveness and anger that certainly existed in my father, and I equally doubt that he showed the human weaknesses and longings which at times almost reversed the roles of father and son in my family, and made me love my father the more deeply because he never wholly ceased to be a child. In Mill's book it always seems that the awareness of his ambivalence toward his own education is pruned like the trees of an eighteenth-century garden.

That we may readily be aware of the suppressed conflict between John Stuart Mill and his father we owe in part to Samuel Butler. Samuel Butler was perhaps not a prodigy in the full sense of the word, but like many of the infant prodigies, he had been brought up under the intimate supervision of a dominant father, and like many of the infant prodigies including myself, this supervision had led to a certain degree of revolt in his reminiscent attitudes. Indeed, I feel that Samuel Butler as the Ernest Pontifex of *The Way of All Flesh* suffered from a parental tutelage at least as strict as my own, and at the hands of a man infinitely more commonplace and less sympathetic than my father. His ambivalence toward his own father had much more in it of hatred than of love, and what respect it contained was more a respect for strength of character than for good will. I cannot deny that in my own attitude to my father there were hostile elements. There were elements of self-defense and even fear. But I always recognized his exceeding ability in intellectual matters and his fundamental honesty and respect for the truth, and these made tolerable the many frequently occurring painful situations which must have been absolutely intolerable to the son of a Rev. Mr. Pontifex.

As far as the impact of the outer world is concerned, the conventionality of the parental Pontifex certainly offered Ernest a most intense situation of conflict, but it spared him from something of the potential disapproval of the world about him as a breakwater spares the ships of the harbor. The Rev. Mr. Pontifex was unconventional in nothing but the massiveness and whole-souledness of his conventionality. For myself, with all the understanding of my father, I had to pay the double penalty of being the unconventional child of an unconventional man. Thus I was isolated from my environment by two separate isolations.

Religious problems seem to have dominated Samuel Butler and also John Stuart Mill in the relations with their fathers. These problems were even more acute in the youth of Edmund Gosse, another writer who must be mentioned in the discussion of father-son relations. Gosse's book *Father and Son* is like Butler's in being the account of the relations of a boy with a desire for independence to a very dominant father with theological interests. Indeed, Mill's book, for all of the want of a formal theology on the part of both father and son, has a strongly ethical tone which echoes similar preoccupation. In my own case, while my father was a man of strong moral sense, it cannot be said that he had any great interest in theology. The source of his humanitarianism was Tolstoy, and even though Tolstoy embellishes his propagandist texts with many quotations from the Bible, he is at home with that side of Christianity which preaches humility and charity and extols the virtue of the oppressed and undervalued. I have already said that I had begun to express doubts of religion at the early age of five, in terms that would have brought me severe castigation and even more severe chiding at the hands of the elder Butler or the elder Gosse.

Let me return to the details of my own history. I certainly do not remember any effective opposition on the part of my father. Indeed, I strongly suspect that my infantile adventures in agnosticism and atheism were scarcely more than a reflection of my father's own attitude which may have reflected the attitude of my scapegrace grandfather, who had already left the fold of Judaism without embracing any equivalent religion. Even a skeptic like James Mill would have found my levity intolerable. My own career as an infant prodigy thus differs from that of these victims or beneficiaries of dominant fathers in that it was entirely on a secular plane.

It is clear that religion or the equivalent moral questions were what made the mid-Victorian tick. With my father as with me, the predominating motive was that of a profound intellectual curiosity. He was a philologist; and for him, philology was more nearly an exploratory tool for the historian than a declaration of learning, or the means of taking to one's own soul the great writers of the past. Although there was always a strong moral implication in my father's personality and in the course of life toward which he directed me, my interest in science started with a devotion rather to the service of truth than to the service of humanity. Such interests in the humanitarian duties of the scientist as I now have are due more to the direct impact of the moral problems besetting the research man of the present day than to any original conviction that the scientist is primarily a philanthropist.

The service of truth, though not primarily a task of ethics, is one which both my father and I conceived to impose upon us the greatest moral obligation possible. In a later interview which my father gave to H. A. Bruce, he stated this in his own words.[1] The legend that Galileo after his conviction was heard to say, *"Eppur'si muove!"* ("But it *does* move!"), while apocryphal, is true in essence as depicting the code of the scientist. My father felt the demand of intellectual honesty to be one which the scholar can as little repudiate on the basis of any personal danger into which it might lead him as the soldier can repudiate the duty to fight at the front or the doctor to stay and be effective in a plague-stricken city. Nevertheless, it was an obligation which both of us conceived to belong to a man, not merely as a human being, but precisely because he had chosen himself for the specific devotion of being a servant of the truth.

I have said that my father was a romanticist rather than a Victorian classicist. His closest spiritual kin, besides Tolstoy and Dostoevsky, were the German Liberals of 1848. His righteousness partook of the element of *élan,* of triumph, of glorious and effective effort, of drinking deep of life and the emotions thereof. For me, a boy just starting life, this made him in many ways a noble and uplifting figure, a poet at heart, amid the frigid and repressed figures of an uninspiring and decadent Boston. It was because of this, because my taskmaster was at the same time my hero, that I was not bent down into mere sullen ineffectiveness by the arduous course of discipline through which I went.

My father not only taught me directly but also had a Radcliffe pupil of his, Miss Helen Robertson, come several times a week to review my Latin with me by ear and to help with my German. It was a delight to have her come and to have opened to me another contact with the world of grown-ups besides that of my family. I learned from her the legends of Harvard and of Radcliffe; of the acerbity of this professor, of the wit of that; of the old peddler known as John the Orangeman and the cart which the Harvard students had given him with the donkey bearing the name of Ann Radcliffe; and of the wonderful blind-and-deaf student, Helen Keller. I learned of the visit of Prince Henry of Prussia and of the student pranks on that occasion. In short, even at the age of eight, I had a foretaste of the life of a college student.

It was about this time that I began to discover that I was clumsier than the run of children about me. Some of this clumsiness was genuinely poor muscular co-ordination, but more of it was based on my defective eyesight. I thought that I could not catch a ball, when the really fundamental fact was that I could not see it. Undoubtedly all this was accentuated by the early age at which I learned to read and by my immoderate indulgence in that pastime.

My appearance of clumsiness was accentuated by the learned vocabulary which I had acquired from my reading. While it was entirely natural and not in any degree an affectation, it emphasized to my elders and particularly to those who did not know me very well that I was in some sense a misfit. As I shall point out in the next chapter, I had a fairly normal acquaintance with other boys of my age, so that I do not believe this anomaly in fact excited as much attention among my contemporaries as it did among my seniors. If my contemporaries received any particular impression from my adult vocabulary, I am inclined to believe it was only a secondary impression conveyed to them by their parents.

During the year when I was eight, my eyes began to trouble me in a rather alarming way. Of course my parents noticed this long before I did. A child is not aware of a constant deficiency of sense such as of eyesight. He accepts his own vision as the norm of vision, and if there are any defects, he assumes that they are common to the human race. Thus while a rapid aggravation of eye trouble is noticeable, a steady level of visual deficiency calls no attention to itself, especially when, as is the case with the myope, the difficulty does not interfere with reading. The myope tends to hold the book too close to his eyes, and this is conspicuous to his more sophisticated parents. But it is not conspicuous to himself until it has been pointed out, and until he has been given the advantage of adequate glasses.

My parents took me to Dr. Haskell, our oculist, who gave strict orders that I was not to read for a period of six months, and that at the end of this period the entire question of my reading was to be reconsidered. Father went ahead teaching me mathematics, both algebra and geometry, by ear, and my chemistry lessons went on. This period of ear training rather than eye training was probably one of the most valuable disciplines through which I have ever gone, for it forced me to be able to do my mathematics in my head and to think of languages as they are spoken rather than as mere exercises in writing. Many years later my training proved of great service to me when I came to learn Chinese, which a complicated notation has rendered far more difficult to the eye than it is to the ear. I don't suppose that this early training created the very good memory which I have carried with me down to the present day, but it certainly showed me that I had such a memory and made it possible for me to exploit it.

At the end of six months, my myopic eyes showed no further alarming symptoms, and I was allowed to read once more. The doctor's judgment in permitting me to go back to my work has been justified by the last fifty years of my life, for in spite of increasing nearsightedness, cataracts, and the removal of both lenses, I still have very fair vision, and I see no prospect that my eyes will let me down as long as I live.

There is one particular passage in Mill's *Autobiography* which excites a certain resonance in my own experience. Mill speaks of passing on his instruction to his younger brothers and sisters. My sister Constance tells me that she suffered much from my juvenile didacticism. I certainly was not made the official pupil-teacher in my family as Mill was. Yet the entire example of a life in which the person one most respects always appears as a teacher can only make the child think of maturity and responsibility as the maturity and responsibility of the school master. It is inevitable that all concentrated teaching teaches the boy to be a teacher. This may be overcome later, but it represents a trend that must always be there.

During the next years, without excessive difficulty but with a severely lacerated self-esteem, I labored under my father's tutelage through the Wentworth textbooks on algebra, plane geometry, trigonometry, and analytical geometry, and learned the rudiments of Latin and German. I recognized that my father spoke with the authority of the scholar, even as I recognized that most of my outside teachers had spoken with something less.

Note

1. *The American Magazine*, July, 1911.

6 Diversions of a *Wunderkind*

The last chapter was devoted to my work in the early days of my career as a *Wunderkind.* However, my life was one of play as well as work. My parents entered me as a member of a playground which had been set up in a vacant lot next to the Peabody School. We had to show a card to get in and to allow us to use the services of the playground teacher as well as to crawl through the jungle gym and to coast down the slide or to employ such other devices as were there for our use and our exercise. I spent much time there talking with the policeman on the beat. Patrolman Murray lived opposite us and he loved to tease me with tall tales of police service.

I had many playmates whom I found at the Peabody School and retained even after my father had taken over my education. There was Ray Rockwood, who later went to West Point and died many years ago as an officer in the service. He was endowed with two aunts, whose efforts mutually canceled each other. One was a Christian Scientist, and the other manufactured some sort of proprietary medicine.

Walter Munroe was a son of a starter of the Boston Elevated Railway, and Winn Willard was a carpenter's son. Another of my playmates was the son of a man who later became mayor of Cambridge. The King boys, the sons of a Harvard instructor, were mechanically gifted and owned a little working steam engine which was my envy. *The Youth's Companion,* for which my parents then subscribed on my behalf, offered such engines among the premiums in their subscription contests, but even without competing, it was possible to buy the premiums through their services at a reduced rate. My parents bought many toys for me in that way, but they never went quite so far as the steam engine.

In those days the papers were full of what was then an unfailing source of news: the persecution of the Armenians by the Turks. How we came to the conclusion that it was any of our business, I do not know, for we certainly knew little about Turkey and less about Armenians. One day the

King boys and I decided to run away to the wars and fight on behalf of the oppressed. How my father got on our trail I do not know, but in about a half hour he found three very confused little boys gazing into a shop window on Massachusetts Avenue half way between Harvard Square and Central Square. He delivered the King boys to the mercy of their own family. To me he administered no punishment except that of a biting ridicule. It was years before my parents stopped teasing me about this occurrence, and even to the present day the memory of this teasing can hurt.

Most of the survivors among my childhood playmates have made good in the world. One of them, who was notorious among us all as a particularly nasty and vicious child, is now a great tycoon of industry. Another, who distinguished himself by chasing a comrade through the streets with a hatchet, has disappointed all of us by eschewing the life of violence for the scarcely more satisfactory career of a petty swindler.

We had all sorts of fights in those days, from snowball fights to a serious gang affair in which two armies of boys met on Avon Hill Street and pelted one another with stones. Our parents soon broke this up. In one snowball fight a companion of mine, who suffered from a high degree of nearsightedness, incurred a detached retina and lost the sight of one eye.

I have said that I, too, was a myope, and I suppose it was this snowball accident as much as anything else that led my parents to punish me for fighting and otherwise discouraged fighting at all costs. I never would have made a good fighter, as the effect of any severe emotion was to paralyze me with such weakness of fear that I could scarcely utter a word, let alone strike a blow. I suppose the reason was as much physiological as psychological, as I have always gone into fits of weakness when my blood sugar was low.

I took a sufficient part in the sports of the children of my age. I helped to make snow forts for snowball battles, as well as the snow prisons in which we immured our captives and in which I occasionally got immured myself. I jumped on behind the delivery sleighs or “pungs” which traversed the yellow slush-covered streets of the winter Cambridge of those days. I scaled the back fences with the best of them, and ruined my clothes when I fell off. I tried to skate on a child’s doublerunner skates, but my ankles were weak and lax, and I never graduated to the more efficient single runners. I coasted down Avon Hill Street and would try to persuade my seniors and betters to give me a ride on their swifter double-runner sleds. In the spring I searched the pavements and the yards for little pebbles which I could grind up with spittle to make a crude sort of paint, and I would chalk the pavements to make hopscotch courts on which my comrades and I could play. I walked over to North Cambridge to get comic valentines or Christmas cards from the stationery shops, according to the time of year, as well as cheap candies and the other delightful trifles of extreme youth.

I used to play a great deal with miniature electric motors. At one time I had the vision of making one of these, following the directions in a book which I had received as a Christmas present. However, the book was written from the point of view of the boy who has a small machine shop at his disposal; and even if I had possessed one, I neither then nor later would have had the mechanical skill to make use of it.

I remember among my toys a megaphone, a kaleidoscope, and a magic lantern, as well as a series of magnifying glasses and simple microscopes. The magic lantern had a number of comic slides with it, which were quite as gratifying to a small boy of that day as is a Walt Disney movie to his present successor. We used to hold magic lantern shows in the nursery and to take our pay in pins.

There were times when we tried to make a little real money for our undertaking. Father had a series of photographs of Greek art which I understood had been given to me, and I tried to sell them around the neighborhood. I had a pretty task to collect them when my parents found out what I had done.

Christmas of 1901 was hard for me. I was just seven. It was then that I first discovered that Santa Claus was a conventional invention of the grownups. At that time I was already reading scientific books of more than slight difficulty, and it seemed to my parents that a child who was doing this should have no difficulty in discarding what to them was obviously a sentimental fiction. What they did not realize was the fragmentariness of the child's world. The child does not wander far from home, and what may be only a few blocks away is to him an unknown territory in which every fancy is permissible. These fancies often become so strong that even when the child has penetrated beyond the previously unknown boundaries, the conviction of his imagination maintains him in accepting a geography that his experience has already shown him to be false.

What is true concerning the physical map is also true concerning the chart of his ideas. He has not yet had the opportunity to explore very far from the few central notions that are his by experience. In the intermediate regions, anything may be true; and what is for his elders at least an emotional contradiction is for him a blank which may be filled in any one of several ways. For the filling of much of this blank he must depend on the good faith of his parents. Thus the breaking of the Santa Claus myth discloses to him that this dependence on the good faith of his parents has its limitations. He may no longer accept what they have told him, but must measure it by his own imperfect criteria of judgment.

The family was enlarged again in the spring of that year. My sister Bertha was born, and her birth nearly cost my mother's life. Our neighbor, Dr. Taylor, attended my mother. He was a gray-bearded, elderly man, with two sons who were among my playmates. As before, Rose Duffy was the midwife. I was full of fancies about what birth might mean, and had a weird idea that if one could put a doll, say a doll made out of a medicine bottle, through the proper course of incantation, one could make a baby out of it.

This naïveté was remarkable in view of my scientific sophistication at the time. The various biological texts which I read between my sixth and my ninth years contained a great deal of material on the sexual phenomena of animals in general and of vertebrates in particular. I was quite aware of the main outlines of mitosis, of the reduction divisions of the egg and the spermatozoon, and of the fusion of male and female pronuclei. I had a fair idea of the elements of embryology and of the gastrulation of some of the lower invertebrates. I knew that these facts were somehow connected with human reproduction, but my inquiries of my parents in that direction were not encouraged, and I was quite aware that at some place in my line of thought there was a clue missing. Intellectually, I was far advanced in the understanding of the phenomena of sex both in plants and animals. But emotionally the whole matter was as indifferent to me as it can only be to a young child: or rather, where it was not a matter of indifference to me, the only emotions it excited were those of puzzlement and terror.

What made the family situation even worse at Bertha's birth was that both my sister Constance and I came down with measles about the time of mother's delivery. I don't remember how we managed to take care of the three of us at the same time.

It was about this time that my parents tried to see if I could be brought into greater conformity with the habits of the other faculty children. They sent me to a Unitarian Sunday school after a considerable amount of protest which I took out in philosophical debate with the minister after Sunday school. The minister was Dr. Samuel McCord Crothers, that admirable essayist and litterateur, who was a friend of the family for many years and who more than twenty years afterward officiated at the marriages of my sisters. Dr. Crothers was not shocked by my youthful rejection of religion, and tried to meet my arguments seriously. At any rate, through his forbearance it was not absolutely impossible for me to continue in Sunday school.

The Sunday school had a good library, and there were two books which I remember impressed me particularly. One was Ruskin's *King of the Golden River.* Many years afterward, when I read his *Modern Painters,* I recognized the same sense for mountain scenery and the same strong ethical attitude which I already knew in his story for children. The other book was an English version of a French story of the seventies entitled *The Adventures of a Young Naturalist in Mexico*. It is only within the current year that I have seen this book again and I have renewed my impression of the rich picture it gives of the lushness of the tropical forests of the Mexican lowlands.

The Sunday school gave a Christmas play in which I was due to appear somehow or other in a minor part. The making up and dressing up embarrassed me exceedingly, and created a disgust for participating in amateur dramatics which has lasted to this day.

That summer, which we spent in a cottage in Foxboro, *Cosmopolitan Magazine* published the serial story by H. G. Wells entitled, "The First Men in the Moon." My cousin Olga and I devoured it, and although I was not able to appreciate all the social significance of the writing, I was properly shocked and terrified by the brittle figure of the Grand Lunar. About the same time, I had been reading Jules Verne's *The Mysterious Island.* These were the two books which introduced me to science fiction. Indeed, for many years I remained an *aficionado* of Jules Verne, and a trip to the library to find yet another volume of his writing was probably a greater delight than this generation of children can get out of the movies.

Parenthetically, for all this I am not enthusiastic over modern science fiction. Science fiction has been rapidly formalized and it is no longer a genre which offers sufficient freedom for the author who tries to follow its accepted canons. I have tried a little fictional writing about scientific matters, but it is entirely outside the frame of the science fiction monopoly. Some writers in this field have let their taste for fiction outstrip their sense for fact, and have allowed themselves to be used as promoters for various schemes of charlatans. The very originality of science fiction has become a cliché. Its slickness is quite different from the enthusiasm and verve with which Jules Verne adapted the romantic milieu of Dumas, or the sincerity by which H. G. Wells made his sociological discourses palatable and fascinating.

Whether it was summer or winter, Father did a considerable amount of literary work, and I always found it very exciting to follow the successive stages of publication. The first book of his own, as contrasted with the *Poems* of Moritz Rosenfeld (which he had helped to see through the press), was a *History of Yiddish Literature*. This was a little too early for me to have clear memories, but I well remember his next book, the two-volume *Anthology of Russian Literature,* which he edited and the component parts of which he in large measure translated. This was followed by a great contract with Dana Estes and Sons, by which my father agreed to translate all the works of Tolstoy for the cash sum of ten thousand dollars. This was a rather skimpy reward even at that time, and today it seems a ridiculously small sum to pay for the translation of twenty-four volumes. My father accomplished this task in twenty-four months. In this he was helped by a very competent secretary, Miss Harper, and I believe that she was paid directly by the publishers. Father's relations with his publishers were never very smooth, and I think that he was justified in his general attitude of suspicion.

I soon learned that a manuscript is followed by the long banners of galley proof, and these in turn by the smooth oblongs of page proof and the leaded oblongs of plate proof. I learned the main signs of the proofreader and the general technique of proof correction. I learned that authors' corrections in galley proof are expensive, while they are exorbitant in page proof and practically prohibitive in plate proof. I saw Father cut up two or three Bibles in order to translate the Biblical quotations of Tolstoy, and I used to play with the discarded proof sheets and remains of these Bibles as if I were reading proof myself.

Although I had met my mother's family before we moved to Avon Street, most of my recollections of them belong to that time. My mother's mother and two of her sisters followed her to Boston at some period which I do not remember. My grandmother lived in a Cambridge lodging house on Shephard Street at the time that my sister Bertha was born; and I can still remember a heroically determined effort on her part to bathe me, in which she showed herself quite indifferent to my imminent suffocation and to the action of soap on my eyes.

I do not think that she had any particular quarrel with me, but she certainly did with my parents. I do not know in what manner the latter had offended, although it seems clear that the old quarrel between German Jew and Russian Jew played at least a role. At any rate, my parents accused my mother's family of trying to break up their marriage, and there followed one of those family feuds that are not even terminated by death. Some of the participants in these feuds may die, but the rancor of the survivor lives with their memory.

I met my Grandfather Kahn only once if at all. I know his looks very well from his photograph, which is that of a tall, grave man with a long, gray beard. He was already separated from my grandmother, and lived in some sort of old people's home in Baltimore. I remember that at some later birthday he sent me a gold watch for a present. He died about 1915.

My father and I spent a great deal of time in the spring of 1903 looking for a place to pass the summer. We made a regular circuit of the villages south of Boston, from Dedham to Framingham, and even along the seacoast around Cohasset, but we never found the right place. We asked the advice of all of father's friends who lived in the outer suburbs. Finally, we decided to look a little farther afield in the northwest sector, and hit on a place called Old Mill Farm in the town of Harvard, about halfway between Harvard Village and Ayer Junction. We spent one summer there getting acquainted with the place, and decided that the next summer should see us engaged in modernizing the farmhouse and preparing to lead the simple life of a farmer and a college professor.

I do not know precisely what relation the name of the town of Harvard has to Harvard University, but any connection between the two places themselves is remote. The town of Harvard is distinguished historically for containing the site of the first or second water-driven grist mill built in inland Massachusetts. Though this mill was not on the farm we eventually bought, the old dam still stood near its boundaries, and the pond had been successively enlarged by later and later structures until the dam had come to stand opposite the farmhouse. Hence the place was called Old Mill Farm, and this is the name by which I shall refer to it in the ensuing chapters.

When Father bought Old Mill Farm and decided to live there later all the year round, I think there were several motives at work. One was his love of the country and his desire to work in the soil. Another (which I think must have been much less important) was the pride in the additional status of the landowner. Without any doubt, Father considered it essential for his children to have as much of their bringing up as possible in the country, and I believe he found my schooling problem less unsolvable than it might have proved in the city where the only choice would have been either a rather rigid public school or a rather expensive private school. I don't think Father could have found the country more conducive to his literary and scientific work than the city, and indeed it was pretty obvious to me that he made a considerable sacrifice in commuting as he did between Ayer and Cambridge.

When we first came to Old Mill Farm in the summer of 1903, the farmhouse was a gaunt, unattractive structure, dating from the decade before the Civil War. The house stood gable-end to the road, and was connected with a large barn by the usual sequence of ells and woodsheds. Opposite the house was the pond, which then seemed to me almost a lake but which could scarcely have been more than two hundred feet wide. It had a marshy island in it and a little grove of trees on the right side, in which we found ferns and trilliums in the early summer. On the other side was the dam, from which two streams led across a boggy meadow and under the road to the extreme limits of our farm. Beside one of the streams, and nestling under the dam, was a shed equipped with a turbine; it had been used by a previous owner as a small factory for some product I cannot remember.

The land between the two streams and the road was a tangle delightful to a youngster. There were frogs and turtles in the streams. The little fox terrier who was my personal pet soon learned that I was interested in them, and would retrieve turtles for me between his jaws. The tangle of weeds in the half-marshy triangle of land was rich in flowers interesting to the child, such as touch-me-not, joe-pye weed, turtle mouth, and spiraea. Down from the stone embankment which carried the road hung festoons of the vine of the wild grape. The meadows were full of blue, yellow, and white violets, of wild iris, and of bluets and sweet grass after their season. In a more remote pasture there grew the two gentians, fringed and closed, as well as both the pink and the white spiraea and an occasional bush of rhodora.

All these were delightful to me. Not less so were the willows which lined the pond, together with an old stump grown up with withies, which formed our playhouse. There was a nearby sandpile, where we pitched a tent made of old rugs and piano boxes. By the sandpile were needle-covered banks under a spreading pine tree, and there we could burrow and make little ovens in which we baked potatoes. The sandpile was the washed-out part of an old road which had led past our house before the present one had been located, and down which Lafayette is said to have ridden on his great tour about the United States, when he came back as the guest of the country. A trail from the sandpile led down through a wet wood of alders to the sandy shore of the lake, where my sister and I used to bathe among tadpoles, leeches, and tiny frogs, before we had learned to swim enough to be trusted off a shelving beach. Later on, when we were older, our favorite bathing place was a pool just above the great dam, where the main stream poured over in a waterfall, and I could just stand up on tiptoe with my nose out of the water.

There was a boat on the pond, and we used to row up past the ruins of the seventeenth-century dam well into the inlet of the pond. With its water lilies, yellow and white, its pickerel weed, its bladderwort, and the mysteries of its turtles, its fish, and its other submerged inhabitants, the pond was always a delightful place for us. So was the old henhouse, with its chicken-wire supported on live willow posts which had struck root and had grown into young trees. So was the barn with its hay loft, where one could hide and slide and jump to one's heart's content. So were the neighboring farmhouses, at whose back doors we always stopped for a glass of cold water and a pleasant word with the farmer's wife. We learned to avoid the front door, with the untrodden grass before it; for it led to the forbidden regions of the front parlor, open only for weddings and funerals, with its reed organ, its stiff haircloth furniture, its tinted family photographs, and its whatnot, laden with the particular treasure of the house and with the family album.

A little farther afield—about a mile and a half—lay the Shaker village. This was a particular treasure; a Protestant monastery, where the brothers and sisters of a sect doomed to perpetual celibacy sat on opposite sides of the aisle of their little chapel, dressed in an extreme version of the traditionally austere Quaker costume. I remember venerable Sister Elizabeth, and Sister Anne as well, who retained the worldly coquetry of wearing false hair under her coal-scuttle straw bonnet. One or the other would preside at the little shop in their great empty main building. They sold souvenirs and simples, as well as sugared orange peel and enormous disks of sugar flavored with peppermint and wintergreen. These were ridiculously inexpensive, and were the one sort of sweet which our parents allowed us to eat as far as our appetites might go.

The colony must have been about a century old and had an atmosphere of antiquity and permanence about it which was far more European than American. A celibate sect is always likely to find recruitment very difficult, and even though the Shakers would adopt children in the hope that they would grow up in their austere faith, something usually happened during adolescence or immediately after, and the youngsters almost always abjured the righteous faith of their foster parents to go the way of Satan and the flesh. Thus the great communal workshops and the two-story stone barns stood empty, and the fields went half tilled, while the outlying community houses under their spreading fir trees became orphanages or boardinghouses. The cemetery was a waste of weeds and brambles battening on the bones below, and the landing platforms which Shaker modesty had ordained to be built before each house, in order that the women mounting into the carriages should not display their feet in an unseemly and indecorous manner, were rotting away.

For some years Constance and I had been continually at odds with each other, and this seemed to my inexperienced parents a sign of original sin. I call them inexperienced because they were just beginning to realize the conflicts implicit in the growing up of brother and sister. Now, however, I was eight and Constance was four, and there began to be a possibility of our being comrades. I know that we explored the thirty-acre farm together, and that for the first time I began to see her as an individual.

Although my new country life had its delights, the breaking of my acquaintance with children of my own age was a strong countervailing disadvantage. I found, indeed, a group of children from Ayer and from neighboring farms with whom I could play. But the frequency of my play was cut down by our relative isolation. Indeed, I never again fully caught up with the richness of companionship which seemed to me on retrospect to belong to our years on Avon Street. I know very well that the. disadvantages of being too much alone and isolated were not easy to overcome in view of the financial struggles of the family at that time, but the effects were serious and long-lasting. When I left Cambridge for Harvard, I broke up the acquaintanceships of my early childhood, and although I made new ones in Ayer and later in Medford, I was never again to feel the continuity of so rich an environment of childhood friendships.

7 A Child among Adolescents

Ayer High School, 1903–1906

Father intended to live on the farm in Harvard and commute into Cambridge each day. He was a very busy man, for, as I said, he had undertaken to translate twenty-four volumes of Tolstoy into English in a period of two years. This was a tremendous task to add to his Harvard teaching and the running of the farm. The amount of time he could devote to my education was limited, so he began looking around for a school to which he could send me. Even so, he intended to go over my lessons with me each night. I was by this time too far along in my studies to profit by any ordinary grade school, and the only solution seemed to be to send me to some one of the high schools in the neighborhood, and to let me find my own level. The Ayer high school was willing to try this unorthodox experiment. Ayer suited my father perfectly, as it was the nearest station on the main railroad to Boston and he had to drive there every morning to take the Cambridge train, leaving his horse and carriage in an Ayer livery stable till he should return in the evening.

I entered the Ayer high school in the fall of 1903 at the age of nine as a special student. We left the problem of my eventual classification for the future to decide. It soon became clear that the greater part of my work belonged to the third year of high school, so when the year was over I was transferred to the senior class to be graduated in June, 1906.

The brains and conscience of the school were Miss Laura Leavitt, who has just retired after fifty years of service. She was gentle but firm, and an excellent classicist, with a feeling for Latin which went far beyond the perfunctory requirements of the average high school. I read Caesar and Cicero with her my first year, and Virgil my second. I also studied algebra and geometry, but these courses were largely review for me. I studied English literature and German from teachers who have left no particular impression on me. They were probably young women filling in their time between graduation from college and matrimony.

Although I could recite my lessons as well as most of the older pupils, and although my sight translations from the Latin were reasonably acceptable, I was socially an undeveloped child. I had not attended school since I had gone to the Peabody School in Cambridge at the age of eight, and I had never attended school regularly. Now at the Ayer high school the seats were much too big for me and my adolescent fellow students seemed to me already full adults. I know that Miss Leavitt tried to relieve me from the alarm of being in this unfamiliar place among unfamiliar figures, and on one occasion during my first few months at school, she took me on her lap during a recitation of the class. This kind act did not lead to any outburst of laughter or ridicule by the class, who seemed to consider me as the equivalent of their kid brothers. It was quite natural for a friendly teacher to take such a child on her lap when he visited the high school.

Of course, such treatment was in the long run incompatible with the proper discipline of the school, and before long I had learned the elements of schoolroom behavior. The discrepancy between my age and that of my classmates continued to protect me from their ridicule. I think this would have been less true had I been only four years younger than they rather than seven. They viewed me socially as an eccentric child, not an underage adolescent. Hence, it was fortunate that the school shared a building with what would now be called a junior high school, where I was able to find some playmates among eleven and twelve-year-olds, some of whom were the younger brothers of my classmates.

My training and social contacts at high school were only the obverse of the coin. The reverse was my continuing recitation to my father at home. My routine when I was in high school scarcely differed from that which had obtained when he was my full teacher. Whatever the school subject was, I had to recite it before him. He was busy with his translation of Tolstoy and could scarcely devote full attention to me even during my recitation hours. Thus I would come into the room and sit down before a father dashing off translations on the typewriter—an old Blickensderfer with an interchangeable type wheel that permitted my father to write in many languages—or immersed in the correction of endless ribbons of galley proof. I would recite my lessons to him with scarcely a sign that he was listening to me. And in fact he was listening with only half an ear. But half an ear was fully adequate to catch any mistake of mine, and there were always mistakes. My father had reproved me for these when I was a boy of seven or eight, and going to high school made no difference in the matter whatever. Although any success generally led at most to a perfunctory half-unconscious word of praise such as "All right," or "Very good, you can go and play now," failure was punished, if not by blows, by words that were not very far from blows.

When I was dismissed from my responsibility with my father, I often spent the afternoon with Frank Brown. He was a boy of my age, the son of the local druggist and the nephew of Miss Leavitt, and he became a lifelong friend. We lived only two miles apart, so that it was not difficult for me to play with him after school or to walk out to see him on Saturdays and Sundays. His family looked favorably upon our acquaintanceship, and I have always considered them among my dearest friends.

Frank and I used to punt our boat from the pond up by the side of the old seventeenth-century mill dam into the brook and work it over stones and shallows until we came into a dark tunnel in the bushes, which led in a mile or two to a back road to Harvard Center. We would imagine all sorts of weird possibilities in an old slough in the woods. We poked sticks into it to see the bubbles of marsh gas rise and burst. We captured frogs and tadpoles in the pond and tried to make pets of these unappreciative and refractory animals.

Once I burned the skin off the back of Frank's hand when we tried to make firecrackers out of material snitched from his father's drugstore. On another occasion we loaded up a tire pump with water and lay in wait on the piazza to spray one of the early motorcars of that remote epoch. We assembled old cartons and carriage wheels into a ramshackle toy train and played railway man. And sometimes we went up to the attic, where we spent part of our time reading *Treasure Island* or *Black Beauty* and part of our time assembling bits of electrical apparatus to make an electrical bell. We once assembled something we conceived to be a wireless. We were boys as boys always have been and as boys will always be. Certainly I was neither particularly oppressed nor particularly impressed by my unusual school status at this age.

On one day every two weeks the high school had a debate and an oratorical contest in which the children recited standard passages from a compilation made for the purpose. Toward the middle of the vacation between my two years, I decided to write a philosophical paper which I could use for future occasions of this sort. I recited it the next winter, but not as an active participant in the competition. It was called the *Theory of Ignorance,* and was a philosophical demonstration of the incompleteness of all knowledge. Of course, the paper was unsuited for the purpose and beyond my age. But my father liked it, and as a reward for this paper he took a long trolley ride with me to spend a few days at Greenacre, Maine, near Portsmouth, New Hampshire, among the mists of the Piscataqua River. Greenacre was a colony of

Bahaists, receptive to all forms of oriental religiosity. It represented a trend which now belongs rather to Los Angeles than to New England. I wonder what some of the good New England Bahaists must have thought when they found that Bahaism was a Sufiist variant of Islam.

Old Mill Farm was a real working farm, with cows, horses, and all the rest of it, under the supervision of a hired man and his wife. My personal possessions among the livestock of the farm were a goat and my special crony, my shepherd dog, Rex. Rex stayed with us until 1911 when my parents could no longer tolerate his habit of chasing automobiles, and they decided that he had better be put away. Though it may have been necessary, I could scarcely bring myself to consider it as anything but an act of treason to an old friend. The goat was bought by my parents as a present to me, to pull a little carriage which they had the hired man make. The carriage was good fun as a toy but rather unsatisfactory as a means of transportation. Rex and the goat seemed to have misunderstandings about a good many issues. The goat's horns and hard head were a perfectly adequate match for Rex's teeth.

The hardest time of the year was the late winter and early spring. Country roads were not paved at the time, and the carts and carriages made them into a mass of ruts which froze into brutally impassable ridges. A neighbor about a half mile away used to invite me to come over in this miserable, dreary time and play bezique with him and his wife. I had a great deal of time alone in the house to spend reading in father's library. I was particularly fascinated by Isaac Taylor's book on the alphabet, which I knew nearly from cover to cover.

But summer was different. Besides rowing and swimming in the pond, a little desultory botanizing, and mushroom-hunting trips with my father, I used to play with Homer and Tyler Rogers, two boys about my age who lived on a neighboring farm. We almost blew ourselves up by trying to make an internal combustion engine out of a tin fly spray, and we nearly shocked ourselves to death in amateur radio experiments, using apparatus which my father bought for me and which I was never able to put to any really effective use.

Father encouraged me to raise a garden, although he was not very enthusiastic over my horticultural proficiency. I dragged one load of beans after me in a boy's express wagon and managed to sell it to Mr. Donlan, the Ayer grocer. Donlan, who combined his grocery business with an agency of a steamship company, was a particular crony of my father's and they used to talk Gaelic together. To keep up with Mr. Donlan's Gaelic, Father borrowed a few books of Irish fairy stories from the Harvard library. He used to translate them to me as I lay in bed, and I was astonished at the grotesqueness and the formlessness of the tales, so different from the tales of Grimm to which I had been accustomed.

Late in the winter my father had a visit from Professor Milyukoff, a member of the Russian Duma (or limited parliament), an authority on political institutions and later a cabinet member under the ill-fated Kerensky regime. Milyukoff was a tall, genial, bearded Russian, and since he came about Christmas time, he brought my sister and me tufted children's snowshoes, with which we could traverse the white countryside as with seven-league boots. My parents had already their own snowshoes, and found that the regions that were too swampy to cross at other seasons were now as accessible as the open highway.

Milyukoff was writing a book about American political institutions, and Father guided him about the local points of interest in political and social history. Our neighbor Farmer Brown drove us around in his sleigh to the Shaker villages, to the estate of a neighboring single-taxer, and to the cottage of Fruitlands, where the Alcotts had lived after the breakdown of the Brook Farm project. Father explained everything to Milyukoff—or at least I thought he did, but I could not understand the Russian in which they spoke.

In the spring of 1906 when I was eleven years old, my brother Fritz was born. He was always a frail child, and I shall have more to say later concerning the problems of his development and education. My sister Bertha was seven years younger than I, but still near enough to share with me a considerable period of growth and development. But Fritz came at a time when I was already approaching adolescence; when he himself had come to adolescence I was already a young adult busy with the problems of making a career and of finding my social place in the world of ideas, so we could never be companions.

As I have said, I was put into the senior class at the beginning of my second year at the Ayer high school. I was almost eleven and full of rebellion. I had a wild idea (which never came to any sort of expression, even among my closest companions) of forming some sort of organization among children of my age to resist the authority of their elders. Then I had fits of conscience, and wondered if I had not committed a crime equivalent to treason by even contemplating such a thing. I consoled myself by thinking that even if I had, I was too young to be subject to serious punishment for it.

Toward the end of the high school spring term, I used to eat lunch with some of my schoolmates and teachers in a scrubby second-growth wild cherry grove near the school building. The ground was carpeted with anemones and violets, and there was an occasional lady's-slipper. The warm spring sun, shining through the branches which were as yet only covered with a green fuzz of foliage, summoned to a new life and activity.

There, in my last year of high school and at the age of eleven, I fell in love with a girl who played the piano at our school concerts. She was about fifteen, the freckled daughter of a railroad man. Futile as it was, it was real love, and not the almost sexless affection of undeveloped children. She was developed beyond her years. I was only eleven, but I was not even physically the typical eleven-year-old. My make-up was a mosaic of elements as young as eight and at least as old as fourteen. This calf love was quite as ridiculous to me as it must have seemed to others, and I was ashamed of it. I tried to show off in what was actually the least effective way open to me, and to compose a piece of music for her—I, the least musical of all boys. Like so many of these primitive attempts at composition, it sounded like nothing so much as the black keys of the piano struck in succession.

Of course, nothing could come of this friendship, not even an "affair." Besides the fact that I was a child in age, I was too alarmed at the new and half-understood powers within me to break with my infancy and indulge in unpermitted pleasures. My parents' inquiries of me and of other people showed them that this girl was not leading my body to destruction and my soul to perdition—though there had never been the slightest danger. The experience marked the end of carefree childhood. Little as I wished to grow up, I found myself rushing toward maturity, with its unknown duties and possibilities.

Calf love is the experience of every normal boy. But after a couple of years the boy finds himself associated with girls about his own age with whom he learns to be at home. And by the time he is in college, he is already sufficiently hopeful of eventual success to court girls seriously with a view to marriage not to be too long delayed. My calf love, however, was especially early, and when I entered my twenties I was still not nearly out of the woods, nor was I able to think of marriage.

The end of the school year was occupied with the graduation parties of my classmates who were then seventeen and eighteen years old. Even when I was the nominal host, and the guests arrived at Old Mill Farm in brakes rented from the Shakers, I was rather an outsider at the feast. I sat on one side of the room in the kneehole of the desk and watched the dancing as a ritual in which I had no part.

After the round of festivities and the graduation ceremonies, I passed the summer on Old Mill Farm with my *St. Nicholas Magazine* and my Ayer playmates and occasional visits to the farm of Homer and Tyler Rogers. I had tried several times to get a contribution accepted in the St. Nicholas League, that cradle of young artists, poets, and novelists, but the best I could make was honorable mention, and I rated that only once. I had to content myself with purchasable pleasures. I got hold of a cheap Brownie camera about this time; and I hoped to buy an air rifle, but because my parents frowned on this, the best I could get was a popgun with a cork.

I owe a great deal to my Ayer friends. I was given a chance to go through some of the gawkiest stages of growing up in an atmosphere of sympathy and understanding. In a larger school it might have been much harder to come by this understanding. My individuality and my privacy were respected by my teachers, my playmates, and my older schoolmates. I was treated with particular affection and understanding by Miss Leavitt. I had a chance to see the democracy of my country at its best, in the form in which it is embodied in the small New England town. I was prepared and ripened for the outer world, and for my college experiences.

Since leaving high school, I have visited Ayer several times although with long intervals between visits. I have seen the town burst from its status as a railway junction into that of a military cantonment, and I have seen the greater part of its lines of railway go away. I have seen the Second World War inflate the village once more, and I suppose that I shall see it sink again into relative unimportance. Yet, through all these vicissitudes, there has been a definite progress toward unity and serenity on the part of the families I have known. They are people living in a small town, but they are emphatically not small-town people. They are well read in an age of little reading. And they know the theater well, although the nearest theater is thirty-five miles away. Two generations have risen to maturity since I left the place, growing up in an atmosphere of love and of deference. I have the impression that my friends in this small industrial town represent a sort of stability without snobbishness which is universal rather than provincial, and that the structure of their society compares well with the best that a similar place in Europe would have to offer. When I go back among them it is expected of me, and rightly expected of me, that I revert in some measure to my status as a boy among the elders of the family. And I do so gratefully, with a sense of roots and security which is beyond price for me.

8 College Man in Short Trousers

September, 1906–June, 1909

With my high school days over, my father had decided to send me to Tufts College rather than to risk the strain of the Harvard entrance examinations and the inordinate publicity which could have been caused by sending an eleven-year-old boy to Harvard. Tufts was an excellent small college, so near to Harvard as to be overshadowed by it in the public eye. By virtue of that very fact, it shared in the intellectual advantages of the Boston metropolis. It was possible for us to live near Tufts on Medford Hillside, and for Father to take the trolley from there every day to his Harvard work.

I was admitted to Tufts on the basis of my high school record and a few easy examinations, which in my case were mostly oral. We bought a nearly finished house on the Hillside from the contractor builder, who lived next door to us, and had him complete it in accordance with our requirements.

We came down from the Old Mill Farm a little early to get settled in our new house and to familiarize ourselves with the college. I read the college catalog assiduously, and was then more familiar with the details of Tufts College than at any time later.

I began to get acquainted with the children of the neighborhood. In my early reading I had learned something about hypnotism and decided to try it out myself. I succeeded in nothing, except in offending and terrifying the parents of my playmates. I played a good deal with children of my own age, but without any great community of interest. I found the clerk at the corner drugstore an interesting young medical student, who was prepared to discuss my scientific reading with me and who seemed to be acquainted with the whole of the writings of Herbert Spencer. I have since found Herbert Spencer to be one of the most colossal bores of the nineteenth century, but at that time I held him in esteem.

My duties began with the beginning of term. I was deeply impressed by the age and dignity of my professors, and I find it hard to be aware that I am now much older than most of them were then. I did not find it easy to make the transition between the special privileges of a child, which I had received in high school, and the more dignified relation I was now to have with these older men.

I started to study Greek under a rather remarkable professor by the name of Wade. His family had come from the neighborhood of Tufts College; and as a boy, while stealing a ride on a Boston-and-Maine freight car, he had fallen off and lost a leg. He must always have been shy, and this accident had made him a lonely man; but it had not seemed to interfere greatly with his love for European and Near Eastern travel. He would spend every summer abroad, and he seemed to know every relic of the classical world, whether it was a statue or a local tradition, from the Pillars of Hercules to Mesopotamia. He had a true poetic sympathy with the Greek classics as well as a gift of imparting this sympathy to others. His lantern lectures on Greek art were a delight to me. My father was fond of him, and he used to come over to our house. I would play on the floor and listen with fascination to the wide-ranging conversation between the two men. If anything could have made a classicist of me, it was these experiences.

I had not yet reached the proper stage of social maturity for my English courses. Moreover, the mere mechanics of writing were a serious hurdle to take. My mechanical clumsiness in writing tended to make me omit any word that I could eliminate, and to force me into a great crabbedness of style.

I was already beyond the normal freshman work in mathematics. There was no course which exactly fitted my requirements, so Professor Ransom took me on in a reading course on the Theory of Equations. Professor Ransom has only just retired from Tufts College after a half-century of service. He was a young man when I studied with him, and it stands to reason that he cannot be young now; but in the course of the years I have seen very little change in his vigorous, alert walk, in the forward poise of his bearded chin, and in his enthusiasms and interests. He was a zealot, but a self-effacing zealot. The course was really over my head, particularly in the parts concerning Galois' theory, but with a great deal of help from Professor Ransom I was able to get through. I had started my mathematics at the hard end. Never again at Tufts did I have a mathematics course that demanded so much of me.

I studied German under Professor Fay, known as "Tard" Fay from his lateness to class. He was a thoroughly cultured gentleman with a strong sense of literary values in French and German; and in addition, he was a great mountain climber. I believe that a mountain in the Canadian Rockies bears his name. Naturally, this seemed very romantic to me. Although we did a little bit of easy prose reading in German, the part of the course that attracted me the most was a collection of German lyrics. Here Professor Fay's efforts were more than supplemented by the emotion with which my father would recite to me the many German poems which he knew by heart, and by the work of memorization which was both part of my class duties and which in its way was very welcome to me. My physics classes consisted as usual of recitations and some lectures and demonstrations. It took me some time to develop a proper physical sense to enable me to handle my exercises correctly, but I was always delighted in the demonstrations. I was equally delighted in my work in the chemical laboratory where, in my last year, I studied organic chemistry at probably the greatest cost in apparatus per experiment ever run up by any Tufts undergraduate.

I had a friend and neighbor, Eliot Quincy Adams, who was an undergraduate at M.I.T. during the time I was an undergraduate at Tufts. He introduced me to the possibility of representing four-dimensional figures on the plane or in three-space, and to the study of the four-dimensional regular figures. On one occasion we tried to make a water-dripping electrical machine from old tin cans.

I had several other extracurricular adventures in physics and engineering, more especially in the study of electricity. I shared electrical experiments with a Medford neighbor. We used to generate electricity by turning a hand-run dynamo for the making of colloidal gold and colloidal silver. Whether we actually made these substances I cannot remember, but we thought we did. We also made attempts to realize in practice two physical ideas of mine. One of them was an electromagnetic coherer for radio messages different from the electrostatic coherer of Branly. It depended on the effect of a magnetic field independently of its direction, in compressing a mass of iron filings and powdered carbon, and thus in changing its resistance. There were times when we thought we had obtained a positive effect, but we were not certain whether it was due to this magnetic cohesion or to something quite different. Nevertheless, the idea was sound and if the day of all such devices had not passed with the invention of the vacuum tube, I should be interested to undertake these experiments over again from the beginning.

The other piece of apparatus we tried out was an electrostatic transformer. It depended on the fact that the energy or charge of a condenser is carried as a dielectric strain. The trick was to charge a rotating glass disk or series of disks through electrodes arranged in parallel and to discharge them through electrodes arranged in series. It differed from the electromagnetic transformer in acting on direct currents, and also in the fact that it was essential to the apparatus that the disks should be revolved. We broke an indefinite number of panes of glass in trying to make the machine, and we never quite got it to work. Unbeknown to us, the idea was already in the literature and had been there for a long time. In fact, I have seen a very similar piece of apparatus within the last two years in the laboratories of the School of Engineering of the University of Mexico. It functioned very well. Two successive stages of this machine multiplied the potential by several thousand.

I had an early interest in wireless. I believe that on rare occasions I was able to get a few successive dots and dashes of code signal from the wireless apparatus which I had on my study desk. I was able neither to learn code nor to distinguish myself as a practical constructor of radio sets.

I was socially dependent far more on those of about my age than on the college students with whom I was to study. I was a child of eleven when I entered and was in short trousers at that. My life was sharply divided between the sphere of the student and that of the child.

I was not so much a mixture of child and man as wholly a child for purposes of companionship and nearly completely a man for purposes of study. Both my playmates and the college students were aware of this. My playmates accepted me as a child with them, although I might have been a slightly incomprehensible child, while my fellow students were willing to allow me to participate in their bull sessions if I wasn't too loud and too insistent. I was homesick for the earlier days when I had had a wealth of playmates in Cambridge.

While I was at Tufts, I continued to spend summers on Old Mill Farm where I kept up my contacts with my Ayer friends, and where an occasional Tufts fellow student would come up to visit us. All summers were alike, with the same mushroom-hunting trips, the same general botanizing, the same tramps, and the same swimming in the pond. With my increasing maturity, I came to be taken in as a part of the family on those occasions when we had visits from my parents' friends.

In term time I tried to renew my acquaintance with my old friends of Avon Street. These attempts at renewing the past were not uniformly successful, and finally ceased altogether. Medford Hillside was too far from Cambridge to make it easy for me to visit my friends except on week ends. In addition, with my own isolation from Avon Street, I had become more exacting and jealous. What is more, my Avon Street comrades were growing in divergent directions. The King boys were showing, indeed, an increasing interest in science. While I later came to see a good deal of the King boys again and to play with them in a desultory fashion in their basement laboratory, I saw very little more of many other Cambridge acquaintances.

I have already had something to say about my taste in scientific reading. My nonscientific reading was omnivorous. I made free use of the resources of the various public libraries to which I had access, and I spent a great deal of time in the Children's Room at the Boston Public Library.

That I loved the writings of Jules Verne I have said already, and for adventure I alternated them with Cooper and Mayne Reid. Later, when I came to years of greater discretion and was able to stand a stronger literary diet, Hugo and Dumas were added to my repertory. Dumas in particular was a writer whom I could not bear to put down, and I have spent many hours unaware of the world about me, immersed in the adventures of D'Artagnan or the Count of Monte Cristo.

Naturally I read many of the children's books which the public library had hoarded from an older generation. Louisa Alcott was pleasant enough, but I was a young male snob, and I considered that it was mostly for girls. Horatio Alger combined a superficial appearance of prudence and morality with a crassness of standards of success which was very distasteful to me. I even ventured on a course of reading dime novels, but found them pretty thin. My favorite author among the American boys' writers in the narrower sense was J. Trowbridge, even though his tales of New England and upper New York State boyhood do not impress me quite as much as they once did. On the other hand, I think his three Civil War novels, *Cudjo's Cave, The Drummer Boy,* and *Three Scouts,* represent as high a level as can be achieved in boys' war stories.

I used to buy the old *Strand Magazine* at the newsstands. This was an English periodical which did very well in the United States for many years. It contained some of the Sherlock Holmes stories, some excellent children's tales by Evelyn Nesbit, and a few of A. E. W. Mason's excellent detective stories. It was better written than the majority of American periodicals at the time, and introduced me to many new authors and renewed in my mind much of the quaint, grim aspect of London.

I was not an indoor boy even in the winter. The road that passed by the Tufts College Reservoir offered a great opportunity for coasting. I also enjoyed the crisp, bitter air of winter as I drew it into my lungs on my jaunts with a friend on his newspaper route; and there was a thrill in rushing from building to building on the open spaces of College Hill even in the intense and numbing cold.

My father took a certain glibness of vocabulary of philosophy on my part as indicating that it was my true intellectual field and he encouraged me in it. Therefore in my second year at Tufts, I took several courses in philosophy and psychology under Professor Cushman. He was rather an amateur in philosophy.

The two philosophers who influenced me most in my reading were Spinoza and Leibnitz. The pantheism of Spinoza and the pseudomathematical language of his ethics mask the fact that his is one of the greatest religious books of history; and if it is read consecutively instead of broken up into axioms and theorems, it represents a magnificent exaltation of style and an exertion of human dignity as well as of the dignity of the universe. As for Leibnitz, I have never been able to reconcile my admiration for him as the last great universal genius of philosophy with my contempt for him as a courtier, a placeseeker, and a snob.

The rather dilute material of my courses in philosophy and psychology showed up poorly in comparison with the outside reading and in particular with the great books of Professor William James, which I devoured almost as much as literary tidbits as for their serious content. I learned that James was one of the heroes of my father, and it was not long before I had a chance to visit him in his own house. I do not recall the visit too distinctly, but I have the impression of an amiable, elderly, bearded man who was kind to me in my confusion, and who later invited me to sit in on his Lowell lectures on pragmatism. I attended them and was delighted when Professor James presented my father with a copy of the book in which he embodied this series of lectures. I learned later that the book was really intended for me, and that neither James nor my father wished to raise my conceit by having him make the gift directly.

I do not have the impression that James was at his best in pragmatism. In the more concrete material of psychology, his insight showed itself in every paragraph; but pure logic was never his strongest point. It is one of the clichés in American intellectual history that while Henry James wrote novels in the style of a philosopher, his brother William James wrote philosophy in the style of a novelist. There was more than the style of the novelist in William James, and perhaps less of the philosopher than one might have thought, for his ability to evoke the concrete was to my mind many times greater than his ability to organize it in a cogent logical form.

Also during my second year at Tufts, I found the biology museum and laboratory a place of fascination. The custodian of the live animal house, who was janitor as well, became one of my particular cronies. These noncommissioned officers of science, without whom no laboratory could ever function, are a fascinating group of people, and particularly attract the imagination of a young boy with ambition to go into science. I thought that I would take a fling at a little biological work. I had already gone with Professor Lambert and a group of students on several spring biological excursions to the Middlesex Falls and elsewhere and watched them collect frog spawn, algae, and other objects of biological interest.

I had long shown an interest in biological matters, and my father wished to find out whether it would be worth my while to take up biology for my specialty in my further studies. We took a train trip together to Wood's Hole, where Professor Parker of the Harvard Biology Department allowed me to try my hand in dissecting some dogfish. All that I remember is that my dissections were not particularly brilliant and that in a few days a notice appeared on the dock where I was working saying: "No fish cut up here."

In my last year I decided to have a serious try in biology. I took Kingsley's course on the comparative anatomy of vertebrates. Kingsley, by the way, was the author of the *Natural History* which had so intrigued me about my eighth year. He was a small, birdlike, alert man, and the most inspiring scientist whom I met in my undergraduate days. I had no trouble whatsoever with the class work, for I had always had a good sense of the arrangement of things; but my dissection was too fast and too sloppy. Kingsley saw to it that I had enough work to do, and he gave me a large number of reptilian, amphibian, and mammalian skulls to learn in order to see whether I could discover the secret of their homology. Even here I worked too fast and too messily. I used to spend a great deal of time in the library of the laboratory where I would read such books as Bateson's *Material for the Study of Variation.*

Now biological study may have a morbid attraction for a young student. His legitimate curiosity is mixed with a prurient interest in the painful and the disgusting. I was aware of this confusion in my own motives. I have said that there were passages in my books, scientific treatises and fairy tales alike, which I turned over quickly to avoid but which I would seize on now and then with grim pleasure. The humanitarian tracts about antivivisection and vegetarianism which cluttered up our study table increased my confusion by their exaggeration. In this confusion, I found myself in more than one doubtful situation.

The most serious experience of this confusion occurred in my last year at Tufts. Several of us had been in the habit of doing our dissection of the cat with the aid of a human anatomy—I forget now whether it was Quain's or Gray's. This was a highly desirable practice inasmuch as the anatomy of the cat and of man, though closely parallel, are not literally identical, and so the very differences put us on our mettle and made us better observers. Now some of these human anatomies contain interesting observations concerning the ligature of arteries and the new anastomoses which are established to set the circulation up again. Two or three of us were particularly interested in these passages. The older boys were naturally much more mature than I was, but I am afraid that I must admit that I was the ringleader in the affair. Through the complacency of the janitor we obtained a guinea pig and ligated one of the femoral arteries. I do not remember whether we used anesthetic or not, although it is my faint impression that we did after a fashion, and that the animal got a whiff of ether. The surgery was botched, as we had not properly separated the artery from the accompanying vein and nerve, and the animal died. When Professor Kingsley found out about our misadventure, he was very indignant as the vivisection was undoubtedly a criminal undertaking and might very well have forfeited important privileges of the laboratory. Although I was not effectively punished from outside, I was humiliated and deeply disturbed. It was very clear to me that I could give to myself no account of my motives that would stand inspection before the court of my own conscience. I tried to bury the act in a premature obliviscence which, of course, drove it further into my consciousness. My feeling of guilt about this episode led me to greater tension.

In spite of this interest in biology, it was in mathematics that I was graduated. I had studied mathematics every year in college, largely under Dean Wren whose point of view was more nearly that of the engineer than that of Professor Ransom, who had taught me in my freshman year. I found the courses on calculus and differential equations quite easy, and I used to discuss them with my father who was thoroughly oriented in the ordinary college mathematics. For my routine of double recitation had not changed so far as my mathematical and cultural courses were concerned. In these my father remained my complete master, and there was not the slightest slackening in his stream of invective.

I was graduated in the spring of 1909, having completed my academic course in three years. This does not represent quite as much of a triumph as it seems, for I had fewer distractions than other boys. Only the child can devote his whole life to uninterrupted study.

I decided to go to the Harvard Graduate School the next year to continue my work in zoology. This was primarily my decision but Father was rather unwilling to concur in it. He had thought it might be possible for me to go to medical school, but Professor Walter B. Cannon advised him strongly against it, saying that my youth would be an even greater handicap there than elsewhere.

Since I was no longer to be at Tufts, my father planned to move into Cambridge for the next year. This meant the buying or building of a house. The persuasion of a firm of architect colleagues at Harvard led my father to buy a pair of lots on the corner of Hubbard Park and Sparks Street, on one of which was erected a truly magnificent structure symbolic of the family's growing prosperity. This involved complicated maneuvers, aimed at disposing of the house on Medford Hillside and of the Old Mill Farm at Harvard. It became a part of the family articles of faith to consider that these maneuvers showed an extraordinary foresight and knowingness. The extra lot was to have been sold as soon as we could find a purchaser, but no purchaser presented himself until the house and lot were sold together fifteen years later.

At any rate, we could not spend the summer at Old Mill Farm. We returned to Harvard for the first part of the summer, but in a different part of the township and in an old, tumbledown house which proved to be bad for the health of all of us. We gave up the house before the end of the summer, and left Harvard to finish the vacation in a boardinghouse at Winthrop, from which location my father could watch the completion of our new house. Through the kind offices of two ladies who worked at the Harvard library, we found a tolerable boardinghouse. I had time to waste until Harvard should open. I spent it, partly in the Winthrop public library, partly in Boston where I visited the museums and movies, and partly among the mechanical amusements of Revere Beach.

This was the exciting time of the discovery of the North Pole and the conflicting reports which came in from Cook and Peary. I remember Cook's ingratiating newspaper personality, and the hopes we had falsely put on him. The Mutt and Jeff caricatures of Bud Fisher had but recently started, and they concerned themselves largely with the tragicomedy of polar exploration. As they were portrayed then, Mutt and Jeff could have scarcely been less than thirty years old. It is astounding how spry they remain at the age of more than seventy-two.

One scientific idea had haunted me that summer. It was that the vertebrate embryo represented a coelenterate polyp in which the pouches leading into the arms had become the myotomes. The nerve-belt about the mouth seemed to me to correspond to the brain and the spinal cord, and the bottom part of the central cavity to the vertebrate digestive tube. I remember that I used the microscope of our doctor in Ayer to examine some slides sent me by a friend from Wood's Hole, and I pestered the Carnegie Foundation with the request to allow me to do research on the subject. Of course, this came to nothing.

9 Neither Child nor Youth

I had not realized until my graduation how much the three years at Tufts had taken out of me. I was exhausted, but I could not stop the wheels from going around, and I could not rest.

I did not prosper physically that summer. Every time I got a scratch I festered mildly, and I was in a continual low fever. My emotional state corresponded with my physical condition. The feeling of growing up out of the protection of childhood into that of responsibility had not been welcome to me. With my undergraduate days over, and an unknown future confronting me, I felt at loose ends.

I had had my due share of the brief gratification of Commencement; but behind this happy moment were running the great questions: what should I do in the future and what hopes might I have of success?

The first question had been partly answered by my decision to do graduate work at Harvard. But the question of my success had an added poignancy. Though I had graduated *cum laude,* I had not been elected to Phi Beta Kappa. My record could be interpreted in two ways, and both my appointment and my nonappointment were defensible. But I was given to understand that the chief reason I did not receive the appointment was the doubt as to whether the future of an infant prodigy would justify the honor. This was the first time that I became fully aware of the fact that I was considered a freak of nature, and I began to suspect that some of those about me might be awaiting my failure.

Fifteen years later when I received the honor denied me at my graduation, I had begun to make my mark on the scientific world. To appoint me then was to bet on a horse after the race was over. The appointment at the time of my graduation would have meant a trust in myself and in my future which would have been a source of strength. For a good measure of conceit mingled in me with a greater measure of unsureness.

I have indeed taken a very dim view of all honor societies. This is, of course, the result of my own experience at Tufts, but it has been fortified and strengthened by my subsequent contact with such societies. The fundamental difficulty is that the recognition given by such societies—and indeed by universities in the awarding of honorary degrees—is secondary. They do not seek out young men deserving recognition, but award recognition largely on a basis of past recognition. There is thus a pyramid of honors for those who already have honors, and, per contra, an undervaluing of those who have behind them accomplishments rather than prior recognition.

There is a certain moral duty which I have felt here. I was only too conscious of the bridge that I had to cross to achieve any recognition whatever, and I resented the closed and serried ranks of my seniors as a direct obstacle to advancement and to confidence in myself. Therefore, when later recognition has come to me I have felt loath to be the beneficiary of a process of secondary recognition which I had resented as a young man. In this way, my early rejection by Phi Beta Kappa has strengthened me in a policy on the basis of which I have resigned from the National Academy of Sciences, and have discouraged my friends in the attempt to obtain for me similar honors elsewhere. I have not been absolutely consistent in this matter because there are cases in which the refusal of honors is viewed, not as a sturdy independence on the part of the recipient, but as an ungraciousness to worthy intellectual groups which are consciously or unconsciously seeking the support of their name. Be this as it may, my reaction is essentially the same at the present day as it has been for nearly forty years—that academic honors are essentially bad, and that other things being equal, I choose to avoid them.

Thus my graduation from Tufts forced me to face one of the greatest realizations that the infant prodigy must make: he is not wanted by the community. He has received no special rebuffs from his contemporaries. All children quarrel, and it is not until they reach the years of discretion that they acquire something superior to the social mores of the zoo. But as the infant prodigy comes to realize that the elders of the community are suspicious of him, he begins to fear reflections of this suspicion in the attitude of his contemporaries.

There is a tradition, not confined to the United States, that the child who makes an early start is intellectually drawing on his life capital of energy and is doomed to an early collapse and a permanent second-rateness, if not to the breadline and the madhouse.

My experience leads me to believe that the prodigy is desperately unsure of himself and underrates himself. Every child, in gaining emotional security, believes in the values of the world around him and thus starts by being, not a revolutionary, but an utter conservative. He wishes to believe that his elders, on whom he is dependent for the arrangement and control of the world in which he lives, are all wise and good. When he discovers that they are not, he faces the necessity of loneliness and of forming his own judgment of a world that he can no longer fully trust. The prodigy shares this experience with every child, but added to it is the suffering which grows from belonging half to the adult world and half to the world of the children about him. Hence, he goes through a stage when his mass of conflicts is greater than that of most other children, and he is rarely a pretty picture.

In my earlier years, I had not been aware that I was an infant prodigy. Even in my high school days the matter just began to become clear to me, and during my college training I could not avoid facing it. One of the less agreeable consequences was that I was pestered by a swarm of reporters who were eager to sell my birthright at a penny a line. I soon learned that whining tone in which a reporter, bent on intruding on one's privacy, will tell you, "But my job depends on my getting this interview!" Finally I learned that reporters were on the whole to be avoided, and I ultimately developed enough fleetness of foot and ability at dodging to conduct a reporter across the college campus, later through the back alleys of Harvard Square, without giving his partner a chance to take a usable photograph.

Most of these articles appeared in the Sunday supplements of the newspapers. They belonged to a class of ephemeral literature which has long since returned to the gutters from which it emanated. While they flattered my childish desire for attention, both my parents and I recognized this for the sickly narcissism which it was; and it was not pleasant to see oneself endowed with a ten days' immortality between an account of a two-headed calf and the more-or-less true tale of the amours of the Count of X with the elderly wife of the millionaire Y.

It was the more serious articles that did the most harm. The suave and flattering articles of H. Addington Bruce[1] gave my father a chance to sound off with his unflattering theories of my education, while an occasional article in the trade journals of the educationalists[2] displayed to me the full tally of my gaucherie and social rejection.

I fear that Father himself was not immune to the temptation to grant interviews to the slicks about me and my training. In these interviews, he emphasized that I was essentially an average boy who had had the advantage of superlative training. I suppose that this was in part to prevent me from being conceited, and that it was no more than a half-representation of my father's true belief. Nevertheless, it rendered me more diffident as to my own ability than I would have otherwise been even under my father's scolding. In short, I had the worst of both worlds.

Besides the direct damage to me, these articles could only have accentuated that feeling of isolation forced on the prodigy by the hostility latent in the community around him.

The end of my college career forced me to take stock of myself and of my position in the world about me. In my exhausted state, this assessment and evaluation took on a dismal tone. I had for the first time become acutely conscious of the fact of death. I would count the fourteen and a half years that I had lived, and would measure my probable future life, and what I might expect of that. I could not read a novel without figuring out the ages of the characters and how many years they had left them to live, nor could I avoid looking up the lives of the authors of great books, and finding out for myself their own ages at the writing of these works and how many more years had been granted them. This obsession, of course, touched my relations to my parents and my grandparents, and for a period made my life utterly intolerable.

The fear of death ran parallel to and was reinforced by the fear of sin. My adventure in vivisection had brought a terrible awareness of possibilities in me of cruelty and the delight in cruelty which could only be brought into full focus with pain and suffering. My years in college had come at almost precisely the period in which I was turning from a boy into a very young and inexperienced man. The consciousness of the possibilities of manhood in me without any corresponding experience and worldly wisdom by which I could guide them led me into a frightful panic in which at times I sought for the blocked way back into childhood. I had been brought up in a doubly Puritan environment which supplemented the original intrinsic puritanism of the Jew with the puritanism of the New Englander. And the most elementary phases of my self-esteem from childhood into adolescence appeared to me either sinful or fraught with the possibilities of sin. These possibilities were things that I could not discuss freely even with my parents. My father would have taken what was on the whole a sympathetic attitude, but an attitude expressing a sympathy which was loath to go into details, and which on the whole was not likely to answer my inarticulate attempts to express myself about disagreeable matters with a willingness to listen and to find out what was really troubling me. On the other side, my mother combined a literal acceptance of the minutest code of puritanism with an adamantine unwillingness to admit that anything that a child of hers might do could be a possible offense against this code.

In other words, in these matters which were of great concern to me, I was met not so much with an accusing hostility as with a blockage of communication which left me substantially alone with my problems. This is no peculiar burden of the infant prodigy; it is the common lot of many adolescents, perhaps most. Yet when it is combined with the manifold other burdens that are thrust upon the prodigy, it is naturally accentuated.

If a child or a grandchild of mine should be as disturbed as I was, I should take him to a psychoanalyst, not with confidence that the treatment would be successful in some definitive way, but at least with the hope that there might be a certain understanding and a certain measure of relief. But in 1909 there were no psychoanalysts in America; or at least, if one or two venturesome disciples of Freud might have strayed so far, they were isolated practitioners. There was no tradition of resorting to them, and they were scarcely available to persons of the moderate means of a college professor. Moreover, even twenty years later, it would have seemed to my parents a blasphemy and a confession of defeat to admit even to themselves that a member of the family might need such treatment.

However, these remarks are after the fact. The fact is that it was then possible to allay neither the agonies of leaving childhood nor the sense of guilt almost inseparable from adolescent sexuality. Like many other adolescents, I walked in a dark tunnel of which I could not see the issue, nor did I know whether there was any. I did not emerge from this tunnel until I was nearly nineteen years old and had begun my studies at Cambridge University. My depression of the summer of 1909 did not suddenly end; rather it petered out.

My relationship with my father was gradually undergoing a change which I was only dimly aware of. Tufts had given me a partial release from his hitherto unrelaxing authority, for I tried my wings in such fields as biology where he could no longer lead me, and where I could hope to excel him. Indeed, my study of mathematics had won his approval, and at the same time had provided a field for me into whose later development it was impossible for him to follow me and break my independence. My study of mathematics gave me a consciousness of strength in a difficult field, and this was one of its great attractions. My mathematical ability at this time was a sword with which I could storm the gates of success. This was not a pretty or a moral attitude, but it was real, and it was justifiable.

From the time that I started to Tufts, my father used to tell me of the philological work that he was doing. Some of it concerned the early history of the Gypsies; some of it was devoted to moot points of philology, such as the origin of the Italian word *andare* and of the French word *aller,* some of it to Hecate worship and its influence in medieval Europe, some of it to Arabic influences on European languages. There was also a great deal of interesting research on the grand outlines of the relations between the established groups of languages such as the Indo-European, the Semitic, the Dravidian, et cetera; and later this comparative study was extended to the languages of the Americas and to the problem of African influence on these languages.

In all these investigations, my father combined a rare extent of linguistic information, an almost unexampled philological-historical sense, and a very modern mistrust of purely formal phonetic philology in favor of the more historical and empirical point of view which has come into ascendancy in the present generation. Father was a great admirer of Jespersen and his work; and when the time shall come to make an objective and judicial inquiry into the sources of modern philological ideas, I myself have no doubt that his name will stand beside that of Jespersen among the greatest in linguistic science.

Nevertheless, Father's intuition, although it was supported by an almost superhuman industry of reading and research, worked too fast for his formal logical processes. Philology was for him a piece of deductive work, a magnificent crossword puzzle; but I am afraid that he often gave up the writing of the words when he had still about a quarter of the paper left to cover. To a very considerable extent he knew what the remaining words were, but was simply too contemptuous of the powers of his readers to dot every *i* and cross every *t.* Nevertheless, I am convinced that (in a not appreciable and important fraction of the cases) he was not sure in his own mind of the last refinement of his work.

Now Father had come to philology with a thorough linguistic training, but without the sponsorship of any of the craftsmen of that guild. He was a man who liked popularity and acclaim, but he was essentially a lonely figure. The great foreign names in philology regarded him as an intruder, and his very ability made him at once dangerous and unpopular. At Harvard, the Slavic department formed a little autonomous enclave in the Division of Modern Languages, and although Father was consulted by everyone, he belonged to no one. Concealed behind his personal popularity, which was great, there was the inevitable distrust of the plodder for the genius. We all know that German aphorism credited to Ludwig Börne: when Pythagoras discovered the theorem of the right triangle, he sacrificed a hundred oxen; since then, whenever a new truth is unveiled, all oxen tremble.

Under these circumstances—a hermit in the midst of a great city—it was not unnatural that my father should turn to me for intellectual companionship and support. I was profoundly interested in his work, but I was not always convinced by every detail of his argument. When I showed my little faith by asking a question, Father was indignant. It was treason for me to question his slightest word. Indeed, I was not entitled to an opinion on most of the questions which he put to me, but I could only answer by the light of what little judgment was in me, and Father's whole precept and example were against saying yes where I could give no real assent. I should have realized, of course, that in much of Father's conversation with me I was merely a lay figure to represent the scientific public in a dialogue which Father was merely holding with his own doubts. Nevertheless, and despite all my protests that I did not know enough of the subject of which he was talking to give a reasonable opinion, my father would often demand of me a direct answer to a specific question. Then I might well find myself between a lying assent and an unwilling defiance. I preferred the defiance; but my father would certainly have seen through the falsity of the assent and would have berated me for my half-heartedness. It was unfair and I knew it was unfair; but I also knew that my father brought these issues to me out of a compelling inner necessity and that he was not a happy man.

Notes

1. *Loc. cit.*

2. Katherine Dolbear, "Precocious Children," *Pedagogical Seminary,* Vol. 19, p. 463.

10 The Square Peg

Harvard, 1909–1910

In the fall of 1909 President Eliot retired from Harvard and was succeeded by Abbott Lawrence Lowell. I went from Winthrop to view this ceremony at which several honorary degrees were conferred in the open air in front of University Hall, and I enjoyed the academic pageantry.

I was not aware, and indeed few of us were at the time, that the departure of Eliot coincided with the end of a great age and the beginning of an age of smaller things. Eliot may have had the temperamental limitations of a New Englander, but he had the outlook of a scholar and man of the world. Lowell was dedicated to a Harvard that should be the private preserve of a ruling class.

The reign of Lowell soon began to contribute much to the prosperity of the members of the faculty, and my father was among those who had good cause to be financially grateful to the president. Yet this gratitude could never be untainted. If Lowell made the professors rich, it was because he wished them to be allies of the rich. He wished them to eschew the companionship of the common man, and to find their comrades in the circles of big business and big industry.

My parents were not originally aware that the cornucopia extended to the faculty by President Lowell had a razor blade concealed in it. Many years later when my father received at his retirement a letter nearly as peremptory as that which one gives to an unsatisfactory kitchen maid, President Lowell, this paragon of all virtues, became in the eyes of my father and mother a monster of iniquity. But he was neither a paragon nor a monster. He was essentially a superficially polished and rather ordinary man of conformist loyalties, who was loyal to his social class and to very little else.

In the early days of his presidency I used to attend the rather stiff and formal parties for college students at the President's House on Quincy Street. We learned to balance tea cups on our knees, and to hear tales of Mrs. Lowell's memories of the great frost that had tied up Boston harbor one remote winter in the past. The president would utter *obiter dicta* running from one end of the field of government and administration to the other, and would trot out his favorite idea, that while the government should make use of experts, they should be heard and not seen. He sang the praises of the amateur in politics, of the man who could judge everything well but who did not need to carry in his mind any particular content of information.

Term at Harvard began a few days before we moved into our new house in Cambridge. I was nearly fifteen years old, and I had decided to make my try for the doctor's degree in biology.

My first memories of my Harvard work are of collecting for my histology course a number of leeches from a small pond on the Fresh Pond reservation in Cambridge. My histology course began as a muddle and continued as a failure. I had neither the manual skill for the fine manipulation of delicate tissues nor the sense of order which is necessary for the proper performance of any intricate routine. I broke glass, bungled my section cutting, and could not follow the meticulous order of killing and fixing, staining, soaking and sectioning, which a competent histologist must master. I became a nuisance to my classmates and to myself.

My clumsiness and ineptness were due to a mixture of several factors. Probably there was a considerable amount of primary muscular lack of dexterity in my make-up, but this was not the whole show by any means. My eyes were another major factor; and though my eyesight was well corrected by efficient glasses and was at that time almost untiring, there are certain secondary disadvantages of the myope which are not immediately obvious to the average man. Muscular dexterity is neither a completely muscular nor a completely visual matter. It depends on the whole chain which starts in the eye, goes through to muscular action, and there continues in the scanning by the eye of the results of this muscular action. It is not only necessary for the muscular arc and the visual arc to be perfect, each by itself, but it is equally necessary that the relations between the two be precise and constant.

Now a boy wearing thick glasses has the visual images displaced through a considerable angle by a small displacement in the position of the glasses upon the nose. This means that the relation between visual position and muscular position is subject to a continual readjustment, and anything like an absolute correlation between these is not possible. Here we have a source of clumsiness which is important but which is not immediately obvious.

A further source of my awkwardness was psychic rather than physical. I was socially not yet adjusted to my environment and I was often inconsiderate, largely through an insufficient awareness of the exact consequences of my action. For example, I asked the other boys the time instead of getting a watch myself, and finally they gave me one. It is a little easier to understand this inconsiderateness if one realizes that although my personal expenses were taken care of very generously by my parents, I had not become accustomed to the complete control of any regular weekly allowance at all. A further psychic hurdle which I had to overcome was impatience. This impatience was largely the result of a combination of my mental quickness and physical slowness. I would see the end to be accomplished long before I could labor through the manipulative stages that were to bring me there. When scientific work consists in meticulously careful and precise manipulation which is always to be accompanied by a neat record of progress, both written and graphical, impatience is a very real handicap. How much of a handicap this syndrome of clumsiness was I could not know until I had tried. I had moved into biology, not because it corresponded with what I knew I could do, but because it corresponded with what I wanted to do.

It was inevitable that those about me discouraged me from further work in zoology and all other sciences of experiment and observation. Nevertheless, I have subsequently done effective work together with physiologists and other laboratory scientists who are better experimenters than I, and I have made some definite contributions to modern physiological work.

There are many ways of being a scientist. All science originates in observation and experiment, and it is true that no man can achieve success who does not understand the fundamental methods and mores of observation and experiment. But it is not absolutely necessary to be a good observer with one's own eyes or to be a good experimenter with one's own hands. There is much more to observation and experiment than the mere collection of data. These data must be organized into a logical structure, and the experiments and observations by which they are obtained must be so framed that they will represent an adequate way of questioning nature.

The ideal scientist is without doubt the man who can both frame the question and carry out the questioning. There is no scarcity of those who can carry out with the utmost efficiency a program of this sort, even though they may lack the perspicacity to frame it: there are more good hands in science than there are good brains to direct them. Thus, although the clumsy, careless scientist is not the type to do the greater part of the work of science, there is other work for him in science if he is a man of understanding and good judgment.

It is not very difficult to recognize the all-round scientist of whose calling there is no doubt. It is the mark of the good teacher to recognize both the laboratory man who may do splendid work carrying out the strategies of others, and the manually clumsy intellectual whose ideas may be a guide and help to the former. When I was a graduate student at Harvard, my teachers did not recognize that despite all my grievous faults, I might still have a contribution to make to biology.

The late Professor Ronald Thaxter was, however, an exception. I took his course in cryptogamic botany. The lectures were elaborate and detailed accounts of the anatomy and phylogeny of algae and fungi, mosses, and the ferns and their allies. This normally involved the student in the taking of elaborate notes and the copying of diagrams which the professor had drawn on the board. The laboratory work consisted of the inspection and the drawing of living plants and sections of cryptogamic tissues. My laboratory work was worse than terrible: it was hopeless. Yet I got a B-plus in the course without having retained in my possession a single note.

Taking notes is the student's craft, and I did not master it. I think that there is a definite conflict between the use of a student's attention for the writing down of the best possible set of notes, and the use of his attention for the understanding of the speaker as he proceeds. The student must choose to do one or the other, and each has its advantages. If, like me, he is so clumsy of eye and hand that his notes are bound to be incomplete and only partly legible, he falls between two stools. If he decides to take notes at all, he has already destroyed much of his ability to grasp the argument in flight, and at the end of the course has nothing but a mass of illegible scribbles. Yet if his memory is as good as mine is, it is far better to give up the idea of taking notes and to organize in his mind the material as it comes to him from the speaker.

My comparative anatomy course went along in a manner intermediate between that of my histology course and that of my course in botany. My drawings were execrable, but I had a good understanding of the facts. My great tendency, as it had been with Kingsley, was to hurry the work rather than go into the fine points. This, indeed, has been my tendency throughout life. It is not difficult for me to explain. I have a rather quick insight into ideas, and an extreme lack of manual dexterity. It has always been difficult for me to hurry my manual efforts into a pace corresponding to the flow of my ideas, or to marshal my ideas into a sequence sufficiently slow to conform to the demands of my physical resources.

I found my physical recreation in the gymnasium. I joined a calisthenics class in which the usual setting-up exercises were combined with some mild folk dancing. I tried to join in the scrub basketball games that were being played in the basement of the gymnasium, but glasses were not to be thought of in the rough and tumble of the sport, and without glasses I was wholly useless.

I spent many happy hours in the library of the Harvard Union. It had recently been founded by the benevolent authorities as a "*club des sans club*" in protest against the exclusiveness of the Harvard clubs. It did not thrive under the new Lowell regime. A new anti-Semitism was the order of the day, and the authorities began to think in terms of a *numerus clausus*. Whispers gradually appeared out of nowhere to the effect that the Union had become the headquarters of Jews and similar undesirables. These rumors received a sympathetic ear in my home. My mother questioned me about the Jewish character of the Union, and began to indicate to me that it might be a little better if I were not seen there so much. As I had no other place to go for my social life, these questions distressed me greatly. However, there was no way to sidetrack them.

In one particular aspect, the year 1909 was an *annus mirabilis* at Harvard. I was one of five infant prodigies enrolled as students. The others were W. J. Sidis, A. A. Berle, and Cedric Wing Houghton. Roger Sessions, the musician, entered Harvard the next year at fourteen. I was nearly fifteen at the time, and a first-year graduate student. W. J. Sidis entered college as a freshman. He was the son of a psychiatrist, Boris Sidis, who, together with his wife had opened a private mental hospital in Portsmouth, New Hampshire. Like my father, Boris Sidis was a Russian Jew, and also like my father he had strongly developed opinions concerning the education of children.

Young Sidis, who was then eleven, was obviously a brilliant and interesting child. His interest was primarily in mathematics. I well remember the day at the Harvard Mathematics Club in which G. C. Evans, now the retired head of the department of mathematics of the University of California and Sidis's lifelong friend, sponsored the boy in a talk on the four-dimensional regular figures. The talk would have done credit to a first- or second-year graduate student of any age, although all the material it contained was known elsewhere and was available in the literature. The theme had been made familiar to me by E. Q. Adams, a companion of my Tufts days. I am convinced that Sidis had no access to existing sources, and that the talk represented the triumph of the unaided efforts of a very brilliant child.

There was likewise no question that Sidis was a child who was considerably behind the majority of children of his age in social development and social adaptability. I was certainly no model of the social graces; but it was clear to me that no other child of his age would have gone down Brattle Street wildly swinging a pigskin bag, without either order or cleanliness. He was an infant with a full share of the infractuosities of a grown-up Dr. Johnson.

In his childhood, Sidis had received more than his share of publicity. The papers had a field day when, after one or two years of limited success at Harvard, Sidis received a job at the new Rice Institute in Houston, Texas, under the sponsorship of his friend Evans. He failed to show the maturity and tact needed to make good at this impossible task. Later on, when he carried a banner in some radical procession and was locked up for it, the papers were even more delighted.

Sidis broke down after this episode. He developed a resentment against his family so bitter that he would not even attend the funeral of his father, and a resentment against all mathematics, science, and learning. Indeed, he developed a hatred for anything that might put him in a position of responsibility and give him the need to make decisions.

I saw him many years afterward, when he used to haunt the halls of the Massachusetts Institute of Technology. His intellectual career was behind him. He asked for nothing more than a job at which he might earn his bread and butter as a routine computer, and the chance to indulge his simple amusement of collecting streetcar transfers from all over the world. He avoided publicity as he would the plague.

By this time, the Second World War was approaching, and there was much computing work to be done at the Massachusetts Institute of Technology. It was not difficult to find Sidis a job, although it was always hard to avoid giving him a better and more responsible job than he was willing to undertake. The reports on his work never varied. Within the limitations that he imposed upon himself, he was an exceptionally rapid and competent computer. He had even managed to acquire a certain minimum degree of neatness in his personal appearance, and was a quiet, inoffensive worker to have around. He had a limited security in his work with us; we all knew his story and respected his privacy.

I have no doubt that even at the time I knew him at Harvard, competent psychoanalytic help of the sort that is readily available today could have saved young Sidis for a more useful and a happier career. I am equally sure that his father, precisely because he was a psychiatrist, and was busy reading the fine print of the psychological map, was unable to see the inscription written on it in the largest characters, stretching from one corner to the other. It was perfectly clear that the later collapse of Sidis was in large measure his father's making.

Without condoning the follies of the older Sidis, it is at least necessary to understand them. Picture a Jew, fresh from the persecutions of Russia, and a denizen of a land that has not yet fully made up its mind to want him. Picture his successes, exceeding what he could have hoped for as a child but still falling short of his wishes. Picture the brilliant child, destined for a still greater success, beyond the possibilities open to the parents. Picture the Jewish tradition of Talmudic learning which, from the times of Mendelssohn, has been transferred to the secular learning open to the whole world, and picture the ambition of the orthodox Jewish family to have among their sons at least one great rabbi—and the universal sacrifice which such a family will make to achieve this end.

I am loath to add my name to those who join in an excessive stream of condemnation of Boris Sidis. I have in my possession a letter from a writer who, after spending a day looking over the published accounts of the case, was sure that the father was guilty of a capital crime, and that this crime was the result of the attitude of the scientist who is so devoted to science that he is willing to commit an act of spiritual vivisection on his own child. I think such judgments are premature and are wanting in the sympathy and compassion that mark the really great writer.

I find it appropriate to discuss Sidis in some detail as his life formed the object of a cruel and quite uncalled-for article in *The New Yorker*. Some years ago, when Sidis was leading a broken but independent life around the Massachusetts Institute of Technology, an enterprising journalist got hold of his saga. I believe he won Sidis's confidence. Sidis, who through his later life was a defeated—and honorably defeated—combatant in the battle for existence, was pilloried like a side-show freak for fools to gape at.

He had ceased to be news for nearly a quarter of a century. If any man had done wrong, it was his father, who was long dead, and the article could right no wrong done the son. The question of the infant prodigy was not a live issue, even in the public press, and had not been for some time, until *The New Yorker* made it so. In view of all this, I do not see how the author of the article and the editors of the magazine can support their conduct by the claim that the actions of people in the public eye are the object of fair newspaper comment.

I suspect certain members of *The New Yorker* staff of muddled thinking. In many literary circles, anti-intellectualism is the order of the day. There are sensitive souls who blame the evils of the times on modern science and who welcome the chance to castigate its sins. Furthermore, the very existence of an infant prodigy is taken as an affront by some. What, then, could constitute a better spiritual carminative than an article digging up the old

Sidis affair, at the same time casting dishonor on the prodigy and showing up the iniquity of the scientist-prodigy-maker? The gentlemen who were responsible for this article overlooked the fact that W. J. Sidis was alive and could be hurt very deeply.

Sidis sued *The New Yorker* for damages. It is not my object to criticize the courts, and I do not know enough law to describe the case fairly. I believe, however, that the main issue was the fact that to obtain damages in a libel suit where the specific allegations are undisputed and where they concern only the tone of ridicule of the article, it is necessary to prove specific damage which will hamper the injured party in the carrying out of his professional work. Now Sidis had no profession, and the proof of such damages was impossible. He was only a day worker, and it is a fact that no criticism of this sort would have closed his employment or led to a decrease in his wages. This was not the sort of libel case in which personal agony was a legal issue. Thus *The New Yorker* won its case.

When Sidis died some years later, I remember how shocked we were. We tried to obtain from the hospital some indication of the disease that had killed him. We were not kinsmen, and the hospital authorities were quite properly reticent. I do not know to this day what the cause of his death was.

The issue was raised again by an article published in *This Week Magazine* of the Boston Sunday *Herald* in March, 1952. It was entitled "You Can Make Your Child a Genius," and was based on an interview with the mother of William James Sidis. As a piece of reporting, it is an ordinary journalistic hack job, neither better nor worse than a thousand others that appear in the Sunday supplements and the slicks. As a human document, it scarcely merits consideration.

Sidis's failure was in large measure the failure of his parents. But it is one thing to have compassion for an honest human weakness, and it is another thing to brandish before the public the wreck of a human adventure as if it had been a success. So you can make your child a genius, can you? Yes, as you can make a blank canvas into a painting by Leonardo or a ream of clean paper into a play by Shakespeare. My father could give me only what my father had: his sincerity, his brilliance, his learning, and his passion. These qualities are not to be picked up on every street corner.

Galatea needs a Pygmalion. What does the sculptor do except remove the surplus marble from the block, and make the figure come to life with his own brain and out of his own love? And yet, if the stone be spaulded and flawed, the statue will crumble under the mallet and chisel of the artist. Let those who choose to carve a human soul to their own measure be sure that they have a worthy image after which to carve it, and let them know that the power of molding an emerging intellect is a power of death as well as a power of life. A strong drug is a strong poison. The physician who ventures to use it must first be sure he knows the dosage.

The striking thing to many about the group of precocious children who were studying at Harvard in 1909–10 is that we were not an isolated group at all: in some ways we were alike and in some ways different. At least three came from homes of very ambitious fathers, but the fathers were not alike in the slightest degree, nor would their ambition take the same form. My own father was primarily a scholar, and his ambition was for me to excel in scholarship. He took his duty in this matter very seriously, and spent a large if not excessive amount of time in my education. Berle's father wanted his son to be a successful lawyer and a statesman. He took a large share in Berle's early education, but I do not believe that he continued it over the period in which Berle was at Harvard. Sidis's father was a psychologist and a psychiatrist by profession. I have said that he had wished his son to excel in scholarship, although I cannot find in what I remember of young Sidis the same degree of continued participation in his son's education that my father had exhibited in mine. I have no doubt that in Sidis's earlier childhood he had received intense paternal discipline. But during the period in which I first met him when he was about eleven years old, he was left alone in a roominghouse for the greater part of the year in a Cambridge in which he had few personal friends and even fewer intimates.

I know nothing about the relations of either Houghton or Sessions with their families. I presume that part of the reason is that there was less to know and that the families did not participate as intimately and overwhelmingly in their education as did the families of the other three of us. I believe that these boys were left more to their own resources and that consequently they were not subject to the same pressure to which we were subject.

I remember the fourteen-year-old Berle as he first visited me, point-device, carrying his little kid gloves and presenting a formal visiting card. This was a phenomenon new to me, for my academic precocity had prevented neither me nor my parents from being quite aware that after all I was a boy not yet fifteen. Yet I was a physically precocious child, well past the beginning of puberty, while this youngster, although practically of the same age, was in every bodily respect at least five years my junior. To find this poise and sense of social protocol in a half-grown boy was a shock to my sensibilities.

Sidis had his collection of streetcar transfers to amuse him, and Berle had a fad almost as individual. He was interested in the various underground passages of Boston, such as the subways and the sewers and various forgotten bolt holes; and in particular he introduced us to that romantic passageway dating from early colonial times, which still passed in those days under the site of the old Province House. The bricks dated back more than two and a half centuries, and we both showed our essential boyishness by joining in plans for a literary hoax by which we should discover a Shakespearean document buried behind the wall.

I have had no contact with Berle since his graduation from college. He became one of that group of young lawyers and statesmen sponsored by Felix Frankfurter, a group that has been a fertile source of talent. Berle's rise has been quick and sure and in no way surprising, for his ambition was matched by his talents. *The New Yorker* handled him a bit roughly in a profile. But I cannot feel any such indignation at the rough profilement of Berle as I did in the case of Sidis. Berle has been a public figure and he has held a great deal of power. So long as certain canons of journalistic decency were adhered to (and I could not say that they were violated by *The New Yorker),* his acts and personality were of legitimate public interest and the subject of fair comment. Sidis was out of public life and it was an act of the utmost cruelty to summon him back.

We five boys, varying in ages between eleven and fifteen, would not naturally have sought one another's companionship if it had not been for the special circumstances in which we were placed. I have said that Berle and I did not find our first contact particularly stimulating to either of us, and that after our formal introduction to one another we found very little to talk over. Later on we used to bowl together in the bowling alley in the basement of the gymnasium, and once or twice we walked to Boston together. Berle told me something of his interest in the underground passages of Boston, and as I have said, we planned to participate in a joint literary hoax. But our acquaintanceship did not last for it had no roots.

Sidis was too young to be a companion for me, and much too eccentric, although we were in one class together in postulate theory and I respected the work that he did. Houghton was a very good friend of mine and I got to know him the best of the lot. I used to visit from time to time in his rooms in Divinity Hall, and he impressed me as a thoroughly agreeable person. He appeared to have a promising future, which was tragically cut off by a premature death caused by appendicitis at about the time he expected to graduate.

As to Sessions, I met him only once or twice, and the great divergence in our interests prevented us from developing a common vocabulary.

Thus there was little acquaintance and attraction between us as a group. I tried at one time to unite the five of us into a sort of prodigy club, but the attempt was ridiculous for we did not possess a sufficient element of coherence to make a joint social life desirable. We associated with students older than ourselves for intellectual purposes, and with the brighter boys of our own age but of normal advancement in school for the sake of our needs as children and adolescents. In all of our cases, our social relations were better taken care of elsewhere than by a close social contact with those of our own kind. We were not cut from the same piece of cloth, and in general there was nothing except an early development of intelligence that characterized us as a group. And this was no more a basis for social unity than the wearing of glasses or the possession of false teeth. Louise Baker, in her witty book *Out on a Limb,* shows that there is no necessary bond of companionship between one one-legged little girl and another one-legged little girl, and my experience leads me to believe that the sharing of a precocious school career is no sounder a basis for one's companionship than the sharing of a mutilation.

By the end of my first term at Harvard, it had already become more than doubtful whether a career awaited me in biology. As usual, the decision was made by my father. He decided that such success as I had made as an undergraduate at Tufts in philosophy indicated the true bent of my career. I was to become a philosopher and to apply for a scholarship at the Sage School of Philosophy at Cornell University, where my father's old friend of Missouri days, Professor Frank Thilly, filled the chair of ethics. I can understand that with our limited family means and the needs of the other children coming to the front, it was not possible to allow me the chance to make serious errors, but this deprivation of the right to judge for myself and to stand the consequences of my own decision stood me in ill stead for many years to come. It delayed my social and moral maturity, and represents a handicap I have only partly discarded in middle age.

I was, however, not reluctant to leave Harvard. I had felt myself to be a misfit there from the first. Harvard impressed me as being overwhelmingly right-thinking. In such an atmosphere, a prodigy is likely to be regarded as insolence toward the gods. My father's publicly announced attitude toward my education had aroused hostility among his colleagues which made my lot no easier.

I had hoped to find a free intellectual life among my fellow students. Some, indeed, I found who were willing to discuss intellectual matters and to fight dialectically for their convictions. But in the Harvard order of things, a gentlemanly indifference, a studious coldness, an intellectual imperturbability joined with the graces of society made the ideal Harvard man. Thirty years later, I have been shocked rather than surprised by the dry emotional and intellectual sterility into which some of these men have settled.

At the close of the academic year, my father made one decision for which I shall eternally thank him. He rented Tamarack Cottage in the township of Sandwich, New Hampshire, as our home for that summer. Sandwich has remained my summer home for much of the time until the present day, and has a special place in my heart for the loveliness of its scenery, for the walks and climbs afforded by its mountains, and for the sober dignity, reserve, and friendliness of its country people. Some of my walks were short trips to Tamworth or to Sandwich Center, on which I stopped at the back-doors of my neighbors for a chat or a glass of milk or water; some were climbs which my father took with my older sister and me over the trails of Whiteface and Passaconaway. There was one long trip of nearly a week during which my father, my sister, Harold King, and I packed a pup-tent and our duffle bags into the Passaconaway valley, up the logging railways to the remains of Camp Six in the Livermore Wilderness, down by the lumber village of Livermore, to the Notch Road and up the Crawford Trail to the top of Mt. Washington. From there we came down over the heights of Boot's Spur to Tuckerman's Ravine and to the Pinkham Notch, Jackson, Intervale, and Tamworth. We afterward heard that we had passed the home of William James on the day of his death.

Nowadays I spend all of my normal year in New England, my winters in Belmont, Massachusetts, and my summers in Sandwich, New Hampshire, when I am not globe-trotting. Yet, although I spend the greater part of my time in the city, it is my country home to which I feel I most deeply belong. The New Englander is reticent whether he comes from the country or the city; but the reticence of a countryman is most humble and proud. The New Hampshire farmer has a sense of living with his ancestors which he derives from tilling the same land, dwelling in the same house, and often from using the same tools. Yet this sense of historical continuity is too personal and too private to flaunt in the face of the visitor. For the city New Englander, the family is often an asset to the individual; but for the countryman, the individual is but a passing phase in the continuity of the family. If the countryman is reticent it is because he thinks that, important as his affairs may be to himself, it is your own affairs which are most important to you, and he waits for some sign that he is welcome before he intrudes upon you. He waits to look you over, but he also waits to give you a chance to look him over. And even in the meantime, he regards you as a whole individual rather than such an abstract half-man as an employer or a customer. He will not start business with you before he has given you five or ten minutes' gossip, as his right and your own, as two human beings who have more important relationships with one another than that of buying and selling. He will accept a gift but not a tip, a cigar but not a dollar bill. In short, whether you can love him or not, and very often you can, you can and must respect him because he respects himself.

We had many friends in the mountains. One eccentric and interesting neighbor was Professor Hyslop, famous for his work on psychic research. We spent many an interesting hour before the fire in his cottage, a structure which had originally been built for a henhouse, and listened to his weird tales of ghouls and ghosts and mystical noises and mediums. His house belonged to a Mr. Hoag of Philadelphia whose family still inhabits the region into the third generation. The Hoag boys, young Hyslop, and I belonged to a very informal baseball team which also comprised the son of Professor Dugald Jackson of the Massachusetts Institute of Technology, two sons of the late President Grover Cleveland, and one or two of the Finlay boys from New York. I do not remember the names of the other players. We used to practice on a none-too horizontal field near the Finlay home, and the practice always meant for me a five-mile walk there and back. We played no more than two games, both of which we lost ignominiously. I was a substitute on the team. I think nothing can express better my exact status as an athlete.

11 Disinherited

Cornell, 1910–1911

I had won a scholarship at Cornell. Father was to accompany me to Ithaca, and at the end of the summer we had to decide how to get there. This was still in the days before the interurban trolley had been superseded by the bus and the coach. Father and I decided on a romantic trolley jaunt to central New York and Ithaca. There we called on Professor Thilly and made plans for the ensuing academic year. It was decided that I was to have the free run of the Thilly house, and to confide my youthful troubles and perplexities to Professor Thilly and his wife.

My father and Professor Thilly had a long evening talk together about the old days at the University of Missouri and about many other matters. In the course of the rather miscellaneous discussion, Thilly casually mentioned to Father that he remembered an allusion many years ago to a much earlier philosopher in the family, Maimonides. Father admitted hearing of rumors, perhaps not authentic but depending on old documents that my grandfather had lost, to the effect that we were descended from Maimonides.

I had not previously heard of the tradition, nor even of the name of Maimonides. Naturally I did not delay long before I had recourse to the encyclopedia. I found there that Maimonides, or Rabbi Moses ben Maimon, known as Rambam according to the conventional Jewish use of initials, was a native of Cordova domiciled in Cairo, and the body physician to the vizier of the Sultan Saladin. I learned that he was the head of the Jewish community of Egypt and a great Aristotelian and that his most famous book is known as *Moreh Nebukim,* or the *Guide of the Perplexed.*

I was naturally interested to have such an important figure on which to hang our family pride, but the implications of the legend came to me with a profound shock. For the first time, I knew that I was Jewish, at least on my father's side. You may ask how it was possible for an intelligent boy like me to have any doubts about this when my grandmother Wiener as far back as I could remember had received a newspaper printed in what I knew to be

Hebrew characters. I can only answer that the world is complex, with ramifications not very understandable to an adolescent, and that it still seemed possible to me that there might be non-Jewish people in eastern Europe who used the Jewish characters. Furthermore, my cousin Olga had once told me that we were Jews; but my mother had contradicted this at a time when I had not yet learned to question the word of my parents.

At that time the social disadvantage of belonging to the Jewish group was considerably greater than it is now and there was definitely something to be said for allowing children to grow up through their early lives without consciousness of the social stigma of belonging to an unfavored group. I do not say categorically that this was the right thing to do; I merely say that it was a defensible thing to do and could be motivated—in fact, it was actually motivated—by a desire for the protection of the children. The moral responsibility of a policy like this is great. It is done nobly or it is done basely.

To put the best possible light on this course of action, it would be necessary that the children brought up in ignorance of the fact that they historically originated from the Jewish group be also brought up with an attitude of understanding. They should be made to see that the disadvantage on the part of others of belonging to such an unfavored minority group is an unmerited burden which they should at least abstain from intensifying. Such an attitude should be carried throughout the whole of life and should be directed equally against the unjust stigmatization of Jews, of Irishmen, of recent immigrants, and of Negroes, et cetera. Of course, the best thing and indeed the only one that would be thoroughly justifiable from a moral point of view would be to excite in the child a resistance, if not hostility, to all forms of belittling prejudice, no matter what the objects might be. However, short of this, every word by which the child's prejudice may be excited or intensified is a blow against his moral integrity, and ultimately a blow against his confidence and belief in himself when he should come to know, as he inevitably must, the truth of his own origin. The burden of the consciousness of Original Sin is hard enough to bear in any form; but a particularly insidious form of it is the knowledge that one belongs to a group that he has been taught to depreciate and to despise.

The responsibility for keeping the fact of my Jewishness secret was largely my mother's. My father was involved in all this only secondarily and by implication. I believe that he had originally intended not to burden us by the consciousness of belonging to an undervalued group, while at the same time he wished to preserve intact our respect for that group and our potential self-respect. He had written a number of articles on Jewish themes as well as a *History of Yiddish Literature*. He was also the first person to bring the name of Moritz Rosenfeld to the attention of the non-Jewish public. Father had been engaged in various negotiations with the Jewish Publication Society and with other similar Jewish organizations, and I gather these had involved considerable friction. Later I found that Father always claimed that the friction was the result of an arrogant insistence on the part of the Jewish organizations that a Jew was a Jew before he was a man, and that he owed inalienable allegiance to his own group before humanity itself. My father was always an individual, and was the last man in the world to stand pressure of this kind.

My mother's attitude toward the Jews and all unpopular groups was different. Scarcely a day went by in which we did not hear some remark about the gluttony of the Jews or the bigotry of the Irish or the laziness of the Negroes. It is easy enough to understand how these sops to the prevailing narrowmindedness of that epoch were thrown out by one who had experienced the disadvantages of belonging to an unfavored group; but though one can understand the motives leading to this conformist spurning of one's own origins, and can even forgive it in the sense in which the religious man hopes that his sin will be forgiven him, one cannot help regretting it and being ashamed of it. He who asks for equity must do equity, and it is not good for the children of a Jewish family, whether they know they are Jewish or not, to hear another Jewish family spoken of contemptuously for making the same efforts to pass over that their own parents have been making.

The maintenance of a family silence such as that which my parents maintained, even if it might be considered advisable, is much more difficult than it appears at first glance. If there is agreement that such a silence shall be maintained, what can one partner do when the other makes disparaging remarks about the race before the children? He can either terminate the secret, or he can be a silent and unwilling observer of a course of conduct that can lead to nothing but delayed emotional catastrophe on the part of the child. The vital danger of even the whitest of lies is that if it is to be maintained it must lead to a whole policy of disingenuousness of which the end is not to be seen. The wounds inflicted by the truth are likely to be clean cuts which heal easily, but the bludgeoned wounds of a lie draw and fester.

In offering the maximum apology which I can for the course of conduct adopted by my parents, I do not wish to justify it as a whole or to condemn it. I do mean to affirm that it had serious consequences for me. I was led very quickly into a rebellion against my parents and to an acceptance of their disfavor. Who was I, simply because I was the son of my mother and father, to take advantage of a license to pass myself off as a Gentile, which was not granted to other people whom I knew? If being a Jew was something to invite a shrug of the shoulders and a contemptuous sniff, then I must either despise myself, or despise the attitude by which I was invited to weigh myself with one balance and the rest of the world with another. My protection may have been well intentioned, but it was a protection that I could not accept if I were to keep my integrity.

If the maintenance of my identity as a Jew had not been forced on me as an act of integrity, and if the fact that I was of Jewish origin had been known to me, but surrounded by no family-imposed aura of emotional conflict, I could and would have accepted it as a normal fact of my existence, of no exceptional importance either to myself or to anyone else. Probably some conflict would have been excited by the actual external anti-Semitism which I found to belong to the times, and which would sooner or later have hit me one way or another. Nevertheless, unless there had been an ambiguity in the family attitude, this would not have hit me where it really hurt, in the matter of my own internal spiritual security. Thus the effect of an injudicious attempt to conceal from me my factual Jewish origin, combined with the wounds which I suffered from Jewish anti-Semitism within the family, contributed to make the Jewish issue more rather than less important in my life.

I say these things with the very clear and explicit intent to help prevent those who may be tempted to repeat this mistake of starting the child into the hurly-burly of life with this unnecessary sense of frustration and damnation.

Thus, when I became aware of my Jewish origin, I was shocked. Later, when I looked up my mother's maiden name and found that Kahn is merely a variant of Cohen, I was doubly shocked. I was not able to defend myself by the divided personality which allowed my mother's family to weigh strangers and their own kinsmen with different weights. As I reasoned it out to myself, I was a Jew, and if the Jews were marked by those characteristics which my mother found so hateful, why I must have those characteristics myself, and share them with all those dear to me. I looked in the mirror and there was no mistake: the bulging myopic eyes, the slightly everted nostrils, the dark, wavy hair, the thick lips. They were all there, the marks of the Armenoid type. I looked at my sister's photograph, and although she was a pretty girl in my eyes, she most certainly looked like a pretty Jewish girl. She had features not unlike those of a Jewish boy who happened to be my fellow lodger in my Cornell boardinghouse. He was a member of a recent immigrant family and appeared very foreign against the background of his Anglo-Saxon classmates. My snobbishness prevented me from accepting him fully as a friend, and the meaning of this was clear to me: I could not accept myself as a person of any value.

In this emotional and intellectual dilemma, I did what most youngsters do—I accepted the worst of both sides. It humiliates me to think of it even at the present day, but I alternated between a phase of cowardly self-abasement and a phase of cowardly assertion, in which I was even more anti-Semitic than my mother. Add this to the problems of an undeveloped and socially inept boy, spending his first long visit away from home, to the release from the immediate educational pressure of my father before I had developed independent working habits, and you have the material of which misery is made.

For I was miserable. I had no proper idea of personal cleanliness and personal neatness, and I myself never knew when I was to blurt out some unpardonable rudeness or *double entendre*. I was ill at ease with my associates in the middle twenties, and there were no youngsters of my own age to replace them. The habits of vegetarianism inculcated in me by my father had increased the difficulty of a social life away from home, and together with other people. Yet I was greatly under his spell, and because of my upbringing, even the remote threat of his powers made me loath to abandon these habits as my sisters were later to do.

My studies at home had always been under the close inspection of my father. This made it hard for me to develop good independent habits of study. I know that Father claimed to have always been in favor of my intellectual independence, and to have wished me to stand firmly on my own feet; yet whatever he purported to wish, the pattern of our life together worked in exactly the opposite direction. I had grown to depend on his support, and even on the support of his severity. To pass from this shelter to the full responsibility of a man among men was too much for me.

I took a rather divers series of courses my year at Cornell. I read Plato's *Republic* in Greek with Hammond and found that I had not lost too much of my Greek fluency in my Harvard year. I also attended the psychological laboratory and took a course with Albee on the English classical philosophers of the seventeenth and eighteenth centuries. Albee's course was dry but instructive, and I believe that there is an element still in my literary style which I owe to the rapid perusal of a large amount of seventeenth-century material.

I tried to take a mathematical course under Hutchinson on the theory of functions of a complex variable, but I found that it was beyond me. Part of the difficulty came from my own immaturity, but another part—to my way of thinking—from the fact that the course did not cover an adequate approach to the real logical difficulties of the subject. It was only later at Cambridge, when I found these difficulties faced boldly by Hardy instead of left to the student with an attitude of "Proceed and faith will come," that I began to find myself at home in function theory.

I did not fall down so badly in the Plato course, for this was but the continuation of my father's teaching under another preceptor. In my metaphysical and ethical courses I suffered from a new and vague adolescent religiosity (which did not last very long), and it needed a sharp logic to keep me from diffuse sentimentality.

I had to write essays for Albee on the seventeenth- and eighteenth-century philosophers. I was cramped by a boyish style, together with a physical awkwardness with the pen. My essays wound up in compact knots of words, so at variance with the norms of the English language that I was more than once asked whether my first language had not been German.

Cornell University had a philosophical periodical of its own, and one of the duties of the Sage Fellows was to write brief abstracts of articles in other philosophical journals to be included in a special section devoted to the purpose. The original languages were English, French, and German; and the exercise of translation familiarized us both with the philosophical vocabulary of these languages and with the ideas current in the world. I cannot vouch for the quality of our translations, but I have a very profound impression of the intellectual value of the work to ourselves.

There were some reliefs in this black year of my life. Although I could not fully share the companionship of my fellow students, the picnics up a neighboring creek and the winter sleigh rides which took place there after the fall of the snow were very pleasant indeed. There were one or two undergraduates in my boardinghouse with whom I had good times and bull sessions, and they used to play childish pranks on me and on one another. The scenery of the campus was gorgeous and later, when spring came around, the plantations of flowering quince were beautiful beyond anything I had seen on the Tufts College campus or elsewhere. There were sailboat rides on Cayuga Lake and tramps to neighboring waterfalls, where we swam and bathed under the plunging masses of water.

I have carried down to the present day the friendship of more than one of my fellow students. Christian Ruckmich, a gaunt, Lincoln-like figure, was my partner in many of my walks and in the psychological laboratory. I have heard from him from Abyssinia within the last few years. He has been engaged there in reforming the educational system of the country, and his boy has taken up aviation.

There was also Tsanoff, the Bulgarian, whom I have seen within the last year or two at the Rice Institute, still teaching philosophy. There was a delightful couple by the name of Schaub with whom I often lunched. Schaub taught a course on comparative religion, and his discussion of the Old Testament fitted in very well with the philological interest which I had acquired from my father, from Professor Wade at Tufts, and from browsing around our library.

As the year went on, it began to appear that I had not earned a renewal of the fellowship which had taken me to Ithaca, or at any rate if I were to receive it again, it would be by special grace. I felt oppressed, not only by my indifferent success in my courses, but by that sense of adolescent guilt that accompanies the internal sexual development of almost every normal young person. My sense of guilt led me to avoid the Thillys, and this alienation ended in a quarrel between my father and Professor Thilly. It was almost impossible to make my father believe that one of his family could be at fault. It was even more impossible for me to stand up to the withering stream of invective which a discussion was bound to bring down upon my head.

Before the end of the year there was other news from home. I had a new brother, a sickly child, who scarcely lived out the year. With the bad news from Cornell, my father snatched me out of the Sage School and forced me to transfer to the department of philosophy of the Harvard Graduate School. I know that the responsibility of my family made it difficult for my father to back a doubtful bet, but I nevertheless wish that as a young man I had received the opportunity to redeem an error in the place in which I had made it. The result of Father's policy of transferring me was to increase my lack of self-confidence, which was already great enough. My blunders became a menacing sequence of dead years never to be undone. Meanwhile I did not have the opportunity to learn the arts and techniques of independence, and the future was for me a turbid and depressing pool.

I had time after I came home to take stock of my moral situation. My achievement of independence during the year at Cornell had been incalculably retarded by the confused mass of feelings of resentment, despair, and rejection which had followed early in the year upon the discovery of my Jewishness.

Some of my friends have asked me to render more specific the discussion of the shock which I experienced and the subsequent adjustment which it was necessary for me to go through in order to be reasonably at peace with myself. Manifestly to be at once a Jew and to have had inculcated in me by certain of my mentors hostile or depreciating attitudes to the Jews was a morally impossible position. It might have led me into a continued Jewish anti-Semitism or, on the other hand, to a flight into Abraham's bosom.

In fact, neither of these escapes was possible for me. I had received too strong a lesson in intellectual and moral integrity from my father to be willing to accept one brand of justice for myself and my near kinsfolk and another brand of justice for the outer world. I had heard enough harsh remarks at home concerning other university families of Jewish origin who had tried to escape from Judaism to realize that there were those close to me who weighed the Wiener family with one scale and the rest of the world with a very different one. Quite crassly, even if I myself and some of my immediate family had been willing to deny my Jewish origin, this denial would not pass current one foot beyond our doorstep.

In short, I had neither the possibility nor the wish to live a lie. Any anti-Semitism on my part must be self-hatred, nothing less. A man who hates himself has an enemy whom he can never escape. This way there lay only discouragement, disillusionment, and in the end madness.

Yet it was equally impossible for me to come into the fold of Judaism. I had never been there, and in my entire earlier education I had seen the Jewish community only from the outside and had the very vaguest idea of its rites and customs, its permissions and obligations. The break with orthodox Judaism had indeed begun in my grandfather's time; in pursuit of his desire to Germanize the eastern Jews and to replace Yiddish by High German, my father had been sent in part at least to a Lutheran school. Thus a return to Judaism on my own part would not be a true return, but a fresh conversion and conviction. For better or worse, I do not regard conversions of any sort with very favorable eyes, nor did my father. There is something against the grain in the attitude of abnegation and of denial of personal judgment in the wholesale acceptance of any creed, whether in religion, in science, or in politics. The attitude of the scholar is to reserve the right to change his opinion at any time on the basis of evidence produced, and I was born and bred to the scholar's trade.

This training of mine went very deep into my nature. I have never had the impulse to gregariousness in my thinking and feeling despite all my very deep respect for man as man, whether he be a scholar or not. It was emotionally impossible for me to hide myself in the great majority as a fugitive from Judaism; but it was equally impossible for me to hide myself and be consoled in a restrictedly Jewish community. I could not believe in the old-line New Englanders as a Chosen People: but not even the vast weight of the Jewish tradition could persuade me to believe in Israel as a Chosen People. The one thing that I had known about my father's relation to Judaism was that he had been an assimilationist, rather than Zionist, and that he had had many arguments with Zangwill and others on this issue. This was a position which I approved not only because he was my father but also because I thought he had seized the stick by the right end.

Thus I was powerfully moved by the discovery of my Jewishness, but I could see no way out in anti-Semitism or in ultra-Judaism. What, then, could I do?

I cannot tell when I arrived at an answer to my problems, for the solution occurred step by step and was not reasonably complete until after my marriage. Yet one thing became clear very early: that anti-Jewish prejudice was not alone in the world but stood among the many forms in which a group in power sought, whether consciously or unconsciously, to keep the good things of the world for itself and to push down those other people who desired the same good things. I had read enough of Kipling to know the English imperialist attitude, and I already had enough Hindu friends to realize how bitterly this attitude was resented. My Chinese friends spoke with me very frankly concerning the aggressions of the Western nations in China, and I had only to use my eyes and ears to know something of the situation of the Negro in this country, particularly if he aspires to be something more than a farm hand or an unskilled worker. I was quite adequately informed concerning the mutual bitterness between the old Bostonian and the rising Irish group which demanded its own share of power in the community and took a very liberal view of what that share should be when other immigrants and minor groups came into question.

The net result was that I could only feel at peace with myself if I hated anti-Jewish prejudice as prejudice without having the first emphasis on the fact that it was directed against the group to which I belonged. I felt anything less than this as a demand for special privilege by myself and by those about me. But in resisting prejudice against the Oriental, prejudice against the Catholic, prejudice against the immigrant, prejudice against the Negro, I felt that I had a sound basis on which to resist prejudice against the Jew as well. For a long time I had been interested in my fellow students from the Orient and from other foreign countries, and I now saw their problems as parallel to my own and, in many cases, far deeper and more difficult.

Moreover, when I heard of our reputed descent from Maimonides, I realized that even deeper than our simple Jewishness, in a sense the Orient was part of our own family tradition. Who was I, a man whose proudest ancestor had led a life in a Moslem community, to identify myself exclusively with the West against the East? Thus I came to study and to observe parallelisms between the intellectual development of the Jews, especially in that interesting period of transition which began with Moses Mendelssohn and led to the integration of Jewish learning with European learning in general, with similar phenomena taking place before my eyes among the non-European men of learning. This came to an even sharper focus later on when I spent part of a year assisting Professor Hattori, a Japanese professor at Harvard, in the routine work of his course on Chinese philosophy.

This covers the intimate personal side of my reaction to my discovery that I was of Jewish origin. It may be well, however, to add a few facts concerning anti-Semitic prejudice and its history in those communities in which I have lived since my childhood. It is fairly clear from the history of those Jewish families who came to the United States before the middle of the last century that anti-Semitic prejudice was not a considerable factor in their lives. As a matter of fact, the dominating Protestants in the United States were more than ready to acknowledge the extent to which they had drawn upon the Old Testament in their writings and thought and to see in the Jewish immigrant a reflection of their own traditions. I have been told that even the “Know Nothing Movement” was not particularly anti-Semitic and, further, that some of the leaders of this unsavory episode in our history were drawn from the Jews themselves. Be that as it may, the beginning of the twentieth century saw the blunting of our national resistance to anti-Semitism as it saw the blunting of New England’s traditional friendship for the Negro and of many other of the broader attitudes of earlier days. The Gilded Age had already come to an end and had left as its heir the Varnished Age.

12 Problems and Confusions

Summer, 1911

That summer we spent at a farmhouse not far from Bridgewater, New Hampshire. There was only one small mountain in the immediate neighborhood, and it was too rough and trailless for my father to permit me to climb it. I tramped the roads of the neighboring countryside in search of summer camps where I might earn a little money as a teacher, and find a little companionship in the bargain, but my services were not in demand. I pitched hay in a local field and fell desperately ill under an allergy to haydust. I read back numbers of *Harper's* and *Scribner's Magazines* and the *Century,* and I longed for the beginning of term to relieve me from the boredom which came from a family living too close together and driven in upon itself.

My father's revolutionary theories of education were confirmed in his eyes by the success which, with all my shortcomings, I had already found in intellectual work. It soon became clear that my sisters, although very clever girls by any ordinary standard, were not responding to my father's training as I had. And in part, my father did not expect as much of them. This was laid to their being girls, unable to stand up to the severe discipline to which I had been subjected.

Our family portioned out the fates of the family members in advance. The expectation that my sister Constance was to be the artistic one made my parents assign music, painting, and literature to be her field. To prevent any contretemps, they were to be strictly eschewed by the rest of us.

Thus Constance, and, in a similar way but later, my sister Bertha too, was removed from the sphere of intellectual competition into which I had been initiated. Occasionally I indulged in a certain envy of their easier fates, and there were times when I would have considered it a privilege to be born a girl and to be faced no longer with a need for the hard work of intellectual effort, and the ultimate requirement of standing alone in a world I felt to be hostile.

The case of my brother Fritz was of course a very different matter from that of my sisters. It was not until I was a graduate student at Harvard that he had reached the age where his education severely impinged upon us. He was destined by my parents for the same career of scholarship as I. This time there was no question of weaker demands on the weaker sex, and my father's educational theories had to be faced in their full significance. My father had reiterated that my success, if indeed I had had any genuine success, was not so much a result of any superior ability on my part as of his training. This opinion he had expressed in print in various articles and interviews.[1] He claimed that I was a most average boy who had been brought to a high level of accomplishment by the merit of his teaching and by that merit alone. When this was written down in ineffaceable printer's ink, it had a devastating effect on me. It declared to the public that my failures were my own but my successes were my father's.

Now that my brother had come of school age, there was a second Wiener candidate for fame and distinction, and for the upholding of the judgment of my father. It was inevitable, and it had been made publicly inevitable, that my father should try to duplicate with his younger boy what he had already accomplished with me. It became almost as inevitable that the anticipation of Fritz's success was to be thrown in my face in order to deflate me and to exalt the authority of my father.

I never agreed with Father in his estimate of me as a boy of average abilities, which I always felt had been adopted to curb my self-conceit and cut me down to family size. It was not fair to expect a priori that Fritz could do what I had done. Furthermore, Father did not take into consideration the fact that although I was a nervous and difficult youngster, I had plenty of stamina, and could absorb without utter destruction a punishment far greater than that which the average child could take. Thus when my brother turned out to be a somewhat frail child, endowed with what I believe to be good average ability but without any exceptional powers, the scene was set for trouble.

The bickering about Fritz's education lasted for well over twenty years. I resented as unfair the extra weight which my parents threw into the scales to equalize the balance between my brother and me. I was also very much displeased with the role that I was given at sixteen as my younger brother's mentor and nursemaid, taking him to primary school every morning before beginning a full day's work. I was expected to display to him a companionship rarely to be shown by a lumbering adolescent to a mere child eleven years younger. This age difference was critical. When I was sixteen, he was five; when I was twenty-five, he was still only fourteen.

In defense of my parents' expectations of my relationship with Fritz, it is necessary to remember that the world was changing even during that period before the First World War when I was growing up. When I and my older sister had been young children, not even the relative poverty of the family had kept my mother from having the assistance of at least two servants, one of whom had been a cook and the other generally an excellent nursemaid. The changes of the century had already begun to dry up the stream of immigrant household labor, and wages had risen sharply. Not even our greater prosperity could make up for the new conditions and re-create a class of labor that had almost ceased to exist. Thus the care of the younger child fell on the older one.

Looking back on the matter from my present point of view, I cannot blame my parents for passing on to me a responsibility which they had taken so readily in the care of the elder children, yet the circumstances of my responsibility were not fair to me. My duty to Fritz was a deputized one, completely unaccompanied by authority. Fritz was tiresome under my tutelage, and if I took any measure, no matter how mild, to make him behave, he had only to complain to our parents. Whatever step I might have taken was inexcusable to them. Furthermore, I was a confused, socially inept adolescent, who by any reasonable standard had been overworked for years and who needed every available moment to develop his social contacts and his social poise.

It will be no surprise that my companions, whether boys and girls, whether men and women, were judged more critically by my parents according to whether they accepted or did not accept Fritz than on any other point whatsoever. This, too, was unfair to me. It is too much to ask young people to take as a friend another youngster who always has an infant brother toddling after him, particularly when he has no authority over his brother and the child knows this. Thus there is plenty of explanation, if not excuse, for the fact that I was often harsh if not cruel to my brother. Irony and sarcasm are the weapons for those who have no other weapons; and these I did not spare. The difficult situation grew even more difficult.

To a certain extent, too, I was given the responsibility but not the authority for Fritz's education. Fritz rapidly developed a vocabulary of the intellectual, far beyond his understanding. In the competition within the family, he tried to hold his own by asking questions of a learned sort, with answers he did not fully understand and in which he had no deep interest. I was told that I was to answer these questions in detail, even when they had ceased to have an interest to Fritz and when his mind was wandering elsewhere. When the family went to the theater together, I was supposed to offer a commentary on whatever features of the show had excited my brother's desire to display his intelligence, and I did not have a decent opportunity for the reflections on it which belonged to me for myself and for discussions with my true intellectual contemporaries.

Of course, in all this I am going well beyond the period of my history that I have made the subject of this chapter and I am giving an account of a festering sore which continued to infect our family life. During a considerable part of this period I was living at home, either as a minor or as an adult paying his contributions to the family fund. It may be asked why I did not leave the family to take lodging somewhere for myself, perhaps even in Cambridge. Many times I was on the point of doing so, and many times my parents indicated that if I continued my course of conduct, this would be the inevitable result. However, it was made dear, particularly by my mother, that the separation would be held against me for all eternity, as a sign of my ultimate failure, and would mean the complete and final collapse of family relations.

During the earlier period of my life at home, I was made aware that I was completely dependent on the family bounty, and that such sums as I gained by scholarships were only a partial offset against this. Later, when I had acquired the ability to earn my own living, I had still not acquired any circle of friends outside the family. Thus, while separation from the family might have been desirable, exile from the family was exile into outer darkness.

Those who read further in this book will see that my summers were marked by long mountain excursions for many years before my marriage. Later on, these excursions were replaced to some extent by trips to Europe, often together with my sisters. These gave some alleviation of the pressure of family life, and in particular of my forced custodianship of my brother, and were absolutely essential to my well-being. However, my parents made every attempt to compel me to accept Fritz as a companion on my mountain hikes. This was inequitable, and represented a demand that I could not accept. This had not been the first time that the rather patriarchal structure of the family had disturbed me. Once, in my youth, my father had planned to join with me in making a museum collection of the fauna and flora of the Old Mill Farm neighborhood and had proposed that we spend a large part of our spare time in maintaining this collection. Once he had spoken of his intention, when Constance and I had grown up, to devote the rest of his life to the conducting of a children's school on his own principles, in which my sister and I were to be teachers. More than once he had talked of returning to the romantic adventure of his youth, and of crossing the continent with us in a covered wagon. All these projects were admirable as indications of his youthful spirit, and would have been most charming suggestions of paternal love and family interest in a household less strictly under parental control. As things were, they represented another turn of the screw.

The summer always found us raising a garden, and I was relegated to the tasks of weeding, thinning asparagus, picking peaches, and the like. These were light tasks, and would have been most agreeable if they had not represented a simple outdoor extension of the regime of my filial servitude. I was clumsy and I was inefficient and I was lazy; and I had to hear these faults ding-donged into me hour after hour as I worked by my father's side in the fields. I was quite as unsatisfactory a farmhand as my father indicated, and I certainly developed a repugnance for work in the fields. This has lasted to the present day, and has hampered me at a time when my diminishing stock of muscular vigor would otherwise make me welcome light garden work as a still admissible form of bodily activity. Nevertheless, so long as my father's mentorship continued to dictate my way of life all winter, it was intolerable that the summer, which I needed badly for recuperation and the formation of new social contacts, should merely be an extension of the winter regime.

Indeed, at a later period after the First World War, when father had sold the Sparks Street house as too large for a family no longer needing growing space, he put the money not only into a smaller and older house on Buckingham Street, but into an apple farm in the town of Groton, Massachusetts. He had hoped that the whole family would co-operate in this work, at least during the apple-picking season, and that the place would furnish in return a cool summer for the married children and the expected grandchildren. The scheme was bound to fail from the beginning. Young people in their early twenties have to consider the pressing necessity of their own social life, and cannot long be denied the opportunity for seeking and meeting their prospective mates.

Note

1. An article entitled "*New Ideas in Child Training*" by H. Addington Bruce, published in *The American Magazine* in July, 1911, quoted my father directly:

> I am convinced that it is the training to which we must attribute the results secured with them. It is nonsense to say, as some people do, that Norbert and Constance and Bertha are unusually gifted children. They are nothing of the sort. If they know more than other children of their age, it is because they have been trained differently.

13 A Philosopher Despite Himself

Harvard, 1911–1913

I returned to Harvard as a candidate for a Ph.D. in Philosophy in September, 1911, when I was nearly seventeen years old. The period between 1911 and the completion of my doctorate was the period of great names in the Harvard philosophy department, and although William James was dead, Royce, Palmer, Münsterberg, and Santayana were alive and active.

In my first year I took a course with Santayana. I remember very little of its content but a considerable amount of its atmosphere. The feeling of a continuity with an old culture and the feeling that philosophy was an intrinsic part of life, or art, and of the spirit, gave me a great deal of satisfaction; and yet, after the passage of all these years, I cannot put my finger on any definite idea which that course has given me.

Palmer's courses mean as little to me in retrospect. They were reading courses, and as far as I remember, they covered the traditional philosophy of the English school. What I do recollect of Palmer was his grave, sweet-tempered personality, a little bowed down by the weight of years, but still eager to encourage the ideas and to allay the natural timidity of the young student.

Ralph Barton Perry was among the chief of those to welcome me on my father's behalf as a student. He and Holt, the psychologist, were two among the five or six authors of a then popular manifesto known as the "new realism." It contained the combined echoes of James's pragmatism with a certain analogy with the work of Bertrand Russell and G. E. Moore in England, and represented a protest against that idealism that dissolves all things into mind and the phenomena of mind. It made a rather plausible case for itself, yet I remember that its chief impression on me was of an intolerable shoddiness and brashness. One of the authors went so far as to try to base his ideas on a mathematical logic in which every other word represented a confusion of misunderstood terms. The literary style of the composition was sophomoric. Nevertheless, I remember Holt as a brilliant and charming personality, as a fluent dialectician within the frame of his seminar, while Perry has remained one of the great and dignified figures of American liberalism.

I had two different sorts of contact with Josiah Royce. One was in his course on mathematical logic. Although I did not regard his own contributions to mathematical logic as of major character, he introduced me to the subject. Royce was a many-sided man, coming at a critical period in the intellectual world when the old religious springs of philosophical thought were drying up and new scientific impulses were bursting into life. His mathematical logic bore the signs that almost always indicate a brilliant man who has come too late into a new field to obtain perfect mastery of it.

This position of facing both the past and the future was also clear in Royce's seminar on scientific method, which I attended for two years, and which gave me some of the most valuable training I have ever had. Royce welcomed into this little group every sort of intellectual who was carrying out a reasonable program of work and who was articulate concerning the methods by which he had come to his own ideas and concerning the philosophical significance thereof.

The group was heterogeneous to say the least. Among us was a Hawaiian expert on volcanos. He has left on my memory only the impression of the two words *pahoehoe* and *aa,* which I understand to be the designation of two types of lava. Among us was also Frederic Adams Woods, the author of *Heredity in Royalty,* a eugenicist of a snobbish cast of mind and a pregenetic point of view. Percy Bridgman, who was even then beginning to be skeptical about the elements contained in experiment and in observation, and who understood the influence on physics of James's pragmatism, was definitely veering toward the operational position which he later assumed. The first head of Boston's Psychopathic Hospital, Southard, spoke interestingly on the problems of psychiatric method. There was also Professor Lawrence J. Henderson, the physiologist, who combined some really brilliant ideas about the fitness of the environment with what seemed to me a distressing inability to place them in any philosophical structure, and whose pomposity of manner was not diminished by the article of faith that led him to place a great business administrator in the scheme of things about halfway between a pure scholar like himself and the Creator. Incidentally, I found that those who undervalue their own profession of learning are rarely those who rise to the greatest heights in it.

I believe that it was in this seminar that I first met F. C. Rattray, an Englishman, later to become a Unitarian clergyman and to occupy the pulpit of a church in the English Cambridge. At that time it was Rattray more than any of my official teachers who showed me what good dialectic was, and to what fineness the art of class discussion could be carried.

I have never seen a man as able to prick a bubble in the froth that often attends such arguments. Nevertheless, I could not help feeling that his devotion to Samuel Butler and his life force, after the fashion of Bernard Shaw, was more a matter of personal emotion adroitly defended by a keen wit than something susceptible to rigorous argument. Rattray and I often used to join forces in the seminars in which we participated, and I am afraid that I became an apt pupil of his and a thorn in the flesh of my mentors.

I also attended Münsterberg's seminars. He was a most puzzling personality. How much of his arrogance was a covert contempt for the America in which he was teaching, and the result of comparison of it with the Germany in which he had failed to find permanent lodgment, we shall never know. His transpontine personality was curiously modeled after that of the German Kaiser, and in my opinion was not a little expressive of the unsureness and brusque assertiveness which went as a false streak through many different social layers of the powerful and able Second Reich. Whatever his private opinion of the America which he adopted and which had adopted him, he had become the master of one of its best rewarded arts: that of personal publicity. His portentous interviews were given a much more intriguing tone by the heavy foreign accent and the slightly foreign phraseology in which they were presented, and Münsterberg became the joy of the reporters.

I learned the mathematical aspect of my philosophy from Professor E. V. Huntington. He was an old friend of my father, and had visited us when we were living in Old Mill Farm in the town of Harvard. I remember that at that period, before I had been graduated from high school, Huntington had tried me out with a little analytical geometry, and had shown me the theory of the nine-point circle.

Huntington was a magnificent teacher and a very kind man. His exercises in postulate theory were all educational gems. He would take a simple mathematical structure and write a series of postulates for it for which we were to find not only examples satisfying the complete list, but other examples failing to satisfy it in just one place or in several specified places. We were also encouraged to draw up sets of postulates of our own. Both Sidis and I were in this class, and it was here that I first became aware of the boy's real ability and of how great a loss mathematics suffered in his premature breakdown.

Huntington's career has always remained a mystery to me. With his keenness and his inventiveness I should have expected some great mathematical contribution from his pen. Nevertheless, all his pieces of work, no matter how much they may contain of ideas, have remained miniatures and vignettes. I do remember one larger piece of work of his in which he attempted to give a basis to plane and solid geometry; but on the one hand, the work did not go greatly beyond the slightly earlier efforts of Hilbert, and on the other, some of his chief ideas had already found a representation in the work of Whitehead. The valuable and honorable career of Huntington seems to me to bear the lesson that one of the most serious possible lacks in mathematical productivity is the lack of ambition, that Huntington simply set his sights too low.

Let me say a word or two about my amusements during these years. During my long mountain tramp with my father in 1910, I had become acquainted with the excellent work done by the Appalachian Mountain Club in the maintenance of trails in the White Mountains. I joined the club in the fall of 1912, and I got a large part of my exercise in its Saturday walks. A group of us, distributed in age and sex, but all of us devoted trampers, were in the habit of foregathering at one of Boston's railroad stations for a suburban train trip and an afternoon's brisk walk in the country.

In 1912 I had obtained my M.A. It did not represent any particular stage in the voyage leading to the Ph.D. but it was convenient to have in case I should meet any obstacle the next year. I had also passed, as I have said, the preliminary examinations on a variety of topics, and they threw me into somewhat closer contacts with my fellow students than I had found already.

Among other things, this was the year of the *Titanic*. It represented a shock to our emotional security which was a fitting introduction for the great shocks to follow. It was perhaps this event rather than the beginning of the First World War two years later that awakened us children of the long peace that had so long protected Europe and America to the fact that we were not the favored darlings of a beneficent universe.

Besides my usual reading from Dumas and Kipling, who were the delights of an adolescent boy, I added to the list of the books that I particularly enjoyed. Swift is no favorite of the very young, even through the veil of bowdlerization which surrounds the carefully expurgated editions to which they have access. But as a boy grows up, he finds the bitter draft of satire a strong and manly tonic, and I came to enjoy Swift, even though I shuddered as I read him. I also came to enjoy the milder but still vigorous vein of Thackeray, and to forgive or even to delight in his long-windedness. But above all satirists, I came to love the heart-rending cries of Heine, in which not one word is missing or in excess to obscure his love and his venom. I know, as did my father, almost every word of his *Hebraische Melodien,* and there are no poems that can move the Jew in me to greater pride or agony.

These books I read, not once, but many times over, lying face down on my bed, and sucking the last savor out of the phrases I had scanned many times before. I have never been a great reader of new things; but what I have read and loved, I have taken into my memory, so that it has become a part of me, never to be discarded.

In the same way, I relearned my Latin and my Greek. The lapidary poetry of Horace is not merely something buried between the pages of my schoolbooks: it is engraved on the tablets of my memory. The sweep and grandeur of Homer are recollections which I can never forget. I may not be much of a classicist in the technical, competitive sense, but the roots of a classical education are firm in me.

At this time, my sister received a copy of Ruskin's *Modern Painters.* I read it avidly, and I thoroughly enjoyed Ruskin's rather academic drawings and the magnificent poetry of his language. Although I found his ventures into what might be called quasi-science dogmatic and incorrect, I could not help paying tribute to his superb talents as an observer. The book was my introduction to an appreciation of painting, sculpture, and architecture, but my later experience has taught me that though it is a brilliant commentary on the arts, there is a certain willfulness in Ruskin's prejudices, and that his study needs to be supplemented with a direct acquaintance with the great works of art and by a more catholic attitude toward the arts of the non-European countries.

The summer of 1912 we returned to the town of Sandwich and, to our delight, settled in the little interior valley at the foot of Flat Mountain and Sandwich Dome. The house we rented was known as the Tappan Place. Our next-door neighbors were the happy-go-lucky family of a Cambridge banker, ranging in ages from less than ten to the middle twenties. With one exception they were girls. They were great trampers, and with my own newly excited interest in climbing, we shared many quick jaunts up and down Sandwich Dome and Whiteface. I found the girls attractive, and was particularly taken by the one who was nearest my own age. Although I do not believe that I ever avowed my own admiration for her, there is probably still standing behind our house more than one beech tree that bears the marks of my jackknife.

I continued to tramp the woods with my father, but it was already clear that as my vigor increased, his had commenced to wane. The mixed pleasures of a heavy pack and a night on a balsam bed were no longer for him.

I had decided to work with Royce for my Ph.D. the next year in the field of mathematical logic. However, Royce's health had broken down, and Professor Karl Schmidt of Tufts College consented to take his place. Schmidt, who, I later learned, was a summer neighbor of ours in New Hampshire, was then a young man, vigorously interested in mathematical logic rather than the religious philosophy which later became his field during his tenure of office at Carlton College. Schmidt set me as a possible topic a comparison between the algebra of relatives of Schroeder and that of Whitehead and Russell. There was a lot of formal work to be done on this topic which I found easy; though later, when I came to study under Bertrand Russell in England, I learned that I had missed almost every issue of true philosophical significance. However, my material made an acceptable thesis, and it ultimately led me to the doctor's degree.

Schmidt was a patient and understanding teacher with Huntington's quality of being able to bring a young man to intellectual productivity by easy stages. If I had not had gentle handling that year, I do not believe that I could have come through it unmarked, for besides my dissertation, there were two ranks of dragons awaiting me.

The first mild fire-dragons were my topical examinations, which were written. Behind these loomed the fiercer dragons of my oral examinations. I passed through my topical examinations with my head bloody but unbowed. One incident in connection with them does not redound to my credit. All of us who had taken the examinations were very curious about our grades, and we found a compliant janitor who had access to the professors' rooms and the papers on which the grades lay. I regret to say that I teased him into telling me what these grades were, and I let at least one other candidate into the secret. It was a mere gesture of misjudged curiosity and misjudged good will, and bribery took no part in it, though I was accused of such a bribery later on.

I dreaded the oral examinations far more than the topicals. I went around to the houses of my various professors to take them. In every case the professor was kind and obliging, and in every case I went through the examination in a sort of trance scarcely understanding the words said to me. With Professor Woods, who examined me in Greek Philosophy, I found that I had forgotten almost every word of Greek that I had ever known, and was scarcely able to construe the simplest passage of Plato's *Republic*.

I must give my father every credit of seeing me through the great ordeal of the oral examinations. Every morning he went for a walk with me to keep up my physical condition and to reinforce my courage. Together we walked over many parts of Cambridge which were as yet unknown to me.

He would ask me questions concerning the examinations that were ahead and would see to it that I had a fair idea as to how to answer them.

Nevertheless, at my own valuation, I should have failed every examination; but examining professors for the doctorate are likely to be more human and sympathetic with the student than the student is with himself, and to give him the benefit of every doubt. The terror of the student is familiar to all examining professors and is a part of the normal environment of the examination for which they automatically compensate, so that no doctoral examination is taken at its full face value but is always interpreted in the light of such other data as the professor may have concerning the student's ability.

I have often been an examining professor myself in the course of my duties at the Massachusetts Institute of Technology. I have learned that terror deserves sympathy and is pardonable and indeed normal; and although the attempt to exert ingenuity on the spot and to lift oneself out of a difficulty while standing before the examiner is commendable, bluff is inexcusable. It is not the timid student but the glib yet ununderstanding student who has the most to fear.

After my oral examinations on specific subjects, I proceeded to the last part of the gantlet that I was to run, which consisted of my examination on my doctoral dissertation before the entire group of Harvard philosophers. This examination is theoretically the most critical stage in the ordeal of the candidate for the doctor's degree. But in fact no well-regulated department will permit a candidate to proceed so far unless it is substantially sure that he is going to pass. Furthermore, the candidate has now the tremendous advantage of reporting on a subject in which he is theoretically better informed than any of his examiners, so that there is no excuse for an honest man to be terrified, except by being tongue-tied and timid; and this, as I have said, is what his professors are most ready to condone. In fact, the oral examination for the doctorate is much more an examination in the student's conduct before a class than in anything else, and exerts a considerable amount of influence in the selection of those students who are going on to preferred academic positions. Here I do not believe that I did particularly well.

In the period between my examination and the last date duly allowed by Harvard statutes, I copied my dissertation on the typewriter in such a form that I might with decency embalm it in the Harvard archives. Some of this work was done at home, and some under the gaze of the portraits of former Harvard worthies in University Hall. Harvard did not at that time require the publication of a doctoral thesis. I am convinced that the Harvard attitude was sound, for it is unjust to accept the judgments of periodical editors as the criterion by which to give a university degree. Moreover, the forced private publication of such theses as do not meet the requirements of the editors of scientific journals places a heavy tax on the pocket of the student without doing any corresponding good to the profession at large. Privately published, these are universally inaccessible and, in general, little read. I am glad that the requirement of publication of the thesis is gradually disappearing.

It is often supposed that a man's doctoral thesis should be one of the best things he ever does, and should give the full measure of the man. I do not believe in this. A doctor's thesis is nothing but a specific piece of work by which a journeyman qualifies himself to become a master of his craft; and if he does not exceed this level a dozen times in the course of his career, he is a very poor master indeed. I know that many believe that the dissertation should stand out for years above a candidate's subsequent work, but this demand is often ignored in practice. It is only when a man has his dissertation behind him and is not pestered with the prospect of future formal requirements to fulfill that he can do his best work as a free man, with his task itself as the goal and not the spurious goal of a certain academic and social position. The thesis should be good, but if the scholar's work does not soon exceed the level of the thesis, the candidate is well on his way to becoming one of those desiccated homunculi you find in faculty meetings of our third-rank colleges.

If my own dissertation had been the only piece of scientific work I have ever produced, it would have been a most unsatisfactory ticket of entry to a career of learning. However, as the facts have developed, it did give me the training in the organization of scientific material which led me in the next two years to a series of papers that I should much prefer to represent my induction into a scholarly career.

I have known more than one student who has waited to present a thesis for his doctorate, even after he has produced a number of acceptable papers, until he can write that one paper that will allow him to break into print in the learned world with a maximum of impetus and *élan.* It is of course a fine thing if a youngster can establish himself as an important figure with his first work. Nevertheless, I feel that many a student has placed too much emphasis on this point, and has wasted years waiting for the great idea to come to him, which he might have devoted to experience in publishing and in receiving the public criticism of his printed work. It is altogether too much to expect to become a great man on the first try; and if the course of one's late work contains such material as need not be a matter of shame, it makes very little difference whether the first paper is excellent or barely conforms to the necessary standards for the doctorate.

Time hung heavy on my hands toward the end of the spring of 1913, after I had passed my doctoral examinations and while I was still waiting for Commencement and the apotheosis of hearing President Lowell declare that I was duly admitted to the society of learned men.

The year 1912–1913 was that of the demolition of the old Harvard library and preparation for the construction of the new building given by Widener. The old building had only been adapted for library purposes by a continuous series of improvisations and interior reconstructions, so that although it was probably one of the most authentic specimens of early American academic Gothic, its time was over and it had to go. There was a certain Roman holiday pleasure in seeing its pinnacles and vaults demolished by a great swinging iron ball; and indeed, the original workmen must have done the work so honestly that at times even this sort of violence made very little headway. The noise was intolerable, and our philosophical classes in Emerson Hall were conducted to an obligato of donkey engines and crumbling walls.

Yet with all the progress that the destruction of the old building signified, we felt that it marked the passing of an age. Nevermore would the library convey to us its anachronistic suggestions of medievalism, and the ample lawns about it were to be cut up forever by the crowding bulk of the Widener Library. My father always felt in view of his familiarity with the stacks, and the work that he did there, that the *d* in Widener might well be dispensed with. Be that as it may, the great convenience of the Widener Library as a storehouse for books never seemed to me to be matched by any particularly endearing qualities in its architecture. It was a cold and forbidding building, and later on during the war years the great stairway was decorated with two cold and forbidding paintings of the military might of America. They were Sargents, but they certainly weren't top Sargents.

At that time one of the places where I was most often to be found was the Harvard Philosophical Library. It was a pleasant place, and the librarian, Dr. Rand, was the exact Harvard equivalent of the slightly desiccated English don. He was an excellent historian and bibliographer, so that it was always interesting to search through the bookcases for something new and exciting. For example, we found that a number of the books which had been left to the library by William James were full of autographic marginal notations by James himself, which were rather less decorous than they would have been if they had originally been intended for public scrutiny. The James copies of the books of Royce and Bertrand Russell were particularly amusing. When Rand found out that the books were really priceless treasures, he locked them up in his private case and removed them from free public inspection.

Even after this there were many treasures which we found interesting reading. The English philosopher, F. C. S. Schiller, notwithstanding the lack of any great profundity on his part, had a pretty satirical vein which was always amusing to us. Then too, one never knew what was to be found among the newly arrived books and periodicals. Thus a casual leafing through these was part of our weekly routine.

During the course of the spring, I began to look at a number of educational periodicals in order to satisfy my curiosity about their treatment of the infant prodigy. This curiosity received a sharp punishment when I found, in a journal edited by G. Stanley Hall of Clark University, an article by a Miss Katherine Dolbear, the daughter of the late distinguished physicist of Tufts College.[1] This article was devoted to a discussion of Berle, Sidis, and me, case by case and name by name. Miss Dolbear was obviously not impressed by our record. She had presented with meticulous precision, not merely our official records at our several schools, but everything that she had been able to gather as to the opinion held of us by our undergraduate companions.

In no aspect was it a gratifying document. I had long been aware that my social development was far behind my intellectual progress, but I was mortified to find how much of a bore, boor, and nuisance Miss Dolbear's record made me out to be. I had thought that I was well on the way to the solution of my problems. Miss Dolbear's article made me feel like the player of parchesi whom an unfortunate cast of the dice has sent back to the beginning of the board.

I showed the article to my father, who was as furious as I had been humiliated. Father sent a letter of protest to be published in the next number of the *Pedagogical Seminary,* although this did not serve any particular end. Our family lawyer was unable to give us much satisfaction in the matter. An attempt to seek a legal remedy would have subjected me to publicity far more dangerous and vicious than anything to which I had yet been exposed. Even in theory American law does not take great cognizance of the right of an individual to privacy, and a libel action, in order to be successful, must allege some specific damage as the result of the libel. Thus it is very dangerous to call a lawyer a shyster or a doctor a quack because such allegations do contribute very definitely to the injury of the professional standing of their object. However, I had as yet no profession; and although I hoped to have one in the future, damage to this would have been hard to prove and impossible to assess. This was the precise point that was raised later by the lawyers for *The New Yorker* in their defense of the suit instituted against them by W. J. Sidis; and the success of this defense confirms our judgment in not pushing the matter.

I regard this attitude toward libel eminently unjust. In the first place, to cast serious suspicions on a budding career seems to me an offense more serious than to interrupt one that is well under way. Secondly, an assault on the self-esteem of a person who is already in a difficult and questionable position is quite as great an injury as any physical assault could be. I think that a reasonable moral standard in such matters is exhibited by the practice of the medical journals, which I consider to be so well established that it would not be very difficult to give it the force of law. It is in the public interest that medical cases be reported precisely and freely in the professional journals. However, it is regarded as a grave offense to give the name of the patient or any data that could serve to identify him, at least without his voluntary and expressed consent. When a photograph of him is shown as part of the necessary documentation of the case, it is the custom, if the eyes and face do not show some essential part of the symptoms of the disease, to obliterate them in the print. I see no reason why a pedagogical journal, or indeed a journal without any scientific pretensions, should be given more latitude in such a matter. This is not a question of the freedom of the press, but it is eminently a question of the necessary correlate of such freedom: the responsibility of the press.

During my last year at Harvard I had applied for a traveling fellowship. Of course I was greatly excited when the news came that I had won it. Two places suggested themselves as alternate destinations: Cambridge, where Russell was then at the peak of his powers, and Turin, which was famous for the name of Peano. I learned that Peano's best days were over, and that Cambridge was the most suitable place for a training in mathematical logic. I then wrote to Russell, for it was necessary to obtain the permission of my teacher before I set out on my studies.

Note

1. Katherine Dolbear, *Pedagogical Seminary,* Vol. 19, *"Precocious Children,"* p. 463.

14 Emancipation

Cambridge, June, 1913–April, 1914

We returned to New Hampshire that summer and I had a good chance to rest up for the year to come and to acquaint myself further with the mountain region. The mountains were an eternal delight to me. They are beautiful even now, but in those days before the war and the threat of war, before the extensive lumbering which the two World Wars called into being, before the motorcar and its reduction of distances to nothing and much of the roadside to a rural slum, the country was beautiful indeed. As one whose physical activity is somewhat limited by his increasing years and the vicissitudes of an active life, I look back with a certain sadness to a time when the mountainsides were as nothing to my efforts, and when twenty minutes of rapid striding would carry me to a bank of lacy wood sorrel. From this bank I could look up to the boles of mighty trees, each fit to be a mast of a king's ship. I felt a sense of romantic union with the hills and the forest.

One of my chief domestic tasks was to fetch the mail and the milk. Every day I tramped two miles to the little post office at Whiteface village and two miles back again, part of it with the milk pail digging into the palms of my hands. I was eager to go for the mail because the key to adventure awaited me there: my letter of acceptance from Russell.

Professor Huntington had recommended to me two mathematical books for summer reading before starting my work with Russell. They were Bôcher's *Modern Algebra* and Veblen and Young's *Projective Geometry.* The first book did not impress me so much then, although I have reread it many times and found it most useful as an introduction to matrix theory. The second book I took to my heart as the most consistent exposition of the postulational standpoint that I had found anywhere. I worked out almost all the problems of the first volume, which was the only one existing at the time. While the book had two authors, Young of Dartmouth was already somewhat invalided, and the personality of the book was chiefly that of

Professor Oswald Veblen of Princeton. He was the founder of the great mathematical school of Princeton as well as the scientific founder of the Institute for Advanced Study, also at Princeton. He is without doubt one of the fathers of American mathematics.

The entire family was to go abroad for the winter. We had been tempted to go earlier, and had even gone so far as to make some negotiations for tickets, but this had been at the time of the Balkan Wars, and my father had considered the political heavens too stormy to risk it. Now, however, we really did embark. We had picked a boat of the Leyland Line, a small twig of the great I.M.M. combine, which ran cattle boats and carried a few passengers between Boston and Liverpool. I can remember that in those lucky days it was possible for fifty dollars to book a cabin for oneself and to have the run of the boat.

We left Cambridge by the subway and the East Boston Tunnel for that desolate region of slums and docks known as East Boston. Here our ship was tied up. I remember that it hurt me to run across a maze of railroad tracks with heavy baggage in my hands, under the conflicting and self-contradictory orders of my father.

It was a heavenly relief to go aboard. The white-jacketed stewards served us biscuits and beef tea even before we had left the harbor. While we were still in the old familiar Boston harbor, with the Bunker Hill monument showing plain, we were already in a foreign territory: the manners of the stewards, the customs of eating and drinking, the very language that people spoke, were all new and strange to us.

My parents had maintained an almost instinctive position that the English they spoke and had learned was the only proper English language, and that all other forms of the speech had something illegitimate about them. I daresay Father would have made the adjustment to Basque or Tibetan more readily than to the change between the English of the American Boston and the English of London or of Lancashire.

For it was the English of Lancashire that predominated on the boat. It is a language that I have heard many times since; and although it is perhaps not the most beautiful of all forms of English, it has something of the winning quality of good bread and good cheese.

The passengers were few, and the radio bulletins of news of the world were not obviously obtrusive. The trip was long, uncrowded, and peaceful. The food was adequate but stodgy. There was nothing to watch except marbling waves, or the casual flirtation of an old sea captain's daughter with the wireless officer. With a little shuffleboard and a little chess, we made the trip very comfortably. And one morning we found ourselves tied up at the landing stage in the Mersey.

The formalities of landing were simple. It was a Sunday morning, and after we had bought our tickets for London, we had a meal of bread and cheese at a pub, and took off. I looked out of the train window, and renewed the impressions of the English countryside I had seen before as a child. In particular, I recalled the ivy, the smaller farms and fields, the brick and stone buildings, the less wooded landscape, and the seemingly smaller trees.

From Euston Station we made our way to Bloomsbury, which was then even more than now the natural barracks of the academic visitor of moderate means. We put up at a hotel at Southampton Row, which I recognized many years later as the scene of one of Graham Greene's more dismal tales of refugees and espionage. With the aid of our Baedeker, we found one or two possible vegetarian restaurants. We looked up father's old friend, Israel Zangwill in his lodgings in the Temple, and made plans for my stay in Cambridge. The rest of the family was to go to Munich for the winter. Constance was to study art and Bertha was to go to a private school for teen-age girls.

Father went up with me to Cambridge. We looked up Bertrand Russell in his rooms at Trinity, and he helped us to orient ourselves. While we were in Russell's rooms a young man came in whom my father took to be an undergraduate and who excited no particular attention in us. It was G. H. Hardy, the mathematician who was to have the greatest influence on me in later years.

It appeared that it was not necessary for me to matriculate inasmuch as Harvard and Cambridge had certain agreements concerning the privileges of advanced students. I therefore could not expect to live in college and it was necessary to find a landlady for me in town. My father did not spend much effort in placing me in lodgings. In one place he asked me in the presence of the landlady what I thought of the place. I was caught. I was forced to tell him on leaving that it seemed to me one of the most miserable, dirty, and inconvenient lodgings I had seen. Instead of canceling the word-of-mouth agreement that we had made, Father trusted to the improbability of my ever meeting the landlady again, and left matters to take care of themselves. He was in a hurry to catch the train back to London. Finally I was left *faute de mieux* with another slovenly little landlady in New Square. She had made some agreement to furnish me at the minimum price with the vegetables and cheese necessary for my vegetarian life.

It was at that time impossible for the American boy with anything like a normal bringing up to be completely free from a certain Anglophobia.

The wars between the two countries, including the undeclared hostilities of our Civil War, were united with a certain latent enmity of tone in some of the English reviews in such a way as to comb a Yankee's hair the wrong way. More than all of these influences, the efforts of a few ardent American Anglophiles had the effect of making the American boy brandish the flag and let the eagle shriek.

Yet later, when I came back from England, I had learned that there was a very close and permanent bond between myself and England, and more especially between myself and Cambridge. I had learned that the English were very different from the Anglophiles in that, once one had penetrated the protective layer which they assumed against Americans and other foreigners, they were quite willing to admit that there were aspects of England in which God was not in his Heaven and something was definitely wrong with the world. I found that the English were as distrustful as I was of the Anglophiles' cure-all, which was to import English institutions to America, cut up into numbered pieces and wrapped in straw, as if they were Tudor manor houses. In short, I found that the England of the Anglophiles was a cloud-cuckoo-land existing neither on one side of the ocean nor on the other, but merely in the souls of the elect.

I came to find that among the institutions in which I had lived those which were most similar to English life and cast the most light upon it were in many cases the most specifically American institutions of my childhood. The country life of Ayer and Harvard, although it was a country life with neither squire nor established vicar, was a country life with very English roots. My New Hampshire farmer friends would probably have damned their opposite numbers in the Lake Country from here to Kingdom Come, and would have been received with similar objurgations; but despite the mutual hostile reserve and the difference in the dialects, the attitude would have been much the same on both sides. It would have taken only a few weeks of mutual contact for the one and the other to become aware that there was not terribly much difference between their attitudes or their presuppositions.

The England that I first saw was one which had not yet been shocked by World Wars and indeed which had remained at peace since the times of Napoleon, except for colonial wars and the major conflicts in the Crimea and South Africa. It was an England that was heaven for the rich and very close to hell for the poor. It was an England in which it was harder for a working man to become a scholar than it now is for a Mexican peon. This stratification and the snobbishness attendant on it—which was even more a masochism on the part of the poor than a sadism on the part of the rich—is something which, while some elements of it may remain, has passed out of the picture as completely as the France of the *ci-devants* did at the time of the French Revolution.

My landlady gave me my first introduction to the sort of English snobbishness and subservience that was then rife but which has since become much less common. She, a slovenly, mean little woman, did not approve of our neighbor two doors away. She said, "Ow, 'e's only a tridesman's son," even though the rank of tradesman was something vastly higher than any to which she could ever lay claim.

The university men of 1913 were young sprouts of the aristocracy, or at least of a well-established middle class. Since then I have seen the rise of the subsidized undergraduate. The working class boy, stunted by undernourishment in his early childhood and in the womb of his mother, with bad teeth and horny hands, wearing a hand-me-down suit and big clumping boots, has come to be supported by exhibitions and scholarships through his primary and secondary schools and his university. These are the men I now know as young dons; accepted because of their ability and character, but often cursed with a social awkwardness which they have had to unlearn with a very genuine and conscious effort. More than one of them has confided to me the pains which he had to take at the beginning to develop a good line of high-table back-chat.

The phenomenon of which I speak is spread far beyond the cloistered courts of England's university. It is a relief to me now to be able to sit on a park bench and talk with an English workingman who will neither resent me as a "toff" nor whine for some advantage. Indeed, to the present generation of Englishmen who read this book it may seem that I am accusing their predecessors of vices that are so far from their own make-up that the newer Englishman is unable to conceive them. But I can say that as I have revisited England year by year, I have seen servility decline and a universal manliness and comradeship come to the front.

So much for my reminiscent view of Cambridge. At the time of my first arrival, after spending a few days learning the lay of the land, I was hopelessly and utterly lonely. Term had not yet begun, so that there was no chance for me to make new acquaintances. I wandered about the colleges and in the Backs, and the utter beauty of the buildings and the foliage was more than a little solace to my nostalgia. Meanwhile I met one or two undergraduates: a Hindu who lodged in the same house as I and a young Englishman two houses away. They both belonged to St. Catherine's College, and they invited me to participate in the meetings of a discussion club belonging to that college.

I have no specific recollection of what was said and done at that club at St. Catherine's. I remember that I was asked to read a paper and to say a few words. I did so, and I have a dim memory that I covered myself with shame and confusion. I certainly spent the first few weeks in Cambridge in learning the English point of view, and in sloughing off some of the most impermissible of my many awkwardnesses. I know that my callow nationalism got me into more than one childish quarrel.

Nevertheless, I feel that this was a critical period in my formation, and that I owe a great deal of gratitude both to my teachers and to my undergraduate friends of those days. I found in them a receptiveness and a tolerance of ideas which had not been characteristic of Harvard, and a challenging dialectical skill in presenting them.

Although I had very good times with several of the young undergraduates in their clubs and social groups and "squashes" and at tea in their rooms, there was a group of slightly older men on the boundary between the undergraduate and the don who were particularly kind and helpful to me. One of them was F. C. Bartlett, now Sir Frederic Bartlett, and a professor of psychology at Cambridge University. My impression is that he had come from one of the more modern English universities, and that at that time his prospects for a career were not particularly bright. I found his steady quietness and his refusal to be stampeded by any argument a healthy tonic for my own impulsiveness. His criticism was always fair and not to be bribed by friendliness. I am glad that our relations have been kept up over these many decades, and that the basis for them has not changed in any essential way.

Bernard Muscio was another one of my seniors who was very kind to me and who helped me to grow up. He was born in Australia, where he had obtained his first degree. His alertness and quickness of reaction made him an important figure in the Moral Science Club, better known as the Moral Stinks Club, and more than once we joined forces in a dialectic assault on those with whom we did not agree.

Two of my early associates of a very different kind were C. K. Ogden and I. A. Richards. Ogden, who had succeeded in prolonging an undergraduate career over an unheard-of period of years, then lived above a gateway in Petty Cury, where his rooms were adorned with photographs of practically every important man in intellectual England. Among the manifold facets of his being, he was a journalist and he solicited from me an article which he published in the *Cambridge Magazine,* and whose nature I have entirely forgotten in the course of these many years. Richards and he were close companions, and I believe that during my stay in Cambridge the collaboration which later led to the publication of *The Meaning of Meaning* had already begun. At any rate, their interest in semantics was manifest.

One of the things that most impressed me at Cambridge was the rather too cloistered atmosphere of the English university scholar. He had come from a school devoted to the needs of adolescence, which constituted the most essential and characteristic part of his education, to a university built according to a scheme closely paralleling that of his adolescence. If he were successful, a career was open for him for his whole life under much the same auspices.

The English universities, although they were no longer exclusively the celibate, clerical institutions which they had been in the earlier nineteenth century, still retained much of their monkish character. Thus the young man going into mathematics carried into his valuation of mathematical work a great deal of the adolescent "play-the-game" attitude which he had learned on the cricket field. This, although it contained much of good, and led to a devotion to scholarship difficult to find in our more worldly life, was not conducive to a fully mature attitude toward his own work.

When G. H. Hardy—as the reader may easily find in his book, *A Mathematician's Apology—values* number theory precisely for its lack of practical application, he is not fully facing the moral problem of the mathematician. It takes courage indeed to defy the demands of the world and to give up the fleshpots of Egypt for the intellectual asceticism of the pure mathematician, who will have no truck with the military and commercial assessment of mathematics by the world at large. Nevertheless, this is pure escapism in a generation in which mathematics has become a strong drug for the changing of science and the world we live in rather than a mild narcotic to be indulged by lotus eaters.

When I returned to Cambridge as a mature mathematician after working with engineers for many years, Hardy used to claim that the engineering phraseology of much of my mathematical work was a humbug, and that I had employed it to curry favor with my engineering friends at the Massachusetts Institute of Technology. He thought that I was really a pure mathematician in disguise, and that these other aspects of my work were superficial. This, in fact, has not been the case. The very same ideas that may be employed in that Limbo of the Sages known as number theory are potent tools in the study of the telegraph and the telephone and the radio. No matter how innocent he may be in his inner soul and in his motivation, the effective mathematician is likely to be a powerful factor in changing the face of society. Thus he is really dangerous as a potential armorer of the new scientific war of the future. He may hate this, but he does less than his full duty if he does not face these facts.

In laying out my course, Russell had suggested to me the quite reasonable idea that a man who was going to specialize in mathematical logic and in the philosophy of mathematics might just as well know something of mathematics. Accordingly, I took at various times a number of mathematical courses, including one by Baker, one by Hardy, one by Littlewood, and one by Mercer. I did not continue Baker's course long, as I was ill prepared for it. Hardy's course, however, was a revelation to me. He proceeded from the first principles of mathematical logic, by way of the theory of assemblages, the theory of the Lebesgue integral, and the general theory of functions of a real variable, to the theorem of Cauchy and to an acceptable logical basis for the theory of functions of a complex variable. In content it covered much the same ground that I had already covered with Hutchinson of Cornell, but with an attention to rigor which left me none of the doubts that had hindered my understanding of the earlier courses. In all my years of listening to lectures in mathematics, I have never heard the equal of Hardy for clarity, for interest, or for intellectual power. If I am to claim any man as my master in my mathematical training, it must be G. H. Hardy.

It was while I sat in this course that I wrote the first mathematical paper which I saw in print. Looking back on this paper, I do not think it was particularly good. It was on a reordering of the positive integers in well-ordered series of large ordinal numbers. Still, it gave me my first taste of printer's ink, and this is a powerful stimulant for a rising young scholar. It appeared in the *Messenger of Mathematics,* which was published in Cambridge, and I had the satisfaction of seeing it through press on the spot.

I attended two courses of Bertrand Russell. One was an extremely elegant presentation of his views on sense data, and the other a reading course on the *Principia Mathematica*. In the first course I could not find myself able to accept his views on the ultimate nature of sense data as the raw material for experience. I have always considered sense data as constructs, negative constructs, indeed, in a direction diametrically opposite to that of the Platonic ideas, but equally constructs that are far removed from unworked-on raw sense experience. Apart from our disagreement on this particular matter, I found the course new and tremendously stimulating. In particular, I found myself introduced to Einstein's relativity, and to the new emphasis on the observer which had already revolutionized physics in Einstein's hands and which was to revolutionize it even more completely in the hands of Heisenberg, Bohr, and Schrödinger.

There were only three of us in Russell's reading course, so that we made rapid progress. For the first time I became fully conscious of the logical theory of types and of the deep philosophical considerations which it represented. I became shamefully aware of the shortcomings of my own doctoral thesis. Nevertheless, in connection with the course I did one little piece of work which I later published; and although it excited neither any particular approval on the part of Russell nor any great interest at the time, the paper which I wrote on the reduction of the theory of relations to the theory of classes has come to occupy a certain modest permanent position in mathematical logic. It was published soon after I was nineteen in the *Proceedings of the Cambridge Philosophical Society,* and this paper represents my true introduction into mathematical thinking and writing.

It is not very easy for me even at this distance to write of my contact with Bertrand Russell and of the work I did under him. My New England puritanism clashed with his philosophical defense of libertinism. There is a great deal in common between the libertine who feels the philosophic compulsion to grin and be polite while another libertine is making away with the affections of his wife and the Spartan boy who concealed the stolen fox under his cloak and had to keep a straight face when the fox was biting him. This does not endear the philosophical libertine to me. The old-fashioned rake had at least the fun of don't-care; the puritan is working within a code of known restrictions which tends to keep him out of trouble. The philosophical rake is as bound as the puritan, and has to steer a course in as narrow a channel; but it is a channel which is poorly lighted and poorly buoyed. I expressed myself very freely in this matter, and I am quite certain that Russell heard my comments to a friend one dark night when we met on the street as we were returning to his quarters. Though he never gave a sign of hearing me, this experience rendered me particularly apprehensive of his criticism.

I know that Russell regarded my Harvard thesis as inadequate, in that I did not enter sufficiently into the problem of logical types and into the paradoxes that mark the difficulties of establishing a fundamental postulational system for logic, as opposed to a derived postulational system for a specific construction with a recognized logic. As for myself, I already then felt that an attempt to state all the assumptions of a logical system, including the assumptions by which these could be put together to produce new conclusions, was bound to be incomplete. It appeared to me that any attempt to form a complete logic had to fall back on unstated but real human habits of manipulation. To attempt to embalm such a system in a completely adequate phraseology seemed to me to raise the paradoxes of type in their worst possible form. I believe I said something to this effect in a philosophical paper which later appeared in the *Journal of Philosophy, Psychology and Scientific Method*. Bertrand Russell and the other philosophers of the time used to term this journal "the Whited Sepulchre," an allusion to the simple white paper cover in which it appeared.

My heresies of that time have been confirmed by the later work of Gödel, who has shown that within any system of logical postulates there are questions that cannot receive a positive answer through these postulates. That is, if one answer to these is consistent with the original postulates, it can be proved that the opposite answer is equally consistent with them. This treatment of the problem of decision has rendered obsolete a considerable part of the task undertaken by Whitehead and Russell in the *Principia Mathematica*.

Thus logic has had to pull in its horns. The limited logic which remains has become more nearly a natural history of what is in fact necessary for the consistent working of a system of deduction than a normative account of how it should be worked. Now, the step from a system of deduction to a deductive machine is short. The *calculus ratiocinator* of Leibnitz merely needs to have an engine put into it to become a *machina ratiocinatrix*. The first step in this direction is to proceed from the calculus to a system of ideal reasoning machines, and this was taken several years ago by Turing. Mr. Turing is now occupied in the actual construction of computing and logical machines, and has thus completed a further step in the direction of the *machina ratiocinatrix*. The remarkable thing is that I myself, quite independently of him, have recently also taken the step from my early logical work to the study of the logic of machines, and have thus again met the ideas of Mr. Turing.

To go back to my student days with Russell, although there were many points of disagreement and even of friction, I benefited enormously by them. His presentation of the *Principia* was delightfully clear; and our small class was able to get the most out of it. His general lectures on philosophy were also masterpieces of their kind. Besides his consciousness of Einstein's importance, Russell also saw the present and future significance of electron theory, and he urged me to study it, even though it was very difficult for me at that time, in view of my inadequate preparation in physics. I do not recall, however, that he was quite as explicit and accurate in his valuation of the coming importance of quantum theory. It must be remembered that the epoch-making work of Nils Bohr was very new at the time, and that in its original form, it did not lend itself particularly to a philosophical interpretation. It was only some twelve years later, in 1925, that the conflicting currents aroused by the earlier work by Bohr began to be resolved and that the ideas of De Broglie, Born, Heisenberg, and Schrödinger showed that quantum theory was to mark as great a revolution in the philosophical presuppositions of physics as had the work of Einstein.

On the social side, the most distinctive aspect of my contact with Bertrand Russell lay in his Thursday evening parties, or as they were called in view of the number of guests, his "squashes." A very distinguished group of men foregathered there. There was Hardy, the mathematician. There was Lowes Dickinson, the author of *Letters from John Chinaman* and *A Modern Symposium,* and the bulwark of the liberal political opinion of the time. There was Santayana, who had left Harvard for good to take up his residence in Europe. Besides these, Russell himself was always an interesting talker. We heard much of his friends, Joseph Conrad and John Galsworthy.

Three of the most important moral science dons with whom I came in contact, all fellows of Trinity, were known as the Mad Tea Party of Trinity. Their identities were unmistakable. It is impossible to describe Bertrand Russell except by saying that he looks like the Mad Hatter. He has always been a very distinguished, aristocratic Mad Hatter, and he now is a white-haired Mad Hatter. But the caricature of Tenniel almost argues an anticipation on the part of the artist, even though I am told that the original of Lewis Carroll's description and Tenniel's caricature was an actual hatter at Oxford, and that his "Anglo-Saxon Attitudes" were really the effect of an industrial mercury poisoning. McTaggart, a Hegelian and the Dr. Codger of Wells's *New Machiavelli,* with his pudgy hands, his innocent, sleepy air, and his sidelong walk, could only be the Dormouse.

The third, Dr. G. E. Moore, was a perfect March Hare. His gown was always covered with chalk, his cap was in rags or missing, and his hair was a tangle which had never known the brush within man's memory. Its order and repose were not improved by an irascible habit of running his hand through it. He would go across town to his class, with no more formal footwear than his bedroom slippers, and the space between these and his trousers (which were several inches too short) was filled with wrinkled white socks. He had the peculiar habit of emphasizing his words on the blackboard by running them through with chalk-marks instead of underlining them. He used to make the most withering remarks in philosophical discussion, in a breathless but smiling and unperturbed manner. "Now really," he would say, "you can't expect any sane person to hold a view like *that*!" On at least one occasion at a meeting of the Moral Science Club, he brought to a state of tears Miss E. E. C. Jones, the Mistress of Girton, lovingly known as

"Mammy Jones" to the unregenerate. Yet when I came to know him and to depend on his criticism of my work, I found him kind and friendly.

There is among the dons a premium on individuality which often becomes a premium on eccentricity. I have been told by some of my Cambridge friends that they thought that certain of my less conventional habits had been adopted with a view of acquiring acclaim. At any rate, the fact is there; and while I do not think that Russell's mannerisms (which were very slight) were any more than a genuine manifestation of his aristocratic background, I am quite certain that the untidiness of G. E. Moore and the academic unpracticalness of McTaggart had been cultivated very carefully. They had the flavor of a crusty old port—a flavor that does not reach its full perfection without the expert intervention of the cellarman.

During the term I made quite a number of acquaintances, and my mantelpiece was adorned with the cards of discussion clubs. I had an invitation to visit some friends of Mr. Zangwill, who lived about fifteen miles out in the country; and I turned up there, dusty and bedraggled, after walking the entire distance. In general, by the end of the term I was finding my social place in Cambridge. I had even begun to have a certain fondness for my new environment.

Yet most of the time I was desperately uncomfortable in a physical sense. My landlady had been paid little enough; and yet that could hardly excuse the raw carrots and inedible Brussels sprouts which she gave me in lieu of proper vegetarian meals. I eked out my diet with occasional penny bars of chocolate and the like, but the net result was that I was half starved.

In my leisure hours, and I had many of them, the Union and its library were my salvation. My membership in the Harvard Union had enabled me to make use of the facilities of its Cambridge counterpart, and I even took part in one or two of the famous undergraduate debates. Moreover, some of my friends occasionally asked me to dine at the Union, so that I learned something of the amenities of an English club.

I found the Cambridge environment far more sympathetic to me than I had found that of Harvard. Cambridge was devoted to the intellect. The pretense of a lack of interest in intellectual matters which had been a *sine qua non* of the life of the respectable Harvard scholar was only a convention and an interesting game at Cambridge, where the point was to work as hard as you could in private while pretending to exhibit a superior indifference. Furthermore, Harvard has always hated the eccentric and the individual, while, as I have said, in Cambridge eccentricity is so highly valued that those who do not really possess it are forced to assume it for the sake of appearances.

Thus when the beginning of December came and I left to spend the Christmas holiday with my family in Munich, I was both happier and more of a man than I had ever been. The trip was a lark. I crossed to the Continent by the Harwich route, and did not have a bad passage. I was up well before dawn to see the lights of the Hook of Holland, and I was pleasantly bewildered by hearing the Dutch speech of the porters. I breakfasted in the big, empty, echoing railway station, and dawn saw me well on the way toward Rotterdam. I don't know by what use of English, bad German, and gestures I persuaded a porter to take my trunks across town in a barrow to another station, but I soon found myself bound for Cologne, uncomfortably seated in a third-class compartment, all windows hermetically sealed, in an atmosphere that seemed to be made partly of commercial travelers and partly of tobacco smoke.

I arrived in Cologne in the early afternoon and found quarters for myself in a very cheap hotel, which I now believe to have been nothing more than a Kellnerheim. There was no way to get to Munich that day, so I took a walk about the town and tried to correlate my impressions with my memories of my childhood trip more than eleven years earlier. I found that in fact there was a good deal that I did remember: for example, the station, the bridge, and the cathedral.

I went to Munich the next day in a through carriage. I was delighted with everything that I saw on the way, from the forests with touches of snow on them to the villages and stations which looked to me like the illustrations accompanying the set of Anker building blocks I had played with as a child. My German was as yet insufficient to enable me to communicate with my fellow travelers, so I spent most of my attention on the landscape outside. The scenery along the Rhine was a reawakening of my memories of the former trip of my boyhood, and the wooded mountains of Franconia had not a little suggestion for me of the White Mountains.

My family met me at the Munich station and took me to the old-fashioned but centrally placed apartment that they had rented. Although the apartment house had long been invading America, I had never lived in one up to that time, and the apartment-house mode of life was to my parents something altogether undesirable. Indeed, I had been brought up to regard the city life of the apartment as a deprivation and a misfortune for the people who had to resort to it. The fact that our landlady spoke no English, and that my mother was not confident of her German, did not ease matters.

My father spent his time working at the Bavarian Court and National Library. Away from the Harvard library (where by long experience he could put his hand on every book he wanted), and under the usual restriction of exclusion from the stacks and the eternal pinpricks of the abominable system of cataloguing which was then standard outside the United States, his work languished. Moreover he was disappointed that his name was less well known by his European colleagues than he had expected, and that he had few personal contacts with them or none at all. To some extent this was only to have been expected, for my father had a very individual way of working, and he had no hesitation in contradicting flatly the presuppositions of the scholars of his time and in writing in a blunt manner which was an affront to their sense of self-importance. Germany was then a hierarchical form of society, from the workingman at the bottom to the Kaiser on top; and within the greater frame the university people were a lesser hierarchy. For a mere foreigner with no place in this system to beard a whole school of learned German *Geheimrats* was a scandal beyond words. My father, who was a most sensitive man, could not fail to be aware of the atmosphere about him.

Until that time, my father had always been a great admirer of German culture and German education. Although he had resented the militarism and officialism that had developed since his own youth, he was fundamentally a German liberal of the middle of the last century. His Russian Tolstoyism was an influence that ran parallel to the German influences in his development and did not contradict them. He had for a long time looked forward to the period when he should return to Germany and be accepted as a great scholar within the German frame of things. When this did not occur, and he found himself rejected, or perhaps merely not accepted, this emotional longing turned to a hate which was as bitter as only a hate for a lost love can be.

My sisters had been properly placed in the appropriate schools. I do not remember through what vicissitudes of attempts at musical and artistic training my sister Constance had gone before she decided to work at the Kunstgewerbeschule, or school for industrial art. Bertha was placed at a fashionable and respectable girls' school, the Institut Savaète, where she made good progress in her general education and her understanding of things German. I do not remember quite how we disposed of Fritz's school time.

By now I had become sufficiently grown up to be a fairly acceptable comrade for my father. We went together to various lectures and beer-hall meetings where interesting subjects were being discussed. I remember one such meeting on international peace and understanding at which the speaker was David Starr Jordan, the famous ichthyologist and president of

Stanford University. I remember drinking my glass of beer and feeling very much the man among the German students.

My parents took me occasionally on their outings to the Plätzl and other cabarets; and I often went with my sisters to the movies, which were just beginning to give signs of their later development. There was also a small amount of visiting fairs and historical museums. However, my chief delight was the Deutsches Museum: a museum of science, engineering, and industry. Part of the exhibits were historical and old-fashioned; but the museum led the world in its demonstration of the technique of scientific experiments, which the visitor could actually work for himself behind the protecting glass cases by pulling strings or by turning knobs. There were some delightful old attendants there who were ready to put themselves at the service of the interested visitor, and to show him particular tidbits not always brought to the attention of the general public. I remember one in particular who put himself out to be nice to me; he possessed a few words of English and a most delightful Bavarian brogue.

The Deutsches Museum had an extremely modern scientific library; there I read assiduously the various works that Russell had assigned to me. I remember among them the original papers of Einstein. I have said that Russell was among the first philosophers to recognize the overwhelming importance of Einstein's work in that *annus mirabilis* 1905, in which he had originated the theory of relativity, solved the problem of the Brownian motion, and developed the quantum theory of photoelectricity.

Another delight of that vacation was the Englischer Garten, even in its snow-covered winter state. I remember the skaters on the pond near the Chinese Pagoda. I was not aware at the time that the Englischer Garten was laid out after the plans of a New England Yankee from Woburn, Massachusetts, the great and disagreeable Benjamin Thompson, Count Rumford, and paymaster for Benedict Arnold.

I returned to Cambridge in January. I felt myself already much more at home in the town, and much less lonely. I continued to distribute my time between philosophy and mathematics, and began a second paper for the Cambridge Philosophical Society. This time I tried to use the language of the *Principia Mathematica* to describe series of qualities, such as those found in the color pyramid, which escaped from a treatment of series given by Whitehead and Russell because they were not infinitely extensible in both directions. What I found necessary was a logical treatment of systems of measurement in the presence of thresholds between observations whose differences were barely noticeable. In the paper I used certain ideas related to those of Professor Whitehead, who was then at the University of London and who had recently employed a new method of defining logical entities as constructs from entities of a primitive system possessing no particular specific properties rather than as the objects of a system of postulates. I wrote to Professor Whitehead for an appointment and visited him in his house in Chelsea, where I met the whole family. Little did I think at that time that Professor Whitehead was to end his long and useful career as my neighbor at Harvard University, and that as a very inept pupil of his daughter I was later to learn some of the rudiments of the art of rock-climbing in the crags of the Blue Hills and in the Quincy quarries.

I had intended to complete the year in Cambridge, but I found that Russell had been invited to Harvard for the second semester and that therefore I would be marking time in Cambridge during the May term. At Russell's own advice, I decided to finish the year at Göttingen, studying mathematics with Hilbert and Landau and philosophy with Husserl. I returned to Munich for the vacation between the last two terms. My father had already left for the United States, where he was consoled in his loneliness by the companionship of some younger colleagues in the German department, but my mother and the rest of the family were still in Munich.

During the year I had read that Harvard offered a number of prizes for essays by students, both undergraduate and graduate. I found that I was eligible to compete for one of the Bowdoin Prizes and submitted a rather skeptical essay, which I called "The Highest Good." It was intended as refutation, or at any rate as a denial, of all absolute ethical standards. Bartlett did not think much of it, either as a composition or as a philosophical essay, but at any rate it won one of the prizes. I am quite sure that Sir Frederic still regards this as a shortcoming of Harvard rather than as a success of my own.

My departure from England was marred by a very unpleasant experience with my landlady. When my father had made the arrangements with her, he had thought that he was committing me for a single term or less. However, by the custom of Cambridge the term is of a certain specified length, which is longer than the period known as full term, during which the students are supposed to be in residence, and all lodgings contracts are or were made for the longer period. As the second term drew to the end, I found my landlady insisting on this contract. From being demanding she became pressing, and from being pressing she became insulting. I retorted in kind, which made the matter worse. Some of my undergraduate friends with whom I consulted suggested a minor riot at my landlady's expense; but although I was foolish, I was not quite so foolish as that. When I tried to take one of my trunks out of the house on my own back, the landlady impounded the other; and when I asked the police to help me in getting back my own property, they told me that it was a civil matter and that they could have nothing to do with it.

I had been living on an absolutely minimal sum, so that when I paid the landlady the sum necessary to redeem my trunk, I found that I did not have enough money left to get to Munich. I borrowed a small sum from the hall porter of the Union. Out of shame I borrowed too small an amount. The result was that when on the train down to Munich, I had to decide whether to have a cheese sandwich for breakfast and go hungry for lunch, or vice versa. I do not remember which way I decided it. The upshot of it all was that I landed in Munich with not a single coin in my pocket. Luckily the check room charges were paid on reclaiming the baggage, so I left my baggage at the station and walked over to our apartment.

I found matters rather in a crisis at the apartment. The smoldering friction between the landlady and my mother had burst into flame, now that my father was no longer there to help with his German. Mother went house-hunting, and after much effort we managed to find an apartment in the suburbs, well out toward the northern end of the Englischer Garten, and almost abutting on it. Here we were completely at peace.

15 A Traveling Scholar in Wartime

1914–1915

After staying in Munich a few weeks, I went on to Göttingen before the beginning of term, to take part in a psychological congress which was being held there and to see my old friend Elliott, the Harvard psychologist, who had come there to participate in it. I have not much recollection of the congress, but I found the town a delightful medieval gem, with the circuit of its old walls almost complete.

For what seemed to me a ridiculously small sum, I immatriculated at the university and began to search for a lodging and for a vegetarian restaurant at which to eat my meals. I found my lodging just outside the walls at the house of a Fräulein Büschen. It was a half-timbered villa in Swiss style, and my room, though dark, was adequate. Fräulein Büschen, who had been a music teacher, attended to the business side of the establishment. She managed this very competently, and left the matter of our breakfast and other domestic needs to her sister, who did not aspire to the social distinction claimed by the music teacher. Somewhere around the establishment there was an obscure brother, who had been trained as a dentist but who did not seem to practice that art.

I remember at least one party the Büschens gave for their student lodgers, which was attended by some nice girls from the neighborhood. I particularly remember the shock to my 1914 New England susceptibilities when I found that the whole company, both men and women, were smoking cigarettes and were not averse to drinking to the point of becoming slightly tipsy.

I found my vegetarian restaurant in the Theaterstrasse, in the home of a Frau Bauer. She was a widow with a considerable number of daughters of varying ages, who assisted her and the cook. The girls waited barefoot at the table, for vegetarianism was not the only respect in which the Bauers departed from the norm. They were clothes-reform people, youth-movement people, health faddists, and anti-Semites as well. It was in their restaurant that I first saw that vile sheet *Hammer*, which even at that epoch already contained all the lies and blasphemies that Hitler and Goebbels disseminated so disastrously at a later period.

Bigoted as they were, the Bauers were not entirely bad. Their food was good and cheap, and they were personally amiable enough. They served an oatmeal preparation with the uninviting name of *Haferschleimsuppe*, or "oat slime soup." It was inexpensive and filling.

I often wonder whether the poor Bauers ever realized what serpents they had taken to their bosom in the person of myself and a young Scottish mathematical physicist by the name of Hyman Levy. Levy, who is now a distinguished professor at the Imperial College of Science and Technology in London, notwithstanding his impeccable Glasgow accent, was, like myself, just what his name purported him to be; and yet we two Jewish sons of perdition defied the wrath of the anti-Semitic periodicals about us and ate, nay even enjoyed, the cheap and savory meals prepared for us. When I think that in addition to his Jewishness, Professor Levy has become a bulwark of the Left in English politics, I imagine that old mother Bauer, if indeed she be dead, is turning cartwheels in her grave.

Service was very slow in the Bauer restaurant. We used to take our plates out to the cook in the kitchen and have her fill them for us from the pots on the stove. This informality was possible because we were a happy-go-lucky, impecunious mixture of Germans and Americans, Britons and Russians, and the low prices attracted many of us who had no fads or dietary interest at all. We used to read our papers there.

The *Kneipen* or drinking bouts of the German students are well known. We too—the English and the Americans—had our *Kneipen* at the meetings of two separate but commensal societies known as the British and the American Colonies. The heads of these two colonies were called the Patriarchs, and Levy was the British Patriarch. The two clubs occupied a room above the Franziskaner Restaurant. The beer supply was steady and unfailing, and the floor was on such a slant that navigation was difficult, even without a cargo of beer. We shared a piano, a *Kommersbuch,* or German students' song book, and a Scottish Students' Song Book, which was Levy's personal property. Our meetings were long, moist, and harmonious. We paid our respects to the land that had welcomed us as well as to the lands that had fostered us by singing indiscriminately in English and in German. We were the rowdiest *Kneipe* in town and had been compelled to leave two or three former quarters by the protest of the proprietor or of the police.

There was one member whose name I shall not mention out of respect to such of his kinsmen as may be alive, though no contemporary Göttinger can ever forget him. It was not Early, though that is what I shall call him. Early was the son of an American publisher of hymn books, and he seemed determined to live down his family's good name. He was married, and his wife and young daughter had the full sympathy of the united colonies. Early had looked around the world for a soft spot to settle, and he had picked Göttingen. In some vague way he had managed to immatriculate at the university, where he had been a student for about ten years, though I had never heard of his taking or attending a course. When any of the American students made a pilgrimage to a nearby city for questionable purposes, it was Early who was their guide, friend, and philosopher.

These activities, I must confess, were only minor manifestations of his personality. The serious purpose of his life was drink. Never did a meeting of the united colonies come to an end but he was drunk as a coot, and some one of us had to see him safely home. I believe he was courteously apologetic on these occasions; and indeed there were about him some curious remnants of a man of breeding.

When I next went to Göttingen in 1925, Early was gone, though his fame had not vanished. I am told that he stayed on well into the First World War but that before we entered that conflict his family had been able to bring him home. In view of his age at that time and of his manner of life, he must long be dead by now. Yet his type will never die; and wherever scholars come together and there is a comfortable life, there will be the blight of the perpetual student. I write this chapter in a room in a hotel just off the Boulevard St. Germain in Paris. Around the corner at this moment dozens of Earlys at the Cafés Flore and Deux Magots are sipping their *apéritifs* and trying to turn the more serious young people into their own likenesses.

I had a varied range of acquaintances in Göttingen. I remember a student who had come from the imperial Russian police and who was studying psychology in connection with his professional career. Another of my philosophical acquaintances was a very bright Russian Jewish boy. On one occasion several of us were at a small party at the house of the landlord of the latter fellow—a retired chief forester with the bluntness that one naturally associates with this profession. I don't remember all the things we discussed, but my philosophic friend asked me to say something about the work of Bertrand Russell. After I had spoken my little piece, my fellow student brought out: "But he doesn't belong to any school."

This was a serious shock to me: that a philosopher should be judged not by the internal implications of his own work but by the company he keeps. It was not, in fact, the first time I had heard of this intellectual gregariousness, which was common in Germany of those times but not confined to it, but I had never really run up against a first-class example of that sort of pedantry. I had, it is true, encountered the collective manifesto of the American New Realists. But the weakness of this group was so apparent that it seemed to me their solidarity and mutual mental support resembled that of a group of college students returning home after an exciting evening following a great football game: they literally could not stand up alone.

However, when I now saw a similar phenomenon in Germany, it claimed to be more than a protective huddle. The implication was that the privilege of a man to think depended on his having the right friends. Later, when I was to come back to the United States, I found that I myself had the wrong friends. I had studied with great men but they were not the men on the American scene. The Harvard department of mathematics would have none of me because I had learned the greater part of my mathematics at Cambridge and at Göttingen. When the new Princeton department recruited its men after the war, I was already too much of a lone wolf to be welcomed there. It is true that these two universities (together with Chicago) never reached the extremes of some of the German universities in their corporate isolationism, but they have made a good try at it.

I attended a course on group theory given by Professor Landau and a course on differential equations under the great Hilbert. At a later period, when I had become more familiar both with the literature of mathematics and with the techniques of mathematical research, I came to a clearer understanding of these two men. Hilbert was the one really universal genius of mathematics whom I have met. His excursions from number theory to algebra and from integral equations to the foundations of mathematics covered the greater part of known mathematics. There was in his work a complete grasp of tools and techniques; however, he never put into the background the fundamental ideas behind these. He was not so much the manipulative expert as the great mind of mathematics, and his work was comprehensive because his vision was comprehensive. He almost never depended on a mere trick.

Landau, on the other hand, was the chess player *manqué*. He believed in presenting mathematics as a sequence of propositions analogous to moves on a chessboard, and he did not believe in the nonsymbolically expressible part of mathematics that constitutes the ideas and strategy behind the moves. He did not believe in mathematical style, and as a consequence his books, effective as they are, read like a Sears-Roebuck catalog.

It is interesting to contrast them with the work of Hardy, of Littlewood, or Harald Bohr, all of whom wrote in the manner of cultivated and mature men. Landau, on the other hand, had intelligence, but he had neither taste nor judgment nor philosophical reflection.

It is impossible to mention the Göttingen of those times without referring to Felix Klein, but for one reason or another I did not meet him the semester I was there. I rather believe that he was out of town or in ill health. When I later met him in 1925, I found him very much of an invalid indeed: a grave, bearded man with a blanket over his knees, who sat in his magnificent study and discussed the mathematics of the past as if he were the Muse of mathematical history herself. He was a great mathematician, but by this time in his career he had become rather the *Geheimrat*, the elder statesman of mathematics, than the producer of mathematical ideas. There was something kingly about him which suggested to the career men of American mathematics that they, too, might be kings if they followed in his footsteps, and they treasured his little mannerisms (such as the way he speared his cigar with a penknife) as if by a careful observation of this ritual they might charm their way to greatness. Many years later I became aware that two generations of Harvard mathematicians had learned this trick from him.

Besides these mathematical courses, I sat in Professor Husserl's course on Kant and his seminar on phenomenonology. The philosophical courses left very little impression on me, as my German was inadequate for the subtleties of the philosophical language. I got something at the time from the mathematics courses, but much more by that sort of intellectual doubletake that allows one to realize at a later date the importance of what one has already heard but not understood.

Even more important for my intellectual training than the courses were the mathematical reading room and the Mathematical Society. The reading room contained not only what was probably the most complete collection of mathematical books in the world but also the reprints that Felix Klein had been receiving over the years. It was a great experience to browse among the books and the reprints.

The Mathematical Society used to meet in a seminar room, where the tables were covered with the latest numbers of all the mathematical periodicals of the world. Hilbert would preside, and professors and advanced students sat together. Papers were read by students and professors alike, and the discussion was free and incisive.

After the meeting we would traipse across the town to Rohn's café in a beautiful park at the top of a hill overlooking the town. There we would have a mild glass of beer or coffee, and would discuss all sorts of mathematical ideas, both our own and those we had learned in the literature.

There I came to know the younger men, such as Felix Bernstein, who had done some remarkable work in Cantor theory, and little Otto Szasz, with his high-heeled shoes and his bristling red mustache. Szasz was my particular crony and protector, and I am very happy that later when the Hitler regime came in, I was able to help place him in the United States.

The combination of science and social life in the *Nachsitzungen* at Rohn's café up the hill was particularly attractive to me. The meetings had a certain resemblance to those of the Harvard Mathematical Society, but the older mathematicians were greater, the younger men were abler and more enthusiastic, and the contacts were freer. The Harvard Mathematical Society meetings were to the Göttingen meetings as near beer is to a deep draft of Münchener.

About this time I had my first experience of the concentrated passionate work that is necessary for new research. I had the idea that a method I had already used to obtain a series of higher logical type from an unspecified system could be used to establish something to replace the postulational treatment for a wide class of systems. The idea occurred to me to generalize the notions of transitivity and permutability, which had already been employed in the theory of series, to systems of a larger number of dimensions. I lived with this idea for a week, leaving my work only for an occasional bite of black bread and Tilsiter cheese, which I bought at a delicatessen store. I soon became aware that I had something good; but the unresolved ideas were a positive torture to me until I had finally written them down and got them out of my system. The resulting paper, which I entitled *Studies in Synthetic Logic*, was one of the best early pieces of research which I had done. It appeared later in the *Proceedings of the Cambridge Philosophical Society* and served as the basis for the Docent Lectures which I gave at Harvard about a year afterward.

Mathematics is too arduous and uninviting a field to appeal to those to whom it does not give great rewards. These rewards are of exactly the same character as those of the artist. To see a difficult, uncompromising material take living shape and meaning is to be Pygmalion, whether the material is stone or hard, stonelike logic. To see meaning and understanding come where there has been no meaning and no understanding is to share the work of a demiurge. No amount of technical correctness and no amount of labor can replace this creative moment, whether in the life of a mathematician or in that of a painter or musician. Bound up with it is a judgment of values, quite parallel to the judgment of values that belongs to the painter or the musician. Neither the artist nor the mathematician may be able to tell you what constitutes the difference between a significant piece of work and an inflated trifle; but if he is never able to recognize this in his own heart, he is no artist and no mathematician.

Granted an urge to create, one creates with what one has. With me, the particular assets that I have found useful are a memory of a rather wide scope and great permanence and a free-flowing, kaleidoscope-like train of imagination which more or less by itself gives me a consecutive view of the possibilities of a fairly complicated intellectual situation. The great strain on the memory in mathematical work is for me not so much the retention of a vast mass of fact in the literature as of the simultaneous aspects of the particular problem on which I have been working and of the conversion of my fleeting impressions into something permanent enough to have a place in memory. For I have found that if I have been able to cram all my past ideas of what the problem really involves into a single comprehensive impression, the problem is more than half solved. What remains to be done is very often the casting aside of those aspects of the group of ideas that are not germane to the solution of the problem. This rejection of the irrelevant and purification of the relevant I can do best at moments in which I have a minimum of outside impressions. Very often these moments seem to arise on waking up; but probably this really means that sometime during the night I have undergone the process of deconfusion which is necessary to establish my ideas. I am quite certain that at least a part of this process can take place during what one would ordinarily describe as sleep, and in the form of a dream. It is probably more usual for it to take place in the so-called hypnoidal state in which one is awaiting sleep, and it is closely associated with those hypnagogic images which have some of the sensory solidity of hallucinations but which, unlike hallucinations, may be manipulated more or less at the will of the subject. The usefulness of these images is that in a situation in which the main ideas are not yet sufficiently differentiated to make recourse to symbolism easy and natural, they furnish a sort of improvised symbolism which may carry one through the stages until an ordinary symbolism becomes possible and appropriate. Indeed, I have found that there are other mental elements that may readily lend themselves for preliminary symbolic use in the solidification of ideas in mathematics. On one occasion during a bout with pneumonia, I was delirious and in considerable pain. But the hallucinations of my delirium and the vague reactions of pain became associated in my mind with some of the difficulties yet hounding me in an incompletely solved problem. I identified my suffering with the very real malaise that one feels when a group of ideas should fit together and yet cannot be brought together. However, this very identification gave me sufficient markers for my problem to enable me to make some real progress in it during my illness.

But life in Göttingen was not all research for me. I needed outdoor exercise, and I took my tramps with my English and American colleagues in the woods south of Göttingen and in the region of Hanover-Münden. My favorite lunch tidbit may seem rather indigestible, but it was cool and delightful: a Tilsiter cheese sandwich, a dill pickle, a glass of lager beer, and a raspberry ice.

There were many interesting things to see in Göttingen. There was a fair on the Walkenmühlenwiese, near our favorite swimminghole in the river Leine; and we were delighted to see the side shows and to hear the barkers of just the sort I had known at a New England carnival, against this unfamiliar background. I remember the different sorts of beer which I furtively tasted at the local Automat, and the bathhouse with its different classes of baths and its ample towels of the more expensive grades. I remember the two-hour classes, and the little buffet at which we bought sandwiches and Leibnitz-Keks in the fifteen-minute interval between the two halves.

The summer term was drawing toward a close, and the coming storm of the First World War made itself known in the papers by the heat lightning of the assassination at Sarajevo. The diplomatic ineptitudes that followed did not relieve the tension. Luckily, I had planned to return to America, and I had already secured my third-class passage on a Hamburg-American steamer.

I derived many lasting benefits from Göttingen. My contact with the philosophers was not very satisfactory. I do not have the type of philosophical mind that feels at home in abstractions unless a ready bridge is made from these to the concrete observations or computations of some field of science. From the mathematicians I also got relatively little in the formal courses. Landau's group theory course was a hard-driving plunge through a mass of detail with which I was not fully prepared to cope. I was able to follow Hilbert's course in differential equations only in parts, but these parts left on me a tremendous impression of their scientific power and intelligence. It was much more the meetings of the Mathematische Gesellaschaft which taught me that mathematics was not only a subject to be done in the study but one to be discussed and lived with.

Besides this, at Göttingen I learned to meet people both like me and different and to get on with them. This marked an important step ahead in my social development. The net result was that I left Germany much more a citizen of the world than when I first went there. I can say this very truly, even though I had not been fond of all aspects of the Göttingen environment and although in the war that immediately followed, I passed through a definitely anti-German phase. Yet when I went back to Germany during the confused years between 1919 and Hitler, for all the alienation I may have felt on political issues, there was a large intellectual element in Germany with which I had a sufficient common basis of past experience to feel myself a part.

My year at Cornell and my two years of graduate study in philosophy at Harvard had represented the continuance of my adolescence and my gradual introduction to independent research. They were satisfactory as far as my purely intellectual progress went, but they did not see me clearly out of the Slough of Despond. I was quite as aware as those about me that the way of the infant prodigy is beset with traps and snares, and while I knew perfectly well that my purely intellectual powers were above the average I knew equally well that I was to be judged by standards according to which a moderate degree of success would take on the appearance of failure. Thus I did not escape the floundering that generally goes with adolescence; and although this floundering was at a far higher intellectual level than that of the majority of teenagers, it represented a more than usually severe and doubtful struggle with the forces of uncertainty and of my own inadequacy.

It was the year in Cambridge and in Göttingen, however, that gave me my emancipation. For the first time I was able to compare myself intellectually with those who were not too much above me in age and who represented in fact the cream of the intellectual crop of Europe and even of the world. I was also subject to the inspection of first-rate men like Hardy and Russell and Moore, who could see me without the glamour of my precocity and without the condemnation which belonged to my epoch of confusion. I do not know whether I was outstandingly brilliant in their eyes, but at least they (or some of them) regarded my career as a reasonable bet. I was not under the immediate tutelage of my father and did not have to weigh myself in his somewhat loaded scales. In short, I had been initiated into the great world of international science, and it did not seem utterly hopeless that I should accomplish something there.

I was learning all the time how to comport myself as a social being and what the requirements were for living among those of other traditions and customs. My study in Germany represented an even greater break with my childhood, and an even greater necessity for me to adapt myself to foreign standards or at least not to come into a head-on collision with them.

The Sarajevo pot gradually went from a simmer to a boil. By the time I had got to Hamburg, there were posters on the street, calling on all Austrians subject to military service to return to their country. The city was full, and the *Christliches Hospiz* at which I stayed could only put me in a bathroom in the waiters' home which was their annex. I heard singing in the street at night and thought the war had come; but it had not, and I spent the time before dawn walking around the outer Alster basin.

I took the train for Cuxhafen, where I embarked on the *Cincinnati* of the Hamburg America Line. A day and a half later I saw the mobilization of the British fleet in Spithead, and about two days afterward we received the news that Germany and England were at war and that the radio station was closed. While we were bound for Boston, we did not know whether we should be able to make it, and at one time there was some talk that we might be sailing for the Azores. However, the sun showed that this was not the case, and we made Boston as intended. This ship was then laid up at a Boston dock until the United States entered the war, when it was taken over as an American transport and later torpedoed by the Germans.

My father met me at the boat, much relieved to find me safe and sound. We took the train together up to New Hampshire. I noticed that Father treated me with more respect than he had ever done: more as a grown man. We talked about the war during the train trip. I was surprised to find how definitely my father's opinion and the university opinion he represented had crystallized against Germany.

The war news was bad. We had hoped for a quick ending of the war, but the German line bit deeper and deeper into Flanders and France, and even when it was held by the taxicab army of Marshal Joffre, it was clear that we were in for a long, desperate, and uncertain war of positions. It was then the children of my generation knew that we had been born too late or—barely possibly—too soon. Santa Claus died in 1914. We surmised that life was to be such a nightmare as Kafka has since described, from which one awakes only to become aware that the nightmare is real, or from which one awakes into an even worse nightmare.

I had written to Bertrand Russell to inquire whether it was advisable to return to Cambridge on the new and augmented traveling fellowship which Harvard had granted me for the academic year 1914–15. He wrote that it would be safe and desirable, and I booked passage from New York on an old ship of the American Line. It dated from the days of auxiliary sail, and had a yacht bow and a bowsprit. It seemed enormously romantic to me.

My two aunts, who had by now risen in the clothing trade world and spent much of their time in Paris, saw me off at New York. The trip was slow but agreeable. There were young people aboard with me who tried to forget the war. We played a version of golf with shuffleboard sticks and disks, chalking the holes on the deck, and using ventilators, cleats, and deck-houses as hazards. There was an elderly couple from Australia who watched our antics benignly. They ran a sort of agricultural school when they were at home. I was later to see them in the grim London of the war, where their cordiality was a great consolation to me.

Thus I arrived in wartime Cambridge. The air was heavily overlaid with gloom. Part of the Backs had been turned into an improvised hospital for wounded soldiers. In all the vacant spaces of the university, ugly shacks were springing up, of a temporariness more devastatingly permanent than any intended permanency.

In the Union there were lists of casualties, and distressed fathers and brothers were reading them in the hope that they might not contain the names of their own kinsfolk, yet with the expectation that sooner or later these kinsfolk would appear on them. *Blackwood's Magazine* contained monthly installments of Ian Hay Beith's book *The First Hundred Thousand,* which brought to us a sense of the immediacy of the war and of a certain participation in it.

The news continued black and ominous. My friends and colleagues were scarcely able to take their intellectual work with full seriousness, and the blacked-out streets and the white painted curbs added to a general feeling of gloom and doom. Finally, we began to hear suggestions that the Germans were soon to undertake a great unrestricted submarine campaign against merchant and passenger ships.

It was embarrassing for me to meet the soldiers everywhere, in the movies, in the streets, and even in the classrooms of the university, and to think that as a foreigner I was immune to the universal sacrifice. Several times I thought of enlisting, but was deterred by the fact that after all it was not yet my war and that to go into it before my parents were ready to accept the situation would be in some sense a very serious disloyalty to them. Then too, with my poor eyesight, I was not exactly the best soldier material; nor did I desire to sacrifice my life for a cause concerning the merits of which I was not yet fully convinced. Though I definitely preferred the English and French side of the war, I had not yet been excited to that righteous pitch of indignation to which my father had been carried by a complex of emotions which I have already described.

I was impressed with that freemasonry which exists among the British ruling class, whatever their political opinions, which makes it possible to share the intimate knowledge of many secrets carefully concealed from the press and the public. My attention was called to this very forcibly during this second stay in Cambridge. I was in the habit of receiving from my parents copies of the old Boston *Transcript*, that former fount of ultrarespectability and of reasonably accurate news. In one number I read of the sinking of the British cruiser *Audacious*. I had not seen a word about the matter in my British newspaper. I went to Russell with the news, and he told me that it had been common knowledge since it occurred, and that the *Illustrated London News* had published a photograph of the ship with the caption, *"An Audacious Picture!"* and no explanation of the audacity.

Furthermore, Russell seemed to be well informed about every other detail of the war which had been concealed from the public at large. Yet, at this time, Russell was an extremely unpopular figure with the officials of the British government. He was a conscientious objector and a pronounced pacifist; and when later America entered the war, he expressed himself concerning the American government in such hostile terms that he was sent to jail and ultimately deprived of his position at Cambridge.

To me this combination of being officially on the black books of the government and still personally in a position to receive from his official opponents information that was refused to the public at large seemed a remarkable tribute both to the stability of England and to the assured position of its ruling class at that time.

By Christmas I could stand the gloom of Cambridge no more and I went down to London. I found rooms in a melancholy turning off Holborn and spent much of my time reading my landlady's books about old London and checking up the places mentioned as they were to be found at the present time. I found my Australian friends in a Bloomsbury hotel. I looked up another Harvard fellow in philosophy, T. S. Eliot, who, I believe, had taken Oxford to himself as I had taken Cambridge to myself. I found him in a Bloomsbury lodging, and we had a not too hilarious Christmas dinner together in one of the larger Lyons restaurants. I also looked up the Whiteheads, and found that war bereavement had already struck them.

Some time after I returned I received a telegram from my parents telling me that the submarine threat was growing worse, and that I should come home on the first possible boat. Actually, Cambridge had almost closed down, and there was very little point in my staying any longer. I decided to finish my year at Columbia, so I booked a passage from Liverpool to New York and ultimately made a gloomy train journey to Liverpool under depressing war conditions. My companions in the compartment were a group of soldiers absent without leave who jumped off the train at the first stop before we arrived at Lime Street and into the arms of the military police.

I have been on winter trips across the Atlantic which have been as calm and as agreeable as any trip in the full summer time, but this March voyage was not one of them. The old ship took it green up to the bridge, and nothing but a youthful resiliency kept me from being sick all the way. Among my fellow passengers the most interesting were a family of Belgian refugees who were leaving a temporary asylum at Cambridge, England, for a more permanent one at Harvard University in the American Cambridge. Professor Dupriez was a distinguished professor of Roman Law at Lovain, and a charming gentleman, but he was also an impractical little scholar of the European type. The practical brains of the family and its energy belonged to his wife, a queenly and downright Flemish lady. There were also four children, two boys and two girls, who were too young to have completely lost the adventurous pleasure of the journey in the depression of defeat and exile. We were to see much of this family in the next few years.

The boat arrived in New York, and I was met by my New York kinsfolk. I went to Boston for a few days to pick up the continuity of our family life, and then I returned to New York to finish my year's fellowship at Columbia University.

I found the skyscraper dormitories of Columbia depressing after Cambridge and Göttingen. I also found the life of the place unsatisfactory in its lack of coherence and unity. Almost the only bond between the professors, who lived widely scattered in University Heights apartment houses or in suburban bungalows, was an almost universal antagonism toward Nicholas Murray Butler and everything that he stood for.

I did not get along too well with the other men in the dormitories. There was no intellectual bond between us, and I seem to have been completely lacking in tact. I insisted on criticizing my seniors intellectually in a way not becoming a boy of the age of a college sophomore. I threw my weight about giving bits of information which were not welcome to the men about me who were predominantly graduate students. It is true that I did not always know that these bits of information were particularly recondite and unwelcome. I intruded upon bridge games between an established set of cronies without making sure that I was welcome. I should have been far more sensitive to the response that my conduct evoked. They used to pester me by setting fire to the newspaper I was reading, and by other such feats of heavy-handed buffoonery.

Following the advice of Bertrand Russell, I studied with John Dewey. I also took courses with some of the other philosophers. In particular, I listened to lectures by one of the New Realists, but I was only able to confirm my impression of an undigested mass of the verbiage of mathematical logic, completely uncombined with any knowledge of what it was all about.

My term at Columbia was a makeshift at best and although I began to develop the intellectual consequences of my own ideas, I did not get much help from my professors. Indeed, the only one of them who was a great name comparable to those I had learned to appreciate at Cambridge and Göttingen was John Dewey; and I do not think I got the best of John Dewey. He was always word-minded rather than science-minded: that is, his social dicta did not translate easily into the precise scientific terms and mathematical symbolism into which I had been inducted in England and Germany. As a very young man I appreciated the help and discipline of a rigid logic and a mathematical symbolism.

About the time that I returned to America, I was told that the philosophy department at Harvard would look out for me for the next year with an assistant's position, and that I would be allowed to give a free series of Docent Lectures which were the prerogative at that time of every Harvard Ph.D. Hence I began to prepare for my Harvard Docent Lectures.

My research in New York was devoted to an attempt to establish a postulational and constructive treatment of *analysis situs* within the ideational and terminological frame of Russell's and Whitehead's *Principia Mathematica*. This was in 1915, many years before Alexander, Lefschetz, Veblen, and others had succeeded in doing very much the same sort of thing that I then tried to do. I had pages and pages covered with formulae, and I made a certain very real progress, but I was disappointed because I considered the bulk of results which I obtained distressingly small in view of the large structure of presuppositions which I had set up to obtain them; consequently I never carried my investigations sufficiently far to put them into publishable form. In neglecting to do so I may very well have lost the chance to be one of the founders of what has become a most fashionable mathematical subject. However, my early start in mathematical logic, a subject at which many mathematicians arrived only after a mature consideration of other fields, rather cloyed me with abstraction for abstraction's sake, and gave me a somewhat exacting sense of the need for a proper balance between mathematical apparatus and the results obtained before I have been able to consider a mathematical theory as intellectually satisfying. This has led me more than once to discard a preoccupation with a theory which I myself had at least partly originated, and which, because of the ease with which it has furnished doctoral theses, has become a fashionable field of studies. Here I refer in particular to the study of Banach's spaces, which I discovered independently of Banach in the summer of 1920, only a few months after his own original work and before its publication.

In this connection, let me say that the fact that I came from a field of the most abstract theory has always led me to put a great value on richness of intellectual structure and on the applicability of mathematical ideas to scientific and engineering problems. I have always had, and I still have, great suspicions and reservations in regard to work that is thin and facile; and until we had the correcting influence of the sense for application required by war work, I cannot deny that a large part of specifically American work and not a little of that found abroad have suffered from a certain thinness of texture.

I used to explore on foot the whole of Manhattan Island, even as far as the Battery. I went for walks on the Palisades near the Jersey side of the river with Professor Kasner of the mathematics department. He then lived in a part of Harlem at the foot of University Heights, before Harlem had come to have its present significance. Kasner would tell me much of his ideas on differential geometry, and he was a pleasant walking companion, who knew a much wilder Palisades region than can be found at the present time.

My stay in New York also marked my introduction to the American Mathematical Society, and my first visual acquaintance with most of the elder scholars of the group. At that time the hotel headquarters was that pile of Gay Nineties respectability, the old Murray Hill Hotel. The society was then more of a New York institution than it is at the present time, for it had indeed been founded by a New York group, and had been known for some years as the New York Mathematical Society. There attached to it a little of a beer-hall flavor, which has evaporated with time and the increased prosperity and respectability of the scientist.

I spent Sunday and sometimes Saturday with my grandmother and other New York relatives somewhere up in the Spuyten Duyvil region of Manhattan. My relatives were very kind to me but I found the *gefüllte-fish* atmosphere of a north Manhattan apartment house a little stifling. On one occasion I ventured to accept the invitation of my cousin Olga to take a walk in the country with her and to visit a few friends. I should have given my grandmother this time, as she was old and diabetic and not likely to live longer than about a year. However, the indignation with which my mother received the news of my dereliction from duty was only in part due to her affection for my grandmother. It was at least in part due to her fear of my acceptance of Jewish environment in the more threatening form provided by Olga and the younger generation.

My mother's father died while I was at Columbia, and I saw my mother on the hurried trip through New York to Baltimore. Not long after that I received a telegram from my father summoning me home for an immediate conference. Short of money as I was always at that time, I rushed to the train and sat the night out in a coach. When I came home the awful news was broken to me. One of my former fellow students, who was an instructor at Harvard, had told the authorities of the philosophy department, who were considering my future career, that just before obtaining the doctor's degree I had bribed the janitor to show me the results of some of the examinations. I have already mentioned this incident and although my conduct was certainly not justifiable, bribery played no part in it. My father took me immediately to Professor Perry's office to confront me with my accuser, and I had the pleasure for once in my life to hear my father's magnificent repertory of invective applied, not to me, but to an enemy. The incident ended in my formal acquittal, but it did no good to me in my later search for a permanent job.

My stay at Columbia had not represented any part of my original plans for that year, and had been forced on me by the exigencies of the war and of my parental fears. It probably represents the low point of my academic career between the summit of my European trips and the gradual ascent by which I climbed to a position as a teacher and an independent research man. Here, if it appears thin, it is because it was in fact thin. Still, it taught me something about New York and something about academic life at a large city university. I had done a piece of my own scientific work which would have been important if I had at that time possessed the courage to see its originality and to bet upon it in the face of a general lack of interest in the new Analysis Situs.

16 Trial Run: Teaching at Harvard and the University of Maine

1915–1917

We returned to New Hampshire for that summer. Raphael Demos, whom I had already known as a student in the Harvard philosophy department, and two other young Greeks, Aristides Evangelos Phoutrides of the Harvard classics department and Bouyoucos of the School of Agriculture at the University of Michigan, met me there in midsummer, and we set out on a mountain-climbing excursion.

It was a long, wilderness trip, the first that I had undertaken without the guidance of my father. Besides the beauty of the landscape and the pleasure of now being taken on a tramping trip by my contemporaries, the trip was enlivened by the personality of Phoutrides, who was a poet in his own right and a reader of much of the best in modern Greek poetry. It was a great revelation to see our own White Mountains through the eyes of a man who had formed his mountaineering skills on Parnassus and Olympus, and associated his pleasure in the sport of it with the genuine tradition of the classics.

Not long after this, we returned to the city and to the duties of term time. I acted as a section instructor in a large freshman course in philosophy, as well as instructor in my own right in a course in logic. In my undergraduate teaching I had to keep a record of attendance, read papers for one or two professors, and conduct a section at Harvard and a section at Radcliffe on elementary philosophy. I was in fact about the same age as most of my students in these two sections. I don't think I had very much stage fright in teaching, but it took a certain amount of courage to stand up before a crowd, particularly a crowd of girls about my own age, and hold the discussion to a progressive and orderly course. I don't know how I got away with it, because, as is natural with insecure beginners, I was far more pontifical then at the age of twenty-one than I am now at the age of fifty-eight. Still, Harvard and Radcliffe classes are traditionally tractable and I was interested in my subject and always ready for a dialectical fight.

I was grateful for the gift of gab. It is easier to prune a youthful loquacity and excessiveness than to cultivate the art of saying something when the words won't come. Besides, I was protected by my very inexperience from the awareness of what a show I was making of myself. This awareness came later, the next year, when I left the amenities of Cambridge for contact with the raw facts of life in the woods at Orono, Maine. Then I was to pay severely for my ineptitudes in classes that delighted to show up my powerlessness at discipline.

In addition to teaching, I had somewhat special duties. Professor Hattori of Tokyo was giving a series of courses on Chinese and Japanese culture and philosophy, and he needed the help of a young American in the routine work of taking attendance and of grading. I took on the job and found that it stimulated my interest in the civilization of the Orient, to which I had already been led in dealing with the problem of my Jewish origin through my interest in the general problem of undervalued peoples. This interest in the Orient was reinforced by the fact that my particular crony of that year, with whom I used to go tramping in the Middlesex Fells and the Blue Hills, was Chao Yuen Ren, a brilliant young Chinese, who had left graduate study in physics at Cornell to study philosophy at Harvard and who was equally versed in phonetics and in the study of Chinese music. Chao has continued to be my close friend all these years, and in the periods in which I have been unable to see him, I have received the *Green Letters* by which he solved the problem of his very extensive correspondence. These are printed documents of considerable length and merit by which he keeps his friends *au fait* with his affairs.

I may say in passing that Chao Yuen Ren has become perhaps the greatest philologist of Chinese and one of the two leading reformers of the Chinese language. He was Bertrand Russell's interpreter in China; and he married a charming Chinese woman doctor who is also the greatest interpreter of Chinese cookery to the West. They have four daughters, of whom the oldest two were born in the United States and are now married, and who aided their father during the late war in teaching Chinese at Harvard.

These friendships made me very conscious of the role of the non-European scholar at American universities. I was living through a period in which great changes were taking place in the relative and the absolute role of America in world science. Properly speaking, these changes were only a part of the general process by which countries have come up and gone down in their creative activities. This has since been evidenced by the way in which the overwhelming primacy of Germany had been reduced by emigration, war, and hardship. What I then found and still find more striking and significant were the changes in lands previously foreign to European culture, such as China, Japan, and India, and in the new colonial countries. Many of these have come within my lifetime to take a major share in the Western intellectual world.

Besides my regular courses, I also gave what was called a Docent course in constructive logic. For a number of years, Harvard granted to every Harvard Ph.D. the right to carry on a series of lectures in his chosen field without pay and without authority for the students to count them for a degree. Otherwise, the lectures were officially recognized by Harvard University. I have already mentioned that it was my intention to supplement postulational methods by a process according to which the entities of mathematics should be constructions of higher logical type, formed in such a manner that they should automatically have certain desired logical and structural properties. The idea had an element of soundness, but there were certain difficulties that I had not foreseen and evaluated, which depended on the fundamental finiteness of scope of our experience. My work was closely connected with Bertrand Russell's notion of perspectives, and I suspect that both pieces of work shared many of the same advantages and disadvantages.

At that time a star of the first magnitude had appeared on the firmament of the Harvard mathematics department. It was G. D. Birkhoff. In 1912 Birkhoff, then twenty-eight, had astonished the mathematical world by solving an important problem in dynamical topology which had been put—but never solved—by Poincaré. What was even more remarkable was that Birkhoff had done his work in the United States without the benefit of any foreign training whatever. Before 1912 it had been considered indispensable for any young American mathematician of promise to complete his training abroad. Birkhoff marks the beginning of the autonomous maturity of American mathematics.

He had continued his work on dynamics of the sort that Poincaré had previously discussed, and was giving a course on the problem of three bodies. I enrolled in the course, but whether it was due to my insufficient preparation or to Birkhoff's rather difficult expository style, or in all probability to both, I made heavy weather of it and was unable to continue.

Both Birkhoff and Münsterberg were among the auditors of my Docent course. As the war went on, Münsterberg found his position at Harvard more and more difficult. He took the German side, whereas most of his colleagues, including my father, took the Allied side. Finally, Münsterberg wrote to my father a letter which my parents took to be insulting, and there followed an active quarrel in which Münsterberg alluded to his interest in my work and to his attendance at my course and his support of it. Naturally, the situation could not have been more embarrassing to me, and I showed more loyalty than tact in taking up the cause of my father.

While I had occasionally attended the Harvard Mathematical Society during my previous stays at Harvard, I now began for the first time to attend it regularly. It was an institution formalized in the typical Harvard way. The professors sat in the front row and condescended to the students in a graciously Olympian manner. Perhaps the most conspicuous figure was W. F. Osgood, with his bald oval head and his bushy divaricating beard, spearing his cigar with his knife after the pattern of Felix Klein, and holding it with a pose of conscious preciosity.

Osgood typified Harvard mathematics in my mind. Like so many American scholars who had visited Germany at the beginning of the century, he had come home with a German wife and German mores. Let me add that there is much to be said for marrying German wives—I am happy to have done so myself. In Osgood's time this New England pseudo-Germanism had been the fashion of the day. His admiration of all things German led him to write his book on the theory of functions in an almost correct German. There was no question that he had been impressed with the status of the German *Geheimrat,* and that he had longed to model academic life in America into such a form that he could imagine himself in such a position. He had done able work in analysis, against the resistance of those inhibitions that perpetually drive a certain type of New Englander from the original into the derivative and the conventional. Some of his ideas should have led him to the discovery of the Lebesgue integral, but he had not brought himself to the final step which might have led him to accept the striking consequences of his own conception. He must have had some rankling awareness of how he had missed the boat, for in his later years he would never allow any student of his to make use of the Lebesgue methods.

Another representative of the German period of American mathematical education was Professor Maxime Bôcher. He was the son of a former teacher of French, but he had been educated in Germany and had married a German wife. As in the case of Osgood, German was the language of his house; but in other ways he departed from the Osgood pattern. His work was more original and on a broader basis, and his personality was free from easily traceable mannerisms.

Of the other professors of mathematics perhaps the two that impressed me most were Professors Edward Vermilye Huntington and Julian Lowell Coolidge. I have already spoken of Huntington, whose very originality had collided with his Harvard career. He had been relegated to the Lawrence Scientific School and to the teaching of engineers, although his ability was greater in the direction of pure mathematics and logical ingenuity. He has lived to see his heresy become an orthodoxy, and today the postulational method attracts even more than its due share of Ph.D. candidates. He was an excellent, inspiring, and patient teacher.

Under the Lowell regimen, a kinsman of Lowell himself and a descendant of Jefferson, such as Julian Lowell Coolidge, was bound to be among the favored. Coolidge had been educated in England and in Germany. His work was in geometry, in which he had shown great diligence and industry. With his wit, he had managed to synthesize an amusing personality, and he had succeeded in turning an inability to pronounce the letter *r* into a rather attractive individual trait.

To give a paper before the Mathematics Club constituted a real training in skill of presentation and in logic. Originality and power were not at a premium. Power in mathematics consists in possessing tools, conventional or otherwise, which enable one to solve a large part of the previously unsolved problems which one encounters in the course of one's work. It is the ability to create or to develop methods to match the demands of the problem—and it was not in high esteem in that environment. There was at that time no place where the interests of the advanced scholar beyond the early graduate-student stage were paramount, although such an organization has developed since in the form of the Mathematical Colloquium, which now has taken over a large part of the functions of the Mathematics Club.

For my physical exercise I combined hiking with a certain amount of wrestling in the gymnasium. Wrestling is one of the sports in which a myope may participate without unduly serious physical disadvantage. I never was really good at wrestling, but I was heavy and strong, and particularly strong about the shoulders, so that I gave better wrestlers a certain amount of exercise in banging me about. For a while I was a mass of boils and mat burns, in the best tradition of the sport.

When I returned from Cambridge and Columbia, I returned to an atmosphere of authority in family discipline which was not much less intense and all-pervading than it had been in my student days. Still there was one difference: I was no longer my father's pupil in any subject. The old idea of the family structure remained, but this time it hinged on the fact that I was now a breadwinner in the full sense and that I had begun to have a certain stature in my own right. Yet it was not until much later after my marriage that I can say I ceased to be in my father's eyes fundamentally the child from whom obedience was demanded.

From the time I returned from Europe, my parents had been accustomed to having Sunday teas for my father's students. My own students were also invited as well as the fellow students of my sisters. The Sunday teas of the professor with adolescent and postadolescent children is indeed a hoary tradition; nor has it changed its original intention. When I read in Thackeray of the Professor of Phlebotomy at Cambridge and of his attempts to introduce his daughters to promising undergraduates, it strikes an echo in my own memory. Nevertheless, I should be the last person in the world to poke fun at these teas, for they did in fact furnish me with the basis for a social life when other bases were scant, and my sisters as well as I first met our future spouses on these occasions. I learned much about how to handle myself socially, and I began to develop friends and acquaintances.

My father was Professor of Russian at Harvard, and it fell to his lot to extend courtesies to Russian visitors. During the war there were a great number of them, first on missions of varying importance involving the Russian government and later the refugees from the threatening storm of revolution or from the revolution itself. These were men and women of varying caliber. Some of them were on serious missions, such as winding up a purchasing campaign made on behalf of the Tsarist government. Some were there for the sake of their own hides and very little else, and these young elegants came to our teas, played Russian songs on our piano, and philandered all over our house. Even among these, there were some who had sufficient serious ability to make good in the new land; but there were also those whose connection with life was about as tenuous as that of froth with beer. For a while my mother and father were charmed with the social position of being hosts for this group of aristocrats, and my parents would compare their courtesy, sophistication, and *savoir-faire* with my own gaucherie, much to my disadvantage; but I was perfectly conscious all the time that our home was nothing but a backdrop to the amorous ballet of these delicate souls, and that they regarded us with utter indifference if not with contempt. In particular I knew that if I had for a moment imitated these refugees in their backstage conduct as well as in their frontstage elegance, and if my parents had got even a suspicion of it, I should have been relegated to outer darkness. Finally my parents became aware that there was something a trifle contemptuous of ourselves in the Chekhov garden party behavior of these exotics, and their visits became rarer and rarer and then ceased altogether.

My father had been desperately opposed to the Communists from the beginning. At least part of this was because his intimate connections with Russia were with such men as Milyukov, who was a Menshevik and associated with the unsuccessful Kerensky regime. The ideal thing for my father to do would have been to retain sufficient contact with the new Russia, whether he liked it or not, to enable him to understand the detail of what was happening, and even to offer warning to the Government of the United States of whatever might have been the new dangers. At any rate, from the time of the revolution on, not only did my father's researches drift further and further away from Russia, but thread after thread of his personal contact with that country was broken. He published a book on Russia and how it might be seen from the American point of view, but it was based exclusively on water that had long since run under the bridge. In short, Father's alienation from Russia led to an alienation from his own Harvard responsibility vis-à-vis Russia, and I have no doubt that his defection from Slavic researches was a factor that later led Lowell to view unsympathetically any request of my father to continue his Harvard connection well beyond the normal age of retirement.

Public opinion was turning even more toward the Allies, and it had become most likely that we should enter the war on their side. In the second term, there was founded an officers' training organization known as the Harvard Regiment, which I promptly joined. In the depth of winter, we would trudge through the snow in sleazy summer uniforms to the baseball cage on Soldiers' Field, where we were initiated into the School of the Soldier and the School of the Squad. When spring came, we continued our exercises out of doors behind the Harvard Stadium, and made several routine marches and other excursions. We also marched out to the State Rifle Range in Wakefield, where for several days we were trained in the art of musketry. Despite my poor eyesight, I made for once in my life the grade of sharpshooter. This does not redound to my credit but rather to the credit of my instructor, a Mr. Fuller, a Boston broker.

The Harvard Regiment left us hanging in the air, but I had plans to go to Plattsburg in the summer and train for a commission in the reserve. All this was, of course, dependent on my finding a job for the ensuing year. I met various deans and heads of departments looking for new staff candidates, but none of them seemed particularly interested in me, and I was assured by Professor Perry that I was not good enough to merit much of a recommendation. I was not a very promising bet at that time, but I cannot help believing that some part of my department's coolness was based on my lack of years and on a conservative unwillingness to experiment with the unknown.

Yet my difficulties were in part a consequence of my development. A year earlier, my assistantship had come easily, since it did not imply any competition with older and more approved Harvard men for anything they really wanted. Now, at the end of my year, matters were different. I was asking for an instructorship, and with it an entrée to a career in the field where desirable jobs were few and far between. This was more than Harvard teachers were willing to bestow on someone hard to manage and without a clear pattern for his future.

Finally, under pressure of my father, I decided to look for a job in mathematics rather than in philosophy and in a way I considered rather humiliating: by registering my name with a number of teachers' agencies. It is a procedure akin to fishing, in which the nibbles are much more frequent than the bites. Finally, I did get a bite, and I agreed that I should spend the next year as an instructor of mathematics at the University of Maine in Orono, Maine. We returned to The Top of the World, our summer home in Sandwich Township, for the summer.

We had another visit from Raphael Demos, and from Jim Mursell, a young Australian student from the Harvard philosophy department. Mursell, Demos, and I went on another tramping expedition north to Mount Washington by way of the Webster train. When that was over, I went to the Officers' Training Camp at Plattsburg, New York, to try for the commission in the Army that would be so desirable when the United States should enter the war.

I took the autostage from Sandwich Lower Corner. On the lake steamer across Lake Champlain I found a youthful school fellow, the unregenerate scoundrel from Walker Street who had chased another boy with an ax, and who had become one of the most promising young swindlers in Massachusetts. He tried to palm off on me as an officer a cavalry private with whom he happened to be traveling, but I had become sufficiently acquainted with army insignia not to be completely misled by the resemblance of the yellow hat-cord of the cavalryman with the gold-and-black hat-cord of the officer.

The Harvard Regiment had prepared me in some measure for the army camp. But I was still somewhat shocked by the bottle-drinking and foul-mouthedness of even those gentleman sham-soldiers. There were only one or two men in my company with whom it interested me to talk, although more than one New York society buck was present. The company mate who attracted me most was a member of a missionary family from Burma, who represented a continuation of the great missionary tradition of Adoniram Judson.

My mountain hikes had put me in good training, and I was in reasonably hard condition to stand the route-marches and sham battles. I was astonished to observe, even in myself, the difference in our conduct caused by the fact that we were members of a large, similarly directed group. For example, I would normally never think of bathing naked by the side of a traveled highway. However, when there are a hundred naked bodies already in the river, one cannot perceive in one's own nakedness any particular additional insult to public decency.

Again, one day when striking across the company street of pup-tents, I accidentally crushed a man's glasses. My normal instinct would have been to make myself known to him and to pay for them; but in the presence of so many uniformed and not very responsible youngsters, I am afraid that I hit and ran.

I was rather miserable during the part of our training devoted to musketry. Without the special coaching that I had received from Mr. Fuller when I was in the Harvard Regiment, my eyesight did not permit me to hit a barn out of a flock of barns. When I explained my shortcomings to the musketry officer and went back to my tent, my tentmates began to accuse me of malingering. They had already learned how easily they could make me squirm by their obscenity, and I was completely miserable. I was so angry that I laid my hand on one of the rifles stacked in the tent, with no intent whatever to use it as a rifle and very little intention of using it as a club, but more as a gesture of anger and despair than anything else. Of course, they disarmed me without any trouble, but I was unspeakably shocked when I saw clearly for the first time the murderous construction that could be put on my actions.

I finished the camp without being recommended for a commission, and with no particular feeling of accomplishment. I returned to the mountains for a week or so, and then made my way to Orono, and to my new job at the University of Maine.

I found Orono a rather raw and less inviting copy of the New England towns to which I had been accustomed. I made arrangements for board at the Orono Inn, which was a sort of commons for junior faculty members, and took lodgings in a rather attractive white New England house owned by the university librarian.

Although it was a great satisfaction for me to have a job beyond the immediate supervision of my father, I was not happy at Maine. The older, permanent professors were for the most part broken men, who had long given up any hope of intellectual accomplishment or of advancement in their career. A few of them still showed ragged traces of cultural ambitions, but the greater part were resigned to their failure. The younger men were almost all transients like myself, who had been bought wholesale at teachers' agencies after the cream of the crop had been skimmed off through the special efforts of their professors. The men who remained, the traveling guests of the university, had no interest whatever in the place, and had as their sole ambition to leave it as soon as possible, before their employment there should fix on them too definitely the stigma of unemployableness at more desirable institutions. Few and far between are the individuals who do not perish of intellectual atrophy at such places.

The president was an importation from the Middle West, and well aware of his authority. The students in those days were largely a strapping lot of young farmers and lumberjacks, who managed to be quite as idle and collegiate as the students of universities of the Ivy League, but at one-third the expense. Their sole interests were in football and in nagging the lives out of their professors. As I was young, nervous, and responsive, I was their chosen victim. Most of my courses were dull routine to them, and many is the penny which I heard dropped in class to annoy me.

Examination cribbing and the copying of homework had been reduced to a system. I soon found that to report such irregularities, as I had been told was my duty, reflected much more seriously on me than on the miscreants. I also found that some of my colleagues, within and without the department, resented my ignorance of and indifference to the very rigorous social protocol of the small college, my precocity, and what they considered my intellectual pretensions.

I tried to get back to mathematical research work. Dr. Sheffer of Harvard had recently suggested to me a way in which mathematical logic could be based on one single fundamental operation. I followed out his suggestion in a somewhat modified form, and published a paper in my name only. I think I gave credit to Dr. Sheffer in the paper, but I do not think that I did so adequately. I now see that my modification of Sheffer's work was scarcely sufficient to have merited publication as a separate paper, and that I should have waited until he had staked his claim in a more definite form. It is not only medicine and law that have a rather rigid code of ethics, and all the good will in the world will not make such a code come to one as a habit until he has at least a certain modicum of experience of the customs in the matter. Luckily, neither Dr. Sheffer nor my other colleagues in mathematics held my offense against me, but it worried me deeply when I came to realize what I had done, and it has been on my conscience to some extent to this day.

My parents were very much discontented with the generally gloomy tone of my letters from Orono. However, I must say that they put themselves out to give me a good time during my brief absences on ticket-of-leave. It was during these times that I learned the beer-and-sauerkraut pleasures of Jacob Wirth's restaurant in Boston and the delights of the new theatrical repertory company which had just been established at the Copley Theater. I also saw more of the rather primitive movies of the time than I had ever seen before, and was now and then allowed to meet some of the Radcliffe fellow students of my sister Constance. But even in the midst of my Boston amusements, there was the dread of my impending return to Orono.

Ultimately I was accepted as a member of a small research group at Orono and the neighborhood. The chief spirit of the group was the statistician, Raymond Pearl, who later had a distinguished career in the medical school of Johns Hopkins University. In his little house by the trolley line which ran between the village and the university, the chosen guests could hear good talk and a proper valuation of ideas. In those days when the English Cambridge seemed infinitely far behind me and the prospect of a civilized career infinitely far ahead, those visits to Dr. Pearl's house made me feel alive again.

Another of the few scholars of the University of Maine was Miss Boring. She was a zoologist and a sister of the psychologist Boring who had been my fellow graduate-student at Cornell. I was again to meet Miss Boring many years afterward in China, when she was teaching at Yenching University and I was teaching at the neighboring Tsing Hua University.

There were also a few doctors of the Bangor General Hospital in our group. I remember some very interesting lectures on pulmonary cancer, which were given long before this mimic of tuberculosis had been generally recognized as a clinical entity.

The meetings of this group did not constitute the only occasion for my going down to Bangor on the trolley. Bangor was no longer the riproaring town where the returning lumberjacks first tasted the pleasures of women and bootleg liquor, but the bad old days had put a blight on it and it lacked the charm of more favored New England cities. What called me there was a military drill corps whose meetings in a gymnasium were attended by good portly Bangor citizens much older than I.

The winter trolley ride to Bangor and the train trips to the city had a charm of their own. The whole landscape was blanketed with deep white snow, which was so cold that it creaked when you stepped on it. The air shocked the lungs like a cold fire. The time had not yet come when the auto roads should be kept open all the winter, and the sound of sleighbells was in the thin air.

There was a Norwegian student living in the same lodging house who was specializing in paper manufacture. The University of Maine was a center of instruction in this subject, and indeed the air reeked of the sulfurous fumes of the local paper mill. The Penobscot backwater opposite our house was divided by the cribs and booms which held back a supply of pulpwood floated down from the north. My Norwegian friend used to ski to school, and to make many ski trips across the snow-filled marshes and woods. The rest of us, students and professors alike, had still to learn this Nordic sport, and went to our classes on the snowshoes manufactured by the Indians in Oldtown nearby. There was always a forest of snowshoes stuck tail down in the snowbanks outside the doors of the college buildings, and the students, boys and girls alike, wore to their classes the woollen stockings and the Barker boots or pac-moccasins which mark the northern woodsman.

Time hung heavy on our hands. I remember reading through the whole of the writings of O. Henry and Mark Twain in a dark corner of the university library stacks, but unfortunately the present vogue of the detective story had not yet developed. I was greatly distressed during the winter both by the coming participation of America in the war and by the news of the death of my friend Everett King. He had been my companion in much boyish experimentation, and I am certain that if he had lived he would have become an important figure in American science.

Spring began to break with the wet suddenness which is characteristic of spring in northern New England. There were one or two new faces to be seen about the campus. I remember one pleasant newly arrived young American married to a French wife. He endeared himself to me by letting me accompany him on fishing excursions to the edge of the vast wilderness on the other side of the Penobscot.

There was little question that we were soon to enter the war. The existing officers' training corps was much enlarged, and every possible drillmaster was pressed into service. In view of my experience in the Harvard Regiment and at Plattsburg, I too was pressed into service; but I proved to be both too unskilled and too peremptory in my commands, and I did not make much of a success of it. When the war finally came, I asked to be released from my teaching responsibilities to enter some branch of the service, for

I was no less eager to leave Maine than the university was to see the last of me.

I passed some sort of preliminary physical examination with the friendly connivance of a Bangor doctor, and left on the steamer for Boston to try my fortunes with the services. On the way it occurred to me almost for the first time that I might be taking a real risk both of life and of limb, and I was distressed. However, I reasoned with myself that I had a good chance of coming out of the war with some sort of usable body attached to my soul.

When I arrived in Boston, I made the rounds of the harbor forts and the enlistment bureaus, in the hope that I could enter one of the services, if not as an officer, at least as an enlisted man. My eyes were against me everywhere. Finally my parents decided, with my acquiescence, that I should try for an army commission in the R.O.T.C. which had just been established officially at Harvard.

With the coming of the war, the new Officers' Training Corps became a much more systematic outfit than the old Harvard Regiment. We were quartered in President Lowell's new freshman dormitories, which were later to become the units of the Harvard house system.

We received some special lectures from a group of academically trained French Army officers, one of whom, Major Morize, remained for many years as professor of French at Harvard. During the summer, we entrained for Barre Plains, where we encamped and held maneuvers. I remember some of our trench-digging, mock fighting, and bayonet instruction. I was there only part of the time because examinations for commissions in the artillery were being held in the new buildings of the Massachusetts Institute of Technology. I knew that this was about my last chance to obtain an artillery commission for the war. Naturally, I did rather well in my mathematical examinations, but I could not show any particular military aptitude. I failed dismally in my physical examinations and in an examination on horseback riding at the state armory. I came totally unprepared for this, and I fell off an old nag which was as steady as a gymnasium horse.

As to my physical examination, my eyes would have damned me anyhow, but I also showed a blood pressure which was high for my age, although my continued survival proves to my satisfaction that it was not within any dangerous limits. The Army doctors probably took it quite correctly as showing that I was of too labile a temperament to be good army material. What chances I might have had for a commission were damned by the fact that, in accordance with the pseudonoble mores of the time, I tried to bluff and argue one of the doctors into giving me a favorable medical report, so that he frog-marched me ignominiously out of the room.

I graduated from the R.O.T.C. with a document that was eminently not negotiable for a commission. The end of the summer was near, and I spent what was left of it at Silver Lake in New Hampshire. I did some reading on the algebraic theory of numbers, which I had begun to study while I was in Maine, and I made a few attempts to extend Birkhoff's results on the four-color problem.

This, together with Fermat's last theorem and the demonstration of Riemann's hypothesis concerning the Zeta function, is one of the perennial puzzlers of mathematics. Every mathematician who is worth his salt has broken a lance on at least one of them. I have tried to solve all three, and each time my supposed proof has crumbled into fool's gold in my hands. I do not regret my attempts, for it is only by trying problems that exceed his powers that the mathematician can ever learn to use these powers to their full extent.

We lived near Professor Osgood's summer home that summer, and I saw a great deal of him. He was a much more genial personality in a New Hampshire summer cottage than he had seemed to be in the full glory of his Harvard position. I also did a bit of mountain climbing, and it is the proudest exploit of my youth that together with my sister Constance and a friend, I covered thirty-four miles in one day in a tramp over Mt. Passaconaway, Whiteface Mountain, and back. Of course, I was exhausted and feverish the next day; but one is resilient in one's twenties, and the effort seemed to do us no harm.

17 Monkey Wrench, Paste Pot, and the Slide Rule War

1917–1919

When we returned to the city, it was obvious that I should have to look for some civilian form of war work. My search for nonacademic employment was precipitated by the war and by the nearly total suspension of normal university life which it involved. I felt that my mathematical training was the most useful thing I had to offer, so I took the trolley out to the Fall River Shipyard in Quincy to see if I might not learn to be of use in ship-propeller design. Nothing came of this, so I made a similar trip to the General Electric factory in Lynn. One of the engineers there was a Russian friend of my father, and another had been one of the instructors in my physics course at Harvard. I naturally met with a more favorable reception. I was told that I could be of no use immediately; but if I were willing, they would take me on as a paid apprentice in their program of engineering training. This meant that I took on the moral responsibility of staying for two years. I accepted, and started to work in the turbine department. I helped run some steam-consumption tests, and used a little mathematics in some thermodynamic problems. When I returned home each day, I was tired but happy, and hopelessly begrimed by that grease which belongs to an engineering factory and which no soap seems able to remove. I took this as the badge of the honest workman.

However, my father was convinced that with my clumsiness I could never really make good at engineering, and began to look around for other work for me. He had written an article or two for the *Encyclopedia Americana,* then located in Albany. He secured from Mr. Rhines, the managing editor, an invitation for me to join them as a staff writer. Though I felt morally obligated to stay on at the General Electric Company, I was too dependent on my father to dare to contravene his orders, so I had to present my shamefaced resignation to the engineers who had given me my chance in Lynn. I was told that I never could come back to a job there, but with my helplessness and lack of independent experience, there was precisely nothing that I could do about it but to follow orders.

Father accompanied me to Albany and saw me placed with a rather pleasant landlady in an old high brick house not far from the State House. He also accompanied me to Mr. Rhines's office. This was situated several flights up a freight elevator in a dismal business building facing on a cabbage-strewn market of the European type. Mr. Rhines was a bearded old gentleman, efficient and strict but kindly.

Albany appealed to me from the beginning. In many ways the downtown part resembled a European city, or Boston's Back Bay. I found good restaurants at which to eat, a good vaudeville theater, and good movies at which to pass the time. I also found a gymnasium at the local Y.M.C.A. at which I could keep myself fit.

My *Encyclopedia* work consisted in the compilation of some of the less important short articles; and I believe that it was paid on a piece basis. I soon found that I was working in a not too uncomfortable Grub Street, with a group of cheaply paid colleagues who were either on their way up or on their way down. Among us was an elderly English businessman who had failed once and was now too old to start on a new business career. He prided himself on his knowledge of the Gilbert and Sullivan operas and his ability to write both words and music in the same style. Another of our writers had risen from engine-driver on a British railway to become librarian (or keeper of the morgue) for the London *Times*. He had a portfolio full of proof sheets of obituary notices of famous men still alive, written at the time of their illness, or (if they were sufficiently important) permanent documents in case of their sudden death. He had lost his position through drink, but had still enough ability left to make himself very useful on the *Encyclopedia*. His fund of stories was mostly improper, but generally improper in an amusing way.

We had also an Irish ex-seminarian, belonging to the Dublin of James Joyce, who sported that delightful literary English of the educated Dubliner, with just the hint of the possibility of a brogue. There was a young American lexicographer who used to play tennis with me and later became chief editor of the *Encyclopedia*.

There was a young girl graduate of Cornell among us. She was the daughter of a Russian-Jewish Albany fur merchant. I found her very attractive and intellectually stimulating. We used to take walks in the country about Albany, and I would call on her at home and take her to the visiting theatrical shows. She and I were two members of the group young enough to be definitely on the up-grade. We enjoyed the eccentricities and the friendliness of the older people who had found a refuge in this queer but agreeable *faubourg* of Bohemia. Nevertheless, as approximate contemporaries there were many minor amusements of the town which we shared with one another rather than with others of the *Encyclopedia* group. Even when I found that she was engaged to a young doctor who was in the Army in France, I continued to take her out from sheer hunger for female companionship.

We used to alternate our work between the office and the New York State Library in the Educational Building near the Capitol. This building held a real fascination for me. Besides being a really good library, and, I believe, the headquarters of the State Board of Regents, it contained the New York State Museum with its mixture of geographical, geological, anthropological, paleontological, botanical, zoological, and petrographical-mineralogical-crystallographic collections. I used to spend much of my spare time in the museum, and perhaps a good deal of time that should not have been so spared. I made the acquaintance of one of the curators, who was a specialist in crystallography and in gems, and burrowed into the article on crystallography in my beloved *Encyclopaedia Britannica*. I also saw a good deal of one of the state paleontologists. These contacts rearoused my interest in the origin of the vertebrates, and I reread Gaskell and Patton to see if there was any possible way of making sense of the arachnid theory of vertebrate origins.

I found that the hack work of compiling an encyclopedia has a peculiar ethics of its own. A compiler should be exactly what the word implies. It is permissible to use information from other existing encyclopedias, if it is carefully collated with outside sources. If you must crib, crib from foreign-language encyclopedias, and do not favor any single source excessively. Be very careful before you give any article a by line. In general, eschew your own original ideas.

It took me some time to become so familiar with the rules that they were at last instinctive; and more than once I succumbed to the boyish urge to cut corners. I reinforced my habit of reading encyclopedias by the habit of writing them, and the considerable scope of my existing information was useful. In one or two articles, such as a part of the one on "Aesthetics," I deviated into individual philosophical writing, and the material still looks fresh to me at the present day. I toyed with the idea of collecting into a small book a number of such articles and of my previous writings on philosophical topics. Besides such reasonably original and good articles, however, I ventured to write on a number of subjects on which I was not adequately prepared. Some of the articles I submitted on ballistics were the most utter balderdash. I hope that Mr. Rhines did not let them get through.

With all the shortcomings and unpleasant sides of hack writing, it was a wonderful training for me. I learned to write quickly, accurately, and with a minimum of effort, on any subject of which I had a modicum of knowledge. In revising my material, I learned the marks and the technique of the proofreader.

The problems of literary style are curiously related to the problems of speaking a foreign language. The experience which one acquires in hack work is quite parallel to the experience of being immersed in a foreign milieu and having to speak the language day after day. For it cannot be denied that literary English as it should be written, although it has deep roots in colloquial English and can only depart too far from the colloquial at its peril, is in many ways a distinct language. For example, the richness of metaphor and of term of speech appropriate to an effective literary style would be heavy and "donnish" in spoken English. Thus the problem of writing for a man who already can speak with reasonable effectiveness is to develop the same freedom in this sophisticated milieu that he already has in his daily speech. If I am to speak Spanish effectively, I must think in Spanish and I cannot be tempted to translate out of an English phrase book. I must say the sort of things that a Spanish-speaking person would say, and these are never precisely the same as those which an English-speaking person would say. Similarly, if I am to write a poem or a novel or a scholarly essay with any ease, I must have had sufficient practice in expressing myself in the suitable form of the language so that my written or dictated words come out in what is already effective poetic English or the effective language of the novelist or that of the philosophical essayist. My metaphors and terms of speech should find themselves without having been looked for, not indeed in their final polished form but in a form not too remotely different from that. I do not deny the virtues of revision and of the progressive weeding out of weaknesses and faulty expressions. Nor indeed do I wish to prescribe to other writers what is of necessity an intensely individual expression of their own thinking. But at least as far as I am concerned, whether I work in mathematics or in writing, I cannot arrive at the fullest expression of my own consciousness until I have already penetrated considerably below the level of consciousness.

I was happy in Albany. I liked my co-workers and employers, I liked the work, and I liked my new sense of independence. In view of the difference in the nature of my new work from that of my father, I was even less under parental pressure and criticism than I had been at the University of Maine. I was also older and more fit to stand on my own feet. Compared with Orono or Bangor, Albany was a paradise of neatness, tradition, and civilization.

Behind my new-found happiness and content, there was always the hollow echo of the war. The R.O.T.C. experience had proved to me my essential unfitness for a commission, but I had still some hopes of being admitted for limited service as a private under the new draft. For the time being, I joined the New York State Guard. This was the organization that occupied the armories when the National Guard went out to fight, and it had the duty of patrolling water supplies and powerhouses. I was not called on for such semiactive duty, but my earlier training in drill stood me in good stead inside the armory. When spring came, we used to spend Saturdays and sometimes even Sundays on an island in the Hudson where there was a rifle range.

I used to return to Cambridge for short vacations from time to time. My sister Constance made some effort to take me into the social life of her Radcliffe friends, and I remember that there was an Australian girl with whom I went out occasionally. I spent a short summer vacation in New Hampshire, then returned to my work on the *Encyclopedia*. But it was by now apparent that this sort of work, while very acceptable as an instructive effort and a way station on my career, would be very unsatisfactory as a terminus.

I was not distressed to find myself engaged in work that was enjoyable even though it led to a dead end. Such work was in many ways in conformity with my actual age. From the standpoint of the dramatic movement of my life, this may seem to be a retrogression but, in fact, I do not think that it was.

In the life of the individual, neither the standard success story of the slicks nor the Greek tragedy is the normal and the significant outcome. That the individual dies in the end is clear, but what is also clear is that this fact of the physical termination of one's life is not its significant outcome. In the course of the voyage from the nothingness before conception to the nothingness of death lies everything that is really important in life, and this voyage is generally neither a dramatic steering into the maelstrom nor a triumphant ascent from success to success, without some intervening setbacks and some periods of calm cruising.

It may seem a step down to follow years of precocity and early academic degrees by the somewhat routine tasks of a shopworker, a hackwriter, a computer, and a journalist. Yet these were for me the equivalents of experiences in the world about me which many a boy obtains much younger as a prosaic part of his normal development. Just because I did not have these experiences at an earlier period, and just because this contact with the world about him is essentially part of the education of everyone, these everyday experiences had for me a glamour and a novelty which they would not have had for a boy of more usual bringing up. Thus a description of these periods, and of their reaction on me, is as essential to this book as all its other and perhaps more exciting parts.

When I started to look for a job again, I heard of a vacancy in mathematics at the University of Puerto Rico. I sent in my name as a candidate, but I did not get even a nibble at my trial balloon. A few days later, I received an urgent telegram from Professor Oswald Veblen at the new Proving Ground at Aberdeen, Maryland. He asked me to join their ballistic staff as a civilian. This was my chance to do real war work. The demand was immediate, so I saw Mr. Rhines and terminated my connection with the *Encyclopedia*. I took the next train to New York, where I changed for Aberdeen.

Aberdeen, Maryland, was then a minor country town of no particular distinction. A little branch railroad line, operated by the Government, ran from the village to a place on the line as yet without a railroad station, where I got off. I found a collection of temporary wooden huts and quagmire streets, which has since become a very pretty little Government station. In those days a tractor was always kept ready to pull the squelching trucks out of the streets.

The establishment of Aberdeen Proving Ground constitutes a very important epoch, both in the scientific history of the United States and in the private careers of the scientists who were stationed there. Although American scientific work had long been important in astronomy, geology, chemistry, and some other fields, most of our best men had been trained in Europe, or even imported from Europe. Mathematics was well behind these other sciences in its American recognition. As I have said before, Birkhoff was the first really great American mathematician to come to the top without European training. It was now only six years since he had taken his degree in 1912. Thus we American mathematicians were a feeble race, and the country at large regarded us as so many useless fumblers with symbols. It was scarcely possible to believe that we could play any useful part in the national war effort.

The war with Germany involved the design of many new types of artillery and of ammunition. For each type of artillery, and each type of ammunition used in it, it was necessary to construct a complete new range table, and to put it in the hands of the men in the field. These range tables consisted of lists of the ranges to be expected from the gun and the ammunition for each angle of elevation, together with corrections for tilt of the trunnions, unit change in the angle of elevation, for unit excess of powder charge or ammunition weight, for wind, for air pressure aloft, and so on. The tables also contained estimates of the probable errors of all the primary data. The old-fashioned methods of computing range tables had proved both too slow and too inaccurate for modern needs, and had broken down completely in the new and very exacting field of antiaircraft fire. Thus there was urgent need for every available man with mathematical training to operate a computing machine, and civilians like myself were pressed into service, drafted mathematicians were transferred to the Ordnance Corps and to Aberdeen, and even officers were recalled from the front to sit at a desk and to work a slide rule.

Professor Oswald Veblen of Princeton was made a major in the Ordnance Corps and put in charge of this motley group. His right-hand men were Captain F. W. Loomis and First Lieutenant Philip Alger, later Captain, whose father had been the great ballistic expert of the Navy. As for the rest of us, we lived in a queer sort of environment, where office rank, army rank, and academic rank all played a role, and a lieutenant might address a private under him as “Doctor,” or take orders from a sergeant.

One thing is clear, however: we accomplished the task expected of us. It was a period in which all the armies of the world were making the transition between the rough old formal ballistics to the point-by-point solution of differential equations, and we Americans were behind neither our enemies nor our allies. In fact, in the matter of interpolation and the computation of the corrections of the primary ballistics tables, Professor Bliss of Chicago made a brilliant use of the new theory of functionals. Thus the public became aware for the first time that we mathematicians had a function to perform in the world. We still did not count in its eyes as magicians comparable with the chemist and engineer.

We were fortunate in this respect, for while our newly won prestige contributed appreciably to our salaries and our ease of finding employment, the authorities did not yet consider us sufficiently important to be worth the trouble of interference and annexation. Emerson did not tell the whole truth about the fate of the man who devises a better mousetrap. Not only does the public beat a path to his door, but one day there arrives in his desecrated front yard a prosperous representative of Mousetraps, Inc., who buys him out for a sum that enables him to retire from the mousetrap business, and then proceeds to put on the market a standardized mousetrap, perhaps embodying some of the inventor’s improvements, but in the cheapest and most perfunctory form which the public will swallow. Again, the individual and often delicious product of the old small cheese factory is now sold to the great cheese manufacturers, who proceed to grind it up with the products of a hundred other factories into an unpleasant sort of vulcanized protein plastic.

In the Second World War and the days that have followed it, the very success of the American scientist has driven him the way of American cheese. This war saw every chemist, every physicist, every mathematician, dragooned into Government service, where he had to put on the blinders attendant on working on classified material, and to confine his efforts to some minute sector of a problem of whose larger implications he was held deliberately ignorant. Although the excuse given for this procedure was that it kept secrets out of the hands of the enemy, and this was undoubtedly part of its real intention, it was not and is not entirely unconnected with the American lust for standardization, and the distrust of the individual of superior abilities. This, in turn, is related to our love for Government projects or private laboratories with budgets running into the millions of dollars which put a premium on the traditional Edisonian search through all possible materials, at the expense of erratic and unpredictable use of reason and intellect.

However, in the early days of Aberdeen Proving Ground, the King Log of indifference was already dead, and the King Stork of regimentation had not yet ascended to the throne. It was a period of nascent energy in American mathematics. For many years after the First World War, the overwhelming majority of significant American mathematicians was to be found among those who had gone through the discipline of the Proving Ground. I am speaking of such names as Veblen, Bliss, Gronwall, Alexander, Ritt, and Bennet.

It was the young men who particularly interested me. I found Hubert Bray there, whom I had last seen when I was graduated from Tufts College. Bray has been associated with Rice Institute for many years and is now head of the mathematics department there. For a while we lodged together. Later I shared a roughly boarded compartment in the civilian barracks with Phillip Franklin, now my brother-in-law and colleague at the Massachusetts Institute of Technology, and with Gill of the College of the City of New York. I also had as companions for a shorter period Poritzky, who later gave up pure mathematics and academic work for applied mathematics and a job with the General Electric Company, and Widder, now of the Harvard mathematics department.

This list is far from complete. Graustein, who had left Harvard for the Proving Ground and finally left the Proving Ground for an officer's commission, was a leading mathematician at Harvard for many years before his untimely death. I have also left out the names of a large number of astronomers, engineers, and secondary school teachers, with whom I have come rather less in contact in recent years.

Franklin and Gill, nineteen and thus considerably younger than I, were my particular cronies. When we were not working on the noisy hand-computing machines which we knew as “crashers,” we were playing bridge together after hours using the same computing machines to record our scores. Sometimes we played chess or a newly devised three-handed variant of it on a board made of a piece of jump-screen, or risked the dangers of burning smokeless powder or TNT. We went swimming together in the tepid, brackish waters of Chesapeake Bay, or took walks in the woods amid a flora that was too southern to be familiar to us. I remember the pawpaws and their exotic tropical manner of growing their fruits directly from the tree trunk.

Whatever we did, we always talked mathematics. Much of our talk led to no immediate research. I remember some half-baked ideas about the geometry of Pfaffians, in which I had become interested through Gabriel Marcus Green of Harvard. I cannot remember all the other subjects we discussed, but I am sure that this opportunity to live for a protracted period with mathematics and mathematicians greatly contributed to the devotion of all of us to our science. Curiously enough, it furnished a certain equivalent to that cloistered but enthusiastic intellectual life which I had previously experienced at the English Cambridge, but at no American university.

I made several trips home on furlough. I saw a lot of G. M. Green on these trips. He had become very much devoted to my sister Constance, who had become a budding mathematician herself. On one of these trips I talked over with my parents a plan which had long been in mind, which consisted of using my contact with the Proving Ground as a means for enlisting or being drafted into the Army for limited service. Finally, in October of 1918, the opportunity came, and with Major Veblen’s co-operation I went to a neighboring market town and county seat to receive my enlistment papers.

I was sent to the recruit depot at Fort Slocum on an island off the coast of Westchester County, New York. It had by now become clear that the war was drawing to an end. I was appalled by the irretrievability of the step I had taken. I felt as if I had been sentenced to the penitentiary. The crowding of the recruits and their incompatible simultaneous attitudes of scared boys and of tough swaggering young soldiers were far from congenial to me. The one alleviation of my life on the island was the presence of another equally unmilitary recruit, Dr. Harry Wolfson of the Harvard department of Semitic languages. My uniform was strained by my corpulence, but

Wolfson's reached nearly twice around him. Not even these uniforms could conceal the college professor in us as we walked around the seawall, discussing Aristotle and medieval Jewish and Arabic philosophy.

At length a group of us were shipped back to Aberdeen Proving Ground. We rode around Manhattan in a tugboat and entrained for Philadelphia from one of the stations on the Jersey shore. In Philadelphia we heard the blowing of steam whistles to celebrate the first false reports of the armistice with Germany and saw the showers of paper floating down from office windows. About two days later, when we were already assigned to our companies at the Proving Ground, we were assembled early in the morning and told that the true armistice had been signed.

The military setup at the Proving Ground was most peculiar. Besides the administrative groups, the ballistic group, and a few other technical groups of the same sort, there was the powder-bag-sewing group and a large group of laborers to do the necessary digging and construction work. The latter were largely men who were not sent to the front because of venereal disease. All the different groups were mixed together indifferently in the companies and in the barracks. I do not need to expatiate on the continual shock of the impact of this foul-mouthed crew on a man who was not yet fully adjusted to the brutal frankness of army life.

I did two turns of guard duty. I got off easily on one of them, carrying a watchman's clock throughout the night in the building which housed the chronoscopes and the scientific library. Between my rounds I found plenty of interesting material to read. The other time I was an ordinary sentinel out of doors, carrying a rifle with a fixed bayonet. I found it hard not to drowse off and to keep sufficiently alert to challenge the Officer of the Day. In the small hours of the morning I had a little rest on the bare bedsprings of a cot in the guard room; and although I felt like a waffle when I woke up, the relief of the big cup of steaming coffee which was passed around with some cheese sandwiches more than made up for it.

Besides such military duties and my work in the office, I did a certain amount of work at the firing "front," collecting range-data for antiaircraft fire. We had a special telephone line connecting the gun station and two or three observing stations, where observers looked through peep sights at the reflections of the shell bursts in flat horizontal mirrors covered by coordinate gratings. In view of my inferior eyesight, I was the gun telephone operator; and I lay upon an earth bank, unpleasantly near to the noise and the blast of the gun-muzzle, and gave the observers the time of the firing of the gun, the burst of the shell, and five-second intervals thereafter. These later intervals enabled the men at the mirrors to synchronize their observations of the drift of the smoke downwind, and thus to compute the wind aloft. I also notified the gun crew when the observers were ready.

The observers rode out to their stations in an old Ford beach wagon, and sometimes they had to cross a range where firing was going on. Theoretically, the safety officer should have interrupted the firing to let them go by; but even safety officers grow careless with time, and this precaution was not always observed. I remember one time when the observers at a tower down the range complained that shrapnel was coming through the roof. "All right," said the safety officer, "we'll just fire another couple of rounds and call it quits."

As long as we had felt that we were doing work necessary to win the war, our morale had remained high. After the armistice, we all felt that we were marking time; and in particular those of us who had enlisted at the last moment considered ourselves a pack of fools. The civilians began to leave at the first opportunity, while the rest of us were going through the motions of military service until we could be sent to a camp where we should be discharged from the Army.

Even with a temperament not suited for a regimented life and a more than average desire to see what I was doing and to know what it meant, I had found a few months of army life a haven at a time when I had been very tired for years from making my own decisions. It has been said many times that the motives of the soldier and the monk are curiously similar. The love of a regimented life and the fear of personal choice and responsibility make some men feel secure in the uniform as well as in the monk's hood.

I was very deeply curious as to how the war was to end and what the new postwar life was to be. Meanwhile, I marked time. This emotional stagnation affected me both before my induction into the service when I was already leading much of the life of an army camp, and afterward; but its main impact began to fade out with the temporary letdown of the armistice and the hope that things could get back into their civilian channels.

While waiting for my orders to Camp Devens at Ayer, Massachusetts, for my final discharge, the influenza epidemic hit us. At the beginning we did not take it seriously, but soon it became a commonplace to ask about a soldier and to find that he had died the day before. We all wore flu masks, and the elephantine Professor Haskins of Dartmouth smoked his pipe through his. One very honest and conscientious soldier, an M.I.T. graduate who had been put to work unloading a freight car, complained that he was unwell. The doctor sent him back to his work, and he died of pneumonia the next day.

It was a gloomy experience to see the rough pine coffins piled high on the station platform, and to wonder where the next blow would fall. I received a telegram from my father, telling me that my friend Dr. G. M. Green of the Harvard mathematics department, and betrothed to my sister Constance, had just died as the result of the epidemic. This news hit me hard. It came just before I left for Camp Devens.

I found Ayer superficially the town of my youth, but much changed in every important way. With the tendency of the railroad to lengthen the locomotive runs and the blight that had already begun to fall on the branch lines, Ayer was no longer the important railroad center it had once been. On the other hand, Camp Devens, which was a newcomer since the beginning of the war, was considerably larger than the town itself, and the merchants fattened on the soldier trade.

There was not much to do while waiting for one's discharge.

There were medical examinations to take and papers to sign. I put in one day working on the coal pile of the power station. I spent much of my time at the various post libraries, reading the writings of G. K. Chesterton. At length the day of my release came, and after a brief visit to my friends at Brown's drugstore, I took the train home.

18 The Return to Mathematics

The end of the war brought a keen sense of loss to all my family in G. M. Green's death. Green had been a charming and unassuming young man, deeply attached to my sister Constance, and of great sincerity and tenderness of disposition. He was a very severe loss to modern science, too, as he had developed a peculiarly individual vein of work in geometry, and it seemed that he was likely to furnish an element that was otherwise missing in the Harvard mathematical scheme of affairs. The death of a young man at the peak of his career is perhaps the greatest tragedy there can be, and it was hard for my sister, my parents, and me to realize that our good friend was gone.

Green's parents gave his mathematical books to Constance as the one who had shared most in that phase of his life and the one to whom they could do the most good. Constance went to Chicago in the hope of forgetting her bereavement in new work, as far as that might be possible. Hence I had the chance to look at these books and to read them over. They came to me at just the right time in my career.

For the first time, I began to have a really good understanding of modern mathematics. The books included Volterra's *Théorie des equations integrales,* a similarly entitled book by Fréchet, another book by Fréchet on the theory of functionals, Osgood's *Funktionentheorie,* Lebesgue's book on the theory of integration (to which I devoted particular attention), and I believe a German book on the theory of integral equations as well.

However, reading mathematical books would not pay my parents for my room and board, nor would it of itself advance me in my career. Again I began to look for a job. I sent my name to various teachers' agencies, but it was only February and no academic job was likely to start until the next September. At Silver Lake, Mr. O'Brien of the Boston *Herald* had been a summer neighbor of my parents, and they sent me to him to see if he could find a place for me on his paper. My encyclopedia experience spoke in my behalf, though I could not see eye-to-eye with Mr. O'Brien's intention to make use of my mathematical ability by training me for a job as financial editor. For this, of course, I had neither the gift nor the inclination.

The confusion between the craft of the accountant and that of the mathematician is very common. The difference between imaginative mathematics and accounting is obvious, but it is equally real between accounting and computing. The accountant works to the last cent. His duty is to eliminate discrepancies which might offer someone a chance to embezzle undetected. The mathematician works to a certain number of decimal places of accuracy. His maximum permissible error is not an absolute quantity, such as a cent, but a stated fraction of the smallest quantity with which he deals. The computer who turns accountant is likely to leave appreciable sums of money unaccounted for, whereas the accountant turned computer often works to just two places beyond the decimal point, where the logic of the problem may call for four or five, or in another case, for none. Unless a man is reasonably young and flexible it is disastrous to shift from one of these seemingly so similar tasks to the other.

Luckily, I escaped from this assignment and was put instead to straight journalistic tasks. In the Boston *Herald,* I was a feature writer, and began to make myself acquainted with the paper litter and the printer's ink, the noise of typewriters and linotypes, and the general sense of hurry and bustle which constitutes the normal background of the city newspaper. I tried writing a few editorials. I learned very soon the extreme care which the editorial writer must take to verify his facts, and to avoid unnecessary treading on toes. Then I was put on Sunday feature writing.

The textile industry of Lawrence was passing through one of its periodic strikes, and I was sent down with carte blanche to cover the larger features of the situation. I was less liberal then than I have become since. Happening to meet one of the senior Lawrence labor union leaders on the train, I was almost prepared to find that he had horns and hooves. On the contrary, he was a fine, sympathetic old Lancashire man, who had left England when the shadow of the industrial revolution was still at its darkest. He had seen the philanthropy of the early New England manufacturers give way to absentee ownership and the English weavers of the early days supplanted by French Canadians, Belgians, Italians, and Greeks. He had continued to exert his authority over the younger generation, although he had found that they needed rather different handling than their predecessors; and they had begun to develop their own labor leaders under his tutelage.

He told me to keep an eye on the living conditions in Lawrence, and on the way in which the Americanization work was handled. He also gave me a list of priests and labor union leaders in order to help me feel the pulse of the various foreign elements in Lawrence. I appreciated the sturdy honesty of the man and found his advice sound and helpful.

Lawrence was a sick town. The mills were already suffering from an overcapitalization in obsolete equipment and from the competition of the South, as yet uninhibited by any standards of employment, and favored by the lower wages made possible by a less severe climate. The owners of the Lawrence mills often had never set foot in Lawrence, and had left the problems of management and employment to their badgered agents, ground between the employers' demands for profits and the workers' demands for higher wages and better working conditions. The housing conditions were atrocious, and although the employers defended them by the hoary argument that the class of labor they employed would rapidly ruin better quarters, one might as readily reply that the foul quarters made it impossible to employ a better class of labor. I took part in one of the Americanization classes at the Y.M.C.A. and was appalled by what I saw. Not only were the teachers utterly ignorant of the language of the men they taught, which happened to be Italian in the class which I attended, but they were completely out of touch with the educated elements in the immigrant communities. The contents of the textbook in use exhorted the workers to love and to honor the boss, and to obey the foreman as if he were Jehovah himself. It was the sort of humiliating tripe which was bound to alienate any workman with a trace of character and independence.

I published my reports of things as I saw them, and they excited some opposition but, on the whole, less than I had expected. I should like to think that my articles had some influence on public opinion, making it more conscious of the importance of the housing question, and that they may thus have had some small effect on the later establishment in the Lawrence region of garden cities, such as Shawsheen Village.

After this job, O'Brien put me on a political undertaking which was very much nearer his heart. This was to build up General Edwards, former commander of the Yankee Division, as a possible candidate for the Presidency of the United States. As far as I could see after meeting the man, Edwards was an amiable enough old gentleman of no particular distinction. I found that I did not relish my assignment at all. I visited in due course his friends and relatives in Cleveland, Ohio, and at Niagara Falls, and I looked up ex-President Taft and other notables in Washington who had known him in the Philippines.

With all my experience of hack writing on the encyclopedia, I had not learned to write with enthusiasm of a cause in which I did not believe. I was fired from the *Herald,* and the Edwards series was put in the hands of a more responsible wheel horse. I was not unready to leave the paper, but I am thankful for the experience in writing which it gave me as well as the knowledge of the American scene.

I came away from my second experience of writing for my living with a new sense of the dignity of hack writing. In general, our courses in English at our colleges are as far from teaching us to write English as our courses in foreign languages are from giving us a real mastery of any foreign language. This applies primarily to the introductory courses. They do not in general make sufficient demands on the student to bring him to the stage at which he has to write a critically acceptable thousand words every day or be punished for it severely by going without eating. They introduce him to the English language as one might be introduced to a delightful young lady at a cocktail party: the student has not quite caught her name and would not know her again. After I left the *Encyclopedia,* and especially after I left the *Herald,* I was reasonably confident that if it should ever be my duty to say anything in print, I could say it fairly correctly and forcefully the first time I should write it down.

Thus, I am glad that I had an easy test of Grub Street between my years of wandering and my years as master in my own shop. In addition to this benefit, my experience as a journeyman gave me the satisfaction of independence which I could achieve in no other way. Not only was I earning my own living, but I was doing it in a fashion that made no demands whatever on my father; and to a large extent, I was doing it away from home and parental tutelage. In short, I was growing up.

I had two mathematical papers under way to which I devoted my temporary unemployment. They both concerned the extension to ordinary algebra of Sheffer's idea of a set of postulates with a single fundamental operation. I wrote them in the stacks of the Harvard Library, near my father's office. They were published in the following year. Although they represent directions of work which as far as I know have not been followed up, they were by far the best pieces of mathematical work that I had yet written. However, I soon afterward gave up algebra and postulate-theory for analysis, which seemed to me to have a richer and firmer intellectual body. Thus it is hard for me to evaluate these papers at the present time, or even to remember exactly what they contained.

I had been trying for some years to find a publisher for my Harvard Docent Lectures. While they were in no sense a finished piece of work, I do not think that I am completely unfair in seeing in the idea of constructive logic which I there developed a certain approach to the ideas by which Gödel was able to demonstrate that in every system of logical postulates, there are theorems whose truth or falsity cannot be decided on the basis of those postulates. I finally sent the manuscript to P. E. B. Jourdain, a remarkable English logician who lived near Cambridge, and with whom I had already been in correspondence. While I was at Cambridge I had asked if I could meet him in his house, but we had not been able to make arrangements. I had not realized at the time, nor indeed until well after I had sent him my manuscript, that he was a hopeless cripple, scarcely able to move a finger. He had known very well that he had suffered from Friedrich's ataxia, a congenital nervous disorder which always ends in paralysis and early death. Notwithstanding this fatal ailment, he had married and had become editor of that important philosophical magazine, *The Monist.* He had written a very humorous and critical book concerning Bertrand Russell's philosophy, in which each chapter was introduced by an appropriate text from Lewis Carroll.

My manuscript reached Jourdain only a few months before his death. If I had known how ill he was, I would of course not have embarrassed Jourdain by sending him my manuscript. However, it was published in the periodical, and I had *The Monist* bind together in book form the reprints of my article, which had run in three numbers. It pleased me then to imagine to myself that this constituted the publication of a book.

The articles found a limited echo. Professor Broad of Cambridge referred to them; but in those days, work in mathematical logic was equally unhelpful in getting jobs in either mathematics or philosophy. Today mathematical logic constitutes a recognized career. As in some other fields, it is a career for *epigonoi,* not for pioneers. There are tables where guests are served only after the silver and the china have been set. The best careers are reserved for the students who do exactly what was timely in the youth of their professors, and the tin gods tolerate no *hybris.*

In the spring of 1919, I heard of two teaching openings which seemed to me almost equally attractive. I heard of one through a teachers' agency at the Case School of Science in Cleveland; the other, which was called to my attention by Professor Osgood of Harvard, was at the Massachusetts Institute of Technology. I do not think that Professor Osgood had a particularly high opinion either of me or of the job, for the previous contributions of the Massachusetts Institute of Technology to mathematical research had been scant, and the department was then almost exclusively a service department to prepare the students for their later mathematical requirements in engineering. However, the postwar rush was absorbing every man who could possibly be considered as a mathematician. Indeed, I had had some hopes that Professor Veblen would take me, as he took Franklin and many others, as a member of the Proving Ground group out of which he was to build the justly famous Princeton department of mathematics; but there were many good candidates, and I was not among his selections.

I called on Professor Tyler, the head of the department of mathematics at the Massachusetts Institute of Technology. He was a small, bearded, bright-eyed man, not a research mathematician, but sagacious and eager for the welfare and the reputation of his department. He appointed me as one of the instructors to handle the new overload, with a possibility of working into a more permanent position if I should make good, but with no promises. He suggested that I should devote myself to applied mathematics. My immediate research in the department happened to be in pure mathematics, but my present happy thirty-three-year association with the Massachusetts Institute of Technology Department of Mathematics and my contact with engineers and engineering problems have given my pure mathematical researches an applied coloring, so that I may be said in some measure to have obeyed the injunction that Professor Tyler laid on me.

At this time, Harvard was involved in a great dispute concerning a Jewish *numerus clausus*. In order to conserve President Lowell's idea of Harvard as a non-sectional institution and the cradle of the ruling class, he proposed to set a certain percentage restriction on the number of Jews admitted. It was understood that it was an administration measure, so that anyone who opposed it risked burning his fingers. My father took an uncompromising attitude against the numerical restriction of the Jewish students, and I am proud to say that when this specific piece of injustice and depreciation came into question, my mother's opinion supported that of my father without any hesitation. All this was happening during the period in which I myself had to look for the firm foundation of my academic career. My sense of belonging to a group which was treated unjustly killed the last bonds of my friendship and affection for Harvard.

I had not been aware at all of anti-Semitic prejudice in my childhood. My parents had many friends who liked and admired them at a certain distance, but they had very few people with whom they felt at liberty to pay unannounced visits, or whom they would have expected to pay unannounced visits to them. I do not believe that this was due to any genuine rejection of my family by the greater part of their Harvard colleagues but much more to a timidity which kept my parents from running the chances of such a rejection.

This was extended to us children. There were very few of the other Harvard children whom I would have been allowed to visit unless such a visit had been organized in advance with the proper amount of interfamily protocol. Thus I was thrown very largely on nonuniversity families for my companionship, and in the end I think this was good.

As to the origin of the family timidity, I think it must have been very mixed. Probably the element contributed by our specifically Jewish race was slight compared with our being new Americans among old Americans, and Westerners among New Englanders. At any rate, it represented a certain degree of aggravation of the relatively isolated position in which we children found ourselves. But all of this is an unimportant element compared with the other features of my situation as a child.

However, by the end of the First World War, I was fully aware of the existence of anti-Semitic prejudice of a most vicious sort. This was a period when it became customary for the friends and faculty advisors of Jewish boys to warn them that their chances of establishing themselves in an academic career were pretty slim. It represents a point of view which lasted for a considerable period but which seems to have gone under in the reassessment of racial attitudes that took place during and after the Second World War.

I have been gratified to see a considerable change of attitude not only toward Jewish scholars in the universities but by Jewish scholars in the universities. With the decline of anti-Semitism, there has been a decline of resentment and fear on the part of the Jewish scholars themselves and a greatly increased possibility for them to participate actively in the problems of the community at large. That this change and maturing has taken place within the circles which I see about me day by day is a matter which is clear to me by the use of my own eyes; and I hope and believe that it is but the counterpart of a phenomenon taking place on a much larger scale.

On the whole, Lowell won the bout in favor of the *numerus clausus,* at least for the period of his own rule. He was defeated, but set up an administrative scheme that enabled him to keep a pretty close watch on the numbers of all Jews not of outstanding talent. I believe that the issue is pretty much dead now after the horrifying example of Nazism and after our more enlightened attitude concerning the right of each individual to employment and to the best education he can manage to get. During the Lowell period, however, those who opposed the president in a matter so close to his heart ran the risk of permanently alienating his good will. After the faculty meetings on the *numerus clausus,* my father was no more able to feel himself in President Lowell's good graces. He felt this keenly sometime later when he hoped for a period of employment by Harvard beyond the statutory age of retirement. He was ultimately denied the privilege and was refused in language that took no cognizance of his long and faithful service to Harvard.

After another New Hampshire summer, but before term began, I found two matters of importance to occupy my attention. I received a call from Barnett, a young mathematician from the University of Cincinnati. As Barnett was working in functional analysis, which was a field in which I aspired to work, I asked him if he could suggest to me a good problem of research. His reply has had a considerable influence on my later scientific career. He suggested the problem of integration in function space. During my first year at the Massachusetts Institute of Technology, I found a formal solution of the problem, which employs some ideas already worked out by the English mathematician, P. J. Daniell, then teaching at the Rice Institute in Texas. However, my first adaptation of the Daniell ideas seemed to me rather lacking in content; so I set out to look for some physical theory that would embody a similar logical structure. I found this in the theory of the Brownian motion. A somewhat similar theory of integration had been discussed by Gâteaux, a young French mathematician who died in the First World War; but his work could not be directly subsumed under that of Daniell and Lebesgue.

Most of my later work in mathematics goes back in one way or another to my study of the Brownian motion. In the first place, this study introduced me to the theory of probability. Moreover, it led me very directly to the periodogram, and to the study of forms of harmonic analysis more general than the classical Fourier series and Fourier integral. All these concepts have combined with the engineering preoccupations of a professor of the Massachusetts Institute of Technology to lead me to make both theoretical and practical advances in the theory of communication, and ultimately to found the discipline of Cybernetics, which is in essence a statistical approach to the theory of communication. Thus, varied as my scientific interests seem to be, there has been a single thread connecting them all from my first mature work to the present.

The other piece of business which I found awaiting me on my arrival in Boston was of a much more worldly nature. The housing and pay of the Boston police force had long been notoriously inadequate, and certain members of the force had stuck their necks out in an effort to have these conditions improved. This led to a threatened police strike. Now there had been a number of such abortive strikes elsewhere at about the same time, and conservative public opinion had begun to be terrified of the possibilities, and to consolidate itself in opposition to the asserted right to strike on the part of those performing vital public functions. Thus there was no difficulty in recruiting a volunteer police force to serve in case the regular police should make their threat good. A friend from the Harvard mathematics department had sent his name in as a candidate for this volunteer police force, and in a moment of misguided patriotism, I followed.

What happened is history, and it made a spurious reputation for Calvin Coolidge, who was Governor of Massachusetts at the time. The regular police struck. Instead of calling out the volunteers to take over the police stations as the regulars left, Coolidge let the city taste twenty-four hours of anarchy and looting before he did anything. This may have been mere indecision or it may have been political sagacity; but whatever it was, it was hard on the shopkeepers whose windows were smashed, and on the nerves and pocketbooks of the public at large. We volunteers received badges and revolvers, and were sent out in pairs to patrol our beats. I was attached to the Joy Street station. The first night I was on duty, there were crowds milling up and down Cambridge Street and Scollay Square and Hanover Street, but we experienced no violence on my beat although a man was killed on a neighboring beat. Later on, I was sent to patrol various streets of the West End. Nothing very exciting happened to me, although I was sent with another recruit to help arrest a wifebeater in a slum near the North Station. I drew my revolver, but it was trembling like the tail of a friendly dog, and I must bless my guardian angel that it did not go off. On another occasion, as I was walking up and down a quiet Jewish slum street, I saw a boy discussing with his comrades some difficulties in an algebra lesson. I interrupted him and put him right, and continued to walk my beat. Sometime afterward, this boy attended the Massachusetts Institute of Technology and became one of my first advanced students in mathematics. I saw him last a couple of years ago at the Carnegie Institute of Technology in Pittsburgh, where he is now a professor.

The net result of the police strike was to make Calvin Coolidge President, to secure the firing of the striking policemen, and to bring in a new police force which was granted most of the demands for which the men of the old police force had sacrificed their careers. As for myself, I was left with nothing but the shame of having acted as the Governor's dupe and strikebreaker.

My arrival at the Massachusetts Institute of Technology meant that I had come safely into port in the sense that I was no longer to be rushed by the problems of finding a job and knowing what to do with myself. When I arrived there, I was one of the large crop of new instructors needed to handle the increased teaching loads which followed in the wake of the First World War. There was no understanding of permanence in my appointment, although I had as good a chance as anyone else to make it permanent if I should prove to be intellectually and emotionally able to make good as a teacher.

The M.I.T. mathematics department itself was then going through a period of transition. Although it was primarily viewed as a service department, there was nonetheless a small nucleus of mathematicians with great scientific enthusiasm who had more or less recently come into the department, and who looked forward to the day when our group might be known as much for its original research and for its training of men capable of carrying out original research as for running interference for the engineering backs.

Among the older men of the department, F. H. Woods had already shown an interest in pure analysis; and E. B. Wilson, who had recently left the department for physics and was destined to leave physics for biostatistical work in the Harvard School of Public Health, represented the great Yale intellectual tradition of Willard Gibbs. Lipka and Hitchcock had been producing for years a certain amount of highly individual mathematical research; this, however, had been off the beaten track, and had a somewhat slender relationship to the type of research favored at other American mathematical schools. The two staunchest supporters of a new policy of research, and the two men who really envisaged what the school might become, were C. L. E. Moore and H. B. Phillips.

Moore was a tall, powerfully built man whose eyesight had only just emerged from a half-blindness because of a displacement of his lenses, and who was to succumb after a few years to a half-blindness resulting from glaucoma. He was kind, intensely loyal to scientific research, and free from any taint of sham. He had studied in Italy before the First World War and had met there an atmosphere of kindness and sincerity which reinforced his own. Italy was then the great home of geometry, and he was accordingly a geometer. Though his field of work was different from mine, he encouraged me with a fatherly interest in my possibilities, which was just what a diffident and awkward young man needed to bring him out. He backed me in the founding of a local M.I.T. mathematical journal, which made it somewhat easier for me to publish my early unorthodox mathematical work.

Professor Phillips, who has officially retired but has not fully withdrawn from teaching, has always seemed to me an utterly timeless figure in the mathematical background of M.I.T. He did not look particularly young when he was young, and he scarcely looks older at the age of seventy. He is a long, loose-jointed Southerner, born in a South where the memory of the Civil War and reconstruction had overshadowed everything else, and he had thus become a skeptic and a bit of a pessimist in a most optimistic and forward-looking way. He was intensely an individual and as fundamentally kind as Moore.

What Moore and Phillips did for me was to discuss their own work with me and to let me discuss mine with them. It must have been very boring for them to receive my ideas in a half-baked form and to suffer under my immature presentation of personal and scientific difficulties. But the great thing was that they listened to me, and that for the first time I had my hopes of becoming a real mathematician reinforced by the confidence of others. Between us we discussed long plans for the future of our department and indeed for the growth of mathematics in the United States. It made me feel more like a man to receive the confidences and the hopes of these men whom I respected, and I became more of a man. Even Professor Moore, who died in 1932, lived to see our department far more than a service department, and indeed one of the constructive research departments of the Institute. Professor Phillips was head of the department for many years after it had assumed its modern functions. What both of these men saw went beyond the wildest dreams they had formed by the end of the First World War.

Within three or four years of my appointment to M.I.T., I began to have accumulated a very considerable body of recognized work. I came to be interested in potential theory, in which I received many suggestions from Professor Kellogg, who was at Harvard at that time. Gradually it began to be clear to me that in the unsolvable cases of the problem of fitting a potential to certain boundary values, there was still a unique potential function fitting these boundary values in a looser sense than that demanded in the literature. Then the question arose, How can one be sure in a particular case that the solution of the generalized Dirichlet problem, as the problem of potential fitting is called, will satisfy the conditions of continuity demanded in classical potential theory?

About that time, a series of papers had appeared by the great mathematician Borel on a different but remotely related subject called quasi-analytic functions. The innovation of Borel's work at that time was that he brought the problem to depend, not on the size of a number, but on the convergence or divergence of a series. It struck me that my problem of singular points on the boundary of a potential function might well receive its answer in that form instead of in the form of the determination of some particular number, as had been suggested in most of the earlier attempts to solve the problem. At any rate, I sweated the answer through, and my conjecture was correct. With the aid of my Mexican student, Manuel Sandoval Vallarta, who later became a professor at M.I.T. and has since become one of the brightest stars in the firmament of Mexican science, I translated my article into French and sent it to Professor Henri Lebesgue for publication in the *Comptes Rendus* of the French Academy of Science. I followed this course because I had recently seen a series of articles by Lebesgue and a young mathematician named Bouligand, which were getting uncomfortably near to the complete solution of the problem in which I was interested, and which would eliminate the problem from the literature.

It turned out that after I had mailed my article, but before it had been received, Bouligand had submitted a sealed envelope containing a very similar result, to be held by Lebesgue to preserve Bouligand's claim to priority in the field. It was a dead heat between Bouligand and me, and when my article came in, Lebesgue advised Bouligand to let his envelope be opened. The two papers appeared side by side in the *Comptes Rendus*. The results were substantially the same, although I like to think my formulation of the problem was logically a little more complete.

This incident was the start of a friendship between Bouligand and myself which has lasted to the present day; and when, some time afterward, I went to visit him at Poitiers, he made himself known to me on the station platform by exhibiting a copy of my reprint on the subject.

There was a mathematical congress at Strasbourg in the summer of 1920. Although this congress was in some ways unfortunately limited in that Germans were not admitted, I attended it. This was my first opportunity to participate in international mathematics. I worked with Fréchet, who was then professor at Strasbourg, and spent part of my summer vacation at a hotel in the Vosges near where he was staying.

As the result of my work, I participated in two papers of research which were destined to have a certain effect later on. I converted my rather awkward and formal work on integration and function space into a study of the Brownian motion, thus uniting it with the ideas of Einstein and Smoluchowski. This work was an intrinsic stage in developing my later techniques which I have applied in communication theory and in Cybernetics.

The other idea which I developed in my discussions with Fréchet was that of a certain generalized vector space, for which I gave a set of postulates. I soon found out that I had missed the boat by the very narrow margin of a few months, as the theory of the same space had been developed and studied by Banach in Poland. Although we had run almost an even race, I gave up the field later, and left it completely for Banach to open up, as its degree of abstractness struck me as rendering it rather remote from that tighter texture of mathematics which I had found to give me the highest esthetic satisfaction. I do not regret having followed my own judgment in the matter, as there is only a certain amount of work that a mathematician can do in a given time, however he distributes his efforts. It is best for him to do this work in a field that will give him the greatest inner satisfaction.

When I returned to M.I.T. I found that the electrical engineers were beginning to count on me for resolving the very serious logical doubts which were attached to the new and powerful communication techniques of Oliver Heaviside. I was able to make a good deal of progress in this direction, and in the course of this work, I found it necessary to expand the theory of Fourier series and integrals into a more general trigonometric theory covering both. Thus when Harald Bohr of Copenhagen developed his theory of almost periodic functions, I found it to be a field in which I had already developed adequate techniques, and I developed two or three significant alternative approaches to this new subject. The relations between Bohr and myself were and always remained friendly until his death a year and a half ago.

From the beginning of my relationship with M.I.T., I have received loyal backing from it and an understanding of my needs, limitations, and possibilities. I had the opportunity very early to do graduate teaching, and from those early days on, I have collaborated with my younger colleagues and have tried to bring out their intellectual powers. I did not find myself particularly adapted to the niche of the undergraduate teacher. However, the important thing is that I did find that there was a niche in university teaching in which I could function effectively, and this lent me the self-esteem necessary to a successful career.

My undergraduate teaching experience differed so greatly from that at the University of Maine that it was a relief. Perhaps the Maine boys had wanted to play; the Tech boys certainly wanted to work. While there were occasional classroom pranks, they were rare; and the spirit underlying the relations of the professor and the student was one of mutual respect. There were now and then sporadic problems of discipline, but these were so few that they did not form an important part of my relations with my students. Moreover, I could be confident that I would have the backing of the authorities of the Institute in any matter involving my reasonable authority.

At the same time, I learned many lessons. I learned to curb my naturally rapid pace of teaching and to adjust it to students not too much above an average level of performance. I learned that in classroom discipline, the sharp tongue is such an advantage to the professor but such a weapon that it is the part of magnanimity and good sense to use it very sparingly. I learned the trick of handling myself before a student audience, and I sloughed for good all stage fright before a class or, for that matter, before any other audience with serious intellectual purposes.

The year I started teaching at M.I.T. at the age of twenty-five, one of the young ladies who came to our family teas particularly attracted my attention. She was of French background and specialized in French at Radcliffe. She had been brought up before and during the World War in Paris, and was beautiful in a pre-Raphaelite way, with that static beauty which dominates over the beauty of motion in the paintings of Rossetti.

I was greatly attracted to her and spent a great deal of time visiting her and taking her out. She did not like the eternal presence of my much younger brother, and as a result my parents and my sisters took a great dislike to her. I was pursued by them with ridicule, and family ridicule was a weapon against which I had no defenses. I do not know how our interest in one another would have proceeded if it had been left alone. But regardless of that, in the second year of my acquaintanceship with her, she told me she was engaged to another man. I did not take this with good grace, but it was not a graceful situation.

After this, I looked more and more to the Appalachian Club walks for outdoor exercise and social amusements It had been nearly eight years since I had gone on these walks, and now I found myself more suited in age and in social maturity to those about me. I met several young people and had the chance to discuss many things of interest to us so that I made a definite step ahead in my social development. Nevertheless, I still needed more social contacts and I found these in my parents' teas as before.

About the time that I had met the young woman of whom I have just spoken, I met another young woman who attracted me very much, and if I had not at the time been in the middle of a courtship, I would have had no hesitation in paying court to her. After the breakdown of the other affair and the period that was necessary to rebuild my self-esteem, I began to see her and finally to hope that she might become my wife.

Her name was Margaret Engemann, and she has been my wife now for more than a quarter of a century. My attention was called to her by seeing the same family name in the list of my own students, who were also invited to these teas, and in the list of my father's students in Russian literature. We learned that Margaret and my student, Herbert Engemann, were sister and brother, and that they had been born in Silesia in Germany but had lived in several parts of our Far West. One line of their ancestors went back to Bavaria, and although they looked much alike, Herbert's hair was fair and Margaret's so dark that it was nearly black. They had come to Cambridge from Utah, where they had been students at Utah State College. They were both serious, vigorous young people, to whom I was greatly attracted, and when I later came to know their mother (their father had died many years earlier in Germany), I recognized her as an active and interesting woman of the pioneer type. Margaret shared her mother's definite character, though with a more feminine touch.

On one occasion during the winter of 1921, my family went to their new country place, a farm in Groton, for a little skiing. My parents invited the Engemanns to come along with us. I had taken Margaret out once or twice before this and had enjoyed our companionship very much. My parents considered her an excellent match for me, and were not silent in their approval of my interest in her. However, I felt greatly embarrassed by their obvious reaction in her favor, and my response was to keep away from Margaret for the time being. A courtship that might end in marriage could be only my own and could not represent a decision imposed on me by parental authority. Thus it was not easy for me to show my attentions to Margaret. She has since told me that her reactions to my parents' obvious hints were exactly the same as mine.

On our return from Groton, I began to feel very ill, and it was not long before I came down with an attack of bronchial pneumonia. I was delirious for some days, and during my delirium and convalescence, I expressed the desire to see Margaret again and to talk over our future together. I now felt that she was the wife for me. Yet our courtship and the steps leading up to our marriage did not proceed quickly. I was still confused by my parents' overactive participation in my own affairs. Furthermore, Margaret was soon to leave Boston to take a position as a teacher of French and German at Juniata College in Pennsylvania. Her four years of contact with Juniata have made her a permanent and beloved tradition there.

Margaret shares with me deep emotional roots both in Europe and in America. She was born in Silesia, where she had her early school training; but she came to America with her mother and brothers at the age of fourteen, to share with them memories of that vital part of America, the frontier. Thus she has always combined deep understanding of her mother country and her foster country, and a sincere loyalty to the true interests of both.

From the beginning when Margaret and I have discussed our problems together, she has insisted firmly that I should recognize with honesty what I am, and that I accept my Jewishness with neither boastfulness nor apology. I believe that when I was contemplating marriage, my family had supposed that Margaret would fit rather easily into the somewhat patriarchal pattern of the Wieners, and would serve as a ready instrument for holding me in line. While my parents seemed to have hoped this, I was delighted to find that in fact this was never a possibility. Yet until we were both clear on the matter, we had to wait.

I think that the possibility of marriage had long been in the back of Margaret's mind as well as of mine. We met once at the house of one of Margaret's friends, about halfway between her college and Boston, but on that occasion we were both too involved in our immediate futures to come to an agreement. Yet as time went on it became more and more obvious that we were strongly attracted to one another. I gradually came to see what was never subject to any real doubt: that my parents had taken a great deal too much for granted if they supposed that my marriage with Margaret would mean an indefinite prolongation of my family captivity.

During the years that followed my trip to the Strasbourg mathematical congress in 1920, I visited Europe several times with my sisters and alone, doing a bit of desultory mountain climbing together with some American mathematical friends, and visiting the University of Göttingen in Germany. In 1925, Professor Max Born of Göttingen came to M.I.T. to lecture on physics. It began to seem as if there might be enough interest in my work for me to receive an invitation to lecture at Göttingen. The money was furnished by the newly formed Guggenheim Foundation, which has done so much to help American scholars and artists in all fields. I decided to go to Göttingen in the spring.

With this ahead of me, I felt that I was for the first time in a position to marry at once. Margaret and I had met again at my parents' Cambridge house at Christmas and had decided to marry. There was, however, the difficulty that Margaret's teaching obligations did not end until June, by which time I should be on the other side of the ocean. We tried to see whether there was any possibility of our marrying in Germany at the United States Embassy, but we finally came to the conclusion that this would cause us more effort than it was worth. Finally, we decided to be married in Philadelphia a few days before my departure for Europe, and then to go about our respective jobs until Margaret could come to Germany at the end of term. We spent a few pleasant days of our interrupted honeymoon at Atlantic City, and then parted in New York in a gloom caused to some extent by the fact that we had taken a room in that ancient mausoleum, the old Murray Hill Hotel, and in part by the fact that the play we had chosen to see together was one of Ibsen's gloomiest.

However, the period of our separation came to an end, although it seemed it never would, and we met again for the beginning of our European honeymoon at Cherbourg. This was twenty-six years ago, and we were thirty-one. I cannot express how my life these twenty-six years has been strengthened and stabilized by the love and understanding of my partner.

19 Epilogue

This, then, concludes the account of my life from my birth in 1894 until 1926, when I was married at the age of thirty-one. I had joined the staff of the Massachusetts Institute of Technology, and there I have remained ever since.

The present book, apart from its interest to those who have had some continued contact with me and my work, will be read primarily by those who are interested in what is unusual in my career, and the fact that I have been what is known as a child prodigy. There will be many who read it from curiosity, to learn what such a fabulous monster is, and how it views itself. Others will want to find some lessons that they can apply to the education of their own children, or of such other children as may be entrusted to their care. They will ask themselves, and will ask me, some serious questions: Has my career as an infant prodigy been more beneficial or more harmful to me? Would I repeat it if I had the chance? Have I tried to bring up my own children on the same basis, and if I have not, do I wish that I had?

These questions are easier to ask than to answer; in fact, one has only one life to live, and the experiment of that life can scarcely have an accurate control. It might be theoretically possible to carry out a controlled experiment with those curious moieties of human beings known as identical twins, but to carry such an experiment out to the bitter end would presuppose a supreme indifference to the development and the happiness of the individual. My father was no such indifferent tyrant. His was anything but a cold nature, and he was firmly convinced that he was doing his best for me. Thus, in the nature of the case, the answer to these questions must be an emotional guess rather than the precise considered verdict of the scientist.

I have tried not to make this volume a *cri du coeur.* It will nevertheless be manifest to the most casual reader that my boyhood life was not all cakes and ale. I worked unconscionably hard, under a pressure which, though loving, was unconscionably severe. With a heredity which of itself would tend to lead to emotional tenseness, I was put through a course of training that was bound to exaggerate this trait, under the impact of another tense personality. I was naturally awkward, both physically and socially; and my training did nothing to alleviate this awkwardness and probably increased it. Moreover, I was intensely conscious of my shortcomings and of the great demands on me. These gave me an unmitigated sense of difference, which did not make it easy for me to believe in my own success.

I was endowed with what was obviously a very real precocity, and with an insatiable curiosity which had driven me at a very early age to unlimited reading. Thus the question of what was to be done with me was one that could not be put off indefinitely. I myself have met more than one able mind which has come to nothing because the ease with which it has learned has insulated it from the discipline of the ordinary school and nothing has been given with which to replace this discipline. It is precisely this rigorous discipline and training which I received from my father, though perhaps in rather excessive portions. I learned my algebra and geometry at so early an age that they have grown into a part of me. My Latin, my Greek, my German, and my English became a library impressed on my memory. Wherever I may be, I can call on them for use. These great benefits I acquired at an age when most boys are learning trivialities. Thus my energies were released for later serious work at a time when others were learning the very grammar of their professions.

Moreover, I had the chance to sit under a very great man, and to see the inner operations of his mind. It is neither family conceit nor filial loyalty which makes me say this. I have lived the life of an active scholar for a third of a century, and I know very well the intellectual mettle of those with whom I have come into contact. My father's work was marred by flights of fancy to which he was unable to give full logical support, and more than one of his ideas has failed to stand the test of later criticism. To be a pioneer in a subject which, like philology, has a very attenuated inner discipline, is to run this risk. My father was a rather isolated worker, an enthusiast, and a man who had come from a different early career. This made his shortcomings almost inevitable; yet his influence in philology is comparable with that of Jespersen, and was an anticipation of the modern school of philologists who see in the cultural history of a language a stronger stream of continuity than in its merely formal phonetic and grammatical development. Both the phoneticists and the semanticists of the present day have come to a position closer to that of my father than to that of most of his contemporaries.

My work with my father may seem to have been an almost unbroken series of clashes, and indeed the clashes were not few. He was a sensitive man, who felt the lack of the general recognition which he conceived to be his due. He sought for me to be not only his disciple but his friendly critic and perhaps his continuator. These were impossible roles for even a mature trained philologist to hold simultaneously; and they were absolutely out of the question for a half-grown boy. When I expressed any doubts of his logic, and I had some sincere doubts, I was berated as an impudent, unfilial child. Yet I could perceive at the same time the agony of my father and his need for approval. I knew that he sought for approval in what he felt was the one quarter in which he could expect it. Thus my self-protective anger and resentment were not unmixed with pity.

Father was disappointed that his work did not achieve what he considered and what I consider adequate early recognition. He was not by any means a failure, nor did he think he was a failure, either in his intellectual contribution or in the general frame of academic success. As to the latter, Father reached and held the rank of full professor at Harvard, and was without any doubt esteemed very highly as a linguist and philologist of most individual genius. Yet among the very colleagues who esteemed him, I think there were few who realized that the position he was taking in the philological world was revolutionary. Neither do I think that, notwithstanding his respect for his Harvard colleagues, many of them represented to him a stage of philological learning and sophistication which could constitute a code whose judgment had any great meaning for him. Before he had repudiated Germany and Germany had repudiated him, his heart had been set on a German recognition which was unattainable in the closed German philological world of that epoch. Even after his break with all things German, I think he still looked toward Europe and hoped that in some miraculous way, a dove would appear from nowhere with an olive branch in its beak. I think he could never have looked forward, except as a dream, to the present state of affairs in which European scholarship is largely concentrated in America and in which his own point of view, instead of being regarded as a vision of a brilliant eccentric, is accepted and accredited.

Yet the fact that a posthumous success was awaiting him so little as fifteen years after his death can scarcely have mitigated the essential tragedy of his position. And it is possible to be a tragic figure even with an honored position at a great university and a considerable degree of regard among one's colleagues. This position Father had attained, and my mother must be given great credit for taking a brilliant and unworldly man and leading him to that degree of personal success at which he eventually arrived. It was a great success and he knew it. But it was not the position of a re-founder of a science which he deserved and to which he aspired. He had aspired to be Prometheus bringing light, and he suffered in his own eyes the fate of a Prometheus.

From him I learned the standards of scholarship which belong to the real scholar, and the degree of manliness, devotion, and honesty which a scholarly career requires. I learned that scholarship is a calling and a consecration, not a job. I learned a fierce hatred of all bluff and intellectual pretense, as well as a pride in not being baffled by any problem which I could possibly solve. These are worth a price in suffering, yet I would ask this price to be exacted of no man who has not the strength to stand up to it physically and morally. This price cannot be paid by a weakling, and it can kill. That I was a boy not only endowed with a certain intellectual vigor, but also physically strong, made it possible for me to bear the wounds of this Spartan nurture. Before I should even think of subjecting any child, boy or girl, to such a training I should have to be convinced not only of the intelligence of the child, but of its physical, mental, and moral stamina.

Even if we take this stamina for granted, it is a special treatment only to be employed where no ordinary treatment is adequate to the needs of the case. With my own children, indications of the need for such a highly specialized procedure have not occurred. At no time have I tried to subject them to a similar training. I cannot say what I should have done if I had found myself faced by the problem that faced my father.

Nevertheless, to confine one's interest only to that part of my development in which my father participated most directly would be to misread the lesson of this book. By the time I had obtained my doctor's degree at Harvard I had completed the usual formal education of the American boy who goes into scholarship. But neither in age nor in sophistication was I ready to take my place in the scholarly world and to earn my living. It is important for me to tell not only how I fell into the rather specialized and stagy-looking life of the infant prodigy, but how I fell out of it and returned to a possible norm. For it is my opinion that this is of interest and importance equal to that of the departure from this norm.

Before I could take my full place as a mature scholar in the world it was necessary for some of the special conditioning which made me to a certain extent an object of show to be replaced by a basis of general experience which must ultimately come to every boy in his teens. I had to learn to study away from the example of my dominating father, and to regulate my affairs among people to whom my record as an infant prodigy meant exactly nothing. I had to become a reasonably competent teacher and to know my assets and limitations in that field. I had to get my hands black in an industrial laboratory and to acquire the satisfaction of working with tools as a member of an active team of men. I had to find out that writing for a living is not done by fits and starts but is a disciplined act which must be repeated for so many hours each day. It was necessary that I should come to see that mathematics was something that dealt with actual numbers and measurements found by observation, and that the results of this mathematics were subject to a critical scrutiny for their accuracy and applicability. And, because I came to maturity in a generation of war, I had to have the knowledge in my own person of what it meant to be a soldier, if not a warrior.

In the career of the average scholar, many of these lessons are learned in the teens and are followed by a period in the twenties when quite as rapid progress is made as I made at an earlier age. This is the more normal procedure and there is much to be said for it. But I hesitate to pronounce dogmatically whether it is better or worse than the alternative procedure which I followed. On the one hand, there have been social difficulties in my nature which not even my belated professional career has eliminated. On the other hand, in this varied period of manifold experiences, my eyes were already open so that I could see and classify and organize in terms of some central principles the mass of individual items that came to my attention. I could come very near the boast that not one of these seemingly desultory years of finding myself was wasted, and that I have integrated them all into a later career centering about a few highly organized principles.

Yet from a contemporary point of view, it must have seemed that I had stepped down from the brilliant glow of publicity belonging to a career of a *Wunderkind* into the half-light of a slightly alleviated failure. I think this interpretation of my career, which would have looked very plausible at about the time I came to the Massachusetts Institute of Technology, is not the true one. I have chosen for the work of my later years the study of communication and communication apparatus. This is a subject with linguistic and philological sides which I have learned from my father, with engineering techniques to which I received my apprenticeship in the General Electric laboratories and at the computing table at Aberdeen Proving Ground, with mathematical techniques stemming from my days at Cambridge and Göttingen, and with the compelling need for a competent vehicle of literary expression which has proceeded from my work on the *Encyclopedia* and the Boston *Herald*. My routine task of assisting a Japanese professor has borne fruit in my teaching in the Orient and my contact with Oriental scholars. Even my exile at the University of Maine, which was a chastisement for me, has proved in the long run to be a salutary chastisement and a true discipline for a man who was to make his living as a teacher and who had the necessity of making his mistakes early when they were of no great seriousness.

This did not result from any particular plan on my part or on the part of my father. The man who wants to work in diverse branches of science must be prepared to take his ideas where he finds them and to use them where they become applicable. Everything is grist to his mill. Indeed, the peculiar advantage of the ex-infant prodigy in science—if he has any advantage and has managed to come through his discipline without major trauma—is that he has had a chance to absorb something of the richness of many fields of scientific effort before he has become definitely committed to any one or two of these. Leibnitz was an infant prodigy, and in fact the work of Leibnitz is precisely the sort of work for which the training of the infant prodigy is peculiarly suitable. The scientist must remember and he must reflect and he must correlate. It does not change the situation in any fundamental way that the field of science has so grown that the scholar of the present day must perforce be nothing more than a half-Leibnitz. The task of scientists is even more essential than it was in Leibnitz's time; and if it cannot be fulfilled with the completeness which seemed possible in the seventeenth century, that part of it which can be carried out is more demanding and less avoidable.

All of this represents a view backward from my later years and not a view forward from my childhood. I began my work early, but my accomplishments did not begin to take their full form until my middle twenties. There was much trial and I went up many false alleyways in going through the maze of life. Yet I doubt that a more single-purposed and unmistaking career would have been better for me in the long run. I do not think that a scientist is at his best until he has learned to draw success from confusion and failure and to improvise new and effective ideas on the basis of procedures which he has begun fortuitously and without purpose. The man who is always right has not learned the great virtue of failure. Intellectual achievement involves a calculated risk and in many cases even an uncalculated risk; but one thing is sure: where nothing is ventured, nothing is gained.

This I should like to say to the administrators of research and education within and without our universities. Theirs is the task of judging the promise and performance of gifted and struggling young men and women, and their decisions may profoundly affect the careers of these young people. The youngsters whom they have to judge must perforce do much of their work in fields in which there do not yet exist any accepted criteria of performance. All true research is a gamble, and the payoff is anything but prompt. A fellowship is a long-term investment in a man, not a sight draft nor a paper collectible twelve months from issue. Creativity cannot be hurried, and Clio takes her time in handing out the awards.

As to the problems of my earlier life which are associated with the fact of my Jewish origin and my discovery of it, these have evaporated with time. I have found in my wife's attitude support for a definite course of conduct and security in that course of conduct. As I have said, this is to sublimate the problem of prejudice directed against a group to which I belong into the problem of prejudice against undervalued groups in general. In addition, whatever temporary recrudescences anti-Semitism may show, it has ceased to be a really important factor in the environment in which I live, and to a large extent in the country as a whole. Among those places at which anti-Semitism is at a low level and has ceased to be an important factor in our daily lives, the Massachusetts Institute of Technology stands at the top.

This decided lessening of anti-Semitism is the result of many factors. The shame of Hitler's anti-Semitism has cut deep into the spirit of most Americans and it is no longer fashionable or even tolerable to adopt such a discredited spirit. Furthermore, the Jews, like many other immigrant groups, have developed a new generation which has grown up with American speech and American mores and which no longer combines the prejudice-forming factors of different dress, language, and background with the prejudice against religious differences and the Jewish group in particular. The struggle for emancipation from the ghetto does not have any very great emotional content for those to whom this emancipation is an old story. Yet the battle against prejudice is never won, and it must be fought on every front where it appears.

All in all, it has come out pretty even in the end. The question of one's social awkwardness looks pretty small after one has gone through the vicissitudes of fifty-eight years of life and has found oneself reasonably able to cope with them. The early start I have had does not appear to me to have impeded me from showing a period of productivity continuing reasonably late, and has greatly increased the level at which I started this productivity. Thus it has added years to my useful life.

I certainly do not look back upon my career as one blighted by my earlier experiences, nor do I feel any particular self-pity for having been "deprived of my childhood," as the jargon goes. That I have arrived at this degree of equanimity is due in particular to the love, advice, and criticism of my wife. Alone, I should have found it difficult and probably impossible of accomplishment. Yet it has been achieved. And now, with increasing age, I find that the image of myself as an infant prodigy has been obliterated in the minds of my acquaintances as well as in my own mind. The question of the success or failure of my adolescence and postadolescence has become unimportant to me as to everybody else through the larger issues developed during my career as a working scholar.

II I Am a Mathematician: The Later Life of a Prodigy

To the Massachusetts Institute of Technology, which has given me the encouragement to work and the freedom to think.

Preface to *I Am a Mathematician: The Later Life of a Prodigy*

I have just finished dictating the last word of this book. It represents that part of my autobiography which dates roughly from my arrival at M.I.T. in 1919, when I was twenty-four years old. The earlier part of my autobiography, under the title *Ex-Prodigy,* concerned my childhood and adolescence, while the present book is occupied with my mature personal and scientific career.

It deals with my work, my travel, and my personal experiences, and I hope that it will give a fair account of my intellectual development. My problem has been to explain to a public that is generally not too deeply interested in science, and which is certainly not possessed of a technical acquaintance with it, the development of ideas which are fundamentally scientific. I have had as far as possible to eschew the technical vocabulary of science and to translate my concepts into everyday language. This is a splendid discipline for an author, but it also is a discipline which runs the risk of falling short of full success. While the use of scientific terms often results in jargon, to express any significant part of scientific ideas without the compact denotation and connotation which the history of science has given these terms is most difficult and is much less likely to be completely successful than the specifically literary critic may be aware.

Thus, I have had two years discipline at a piece of work in which, by the nature of things, I must wait for the judgment of the public in order to be sure that I have achieved a measure of success. Why did I assume this uninviting labor, which at the best can add little to my stature as a working scientist and at the worst must offer new opportunities for those who may be inclined to criticize me? All in all, I don't know. There certainly have been motives of literary vanity and the desire to show that, both as an individual and as a scientist I have been able to accomplish a task off my regular beat.

Admitting this freely, there are other, more important motives. As in the first volume of my autobiography, so here, too, I wish to think out to myself what my career has meant and to come to that emotional peace which only a thorough consideration and understanding of one's own past can bring. I have also wished to make this understanding available to young men coming up through similar careers in mathematics and the other sciences. I have felt that the scientist, his mode of life, and the demands on him are not sufficiently known to the larger intellectual public, and I believe that here I have the duty of exegesis. Then, finally, I have not had any previous opportunity to write up many ideas—treated singly in my existing literary work—in the form of a consistent historical account of how I came by them.

I should like to mention, among the colleagues with whom I have discussed this undertaking, Professor Karl Deutsch of the M.I.T. Department of Humanities; Professor Armand Siegel of Boston University; Dr. Dana L. Farnsworth, formerly of the Medical Department at M.I.T. and now professor of hygiene at Harvard University; and Dr. Morris Chafetz. In addition, I wish to give my thanks to the several members of the sequence of secretaries who have taken my dictation, who have expressed their criticism of my ideas, and who have helped put my work into printable form.

Norbert Wiener
Cambridge, Massachusetts
Washington's Birthday, 1955

20 My Start as a Mathematician

This book is the second volume of my autobiography. The first volume, entitled *Ex-Prodigy*, was devoted to my early education, to my relations with my father, and to the unusual experience of being an infant prodigy. The present book is devoted to my career as a working mathematician.

For purposes of organization, I must begin this volume at some definite period, and the natural time to begin it is in 1919, when I first came to the Massachusetts Institute of Technology. I was then twenty-four years old: too old to be an infant prodigy any longer but not too old to have the marks of having been one firmly impressed upon me.

I do not intend to go back to my childhood, which was the subject of my earlier book, to explain the origins of these marks, but I must report certain features of my adolescence which in the previous volume represented the last stages in the life of a child but here are rather to be interpreted as the first stages in the life of a man.

Of all the influences which operated on me during my childhood and adolescence, the most important was my father, Leo Wiener, professor of Slavic languages and literatures at Harvard University—a small, vigorous man, of emotions both deep and quick, sudden in his movements and his gestures, ready to approve and to condemn, a scholar rather by nature than by any specific training. In him were joined the best traditions of German thought, Jewish intellect, and American spirit. He was given to overriding the wills of those about him by the sheer intensity of his emotion rather than by any particular desire to master other people.

Having been more than twenty years in the shadow of such a man, with the knowledge that I was flesh of his flesh and bone of his bone, I myself was cast into a mold sharply different from that of most of those about me. I became a scholar partly because it was my father's will but equally because it was my internal destiny. From my earliest years I had been interested in the world about me and very inquisitive into its nature. I had learned to read by the time I was four, and almost from then on I plunged into scientific reading of the most varied character. By the time I was seven, my reading had come to range from Darwin and Kingsley's Natural History to the psychiatric writings of Charcot and Janet and others of the school of the Salpétrière. This miscellaneous collection of learning had been assembled in those remarkable and diversified volumes of small and blunt print known as the Humboldt Library.

My own free curiosity was matched by my father's insistence that my training be disciplined. I learned my science for myself; my father introduced me to languages, both ancient and modern, and to mathematics. All these subjects had a certain interest for me, but no casual interest could satisfy my father's demands for precise and ready knowledge. These demands were severe and painful, but they were made tolerable by my complete awareness of my father's integrity and intellectual power.

The arduous course of training to which I was put tended to isolate me from the world and to give me a certain aggressive, unlovable naïveté. I played a good deal with other boys, but I was not greatly welcomed by them. When I entered high school, at the age of nine, the few companions that I found were not among the high-school boys but among children of my own age.

The special nature of my position was aggravated by a nearsightedness which at one time seemed to threaten my vision altogether. While this had no direct effect on my physical vigor, it cut me out of that whole sector of boyish life which depends on skill at games. It also tended to accentuate my very marked physical clumsiness. This clumsiness was serious enough on its own merits, but it was further brought out by the way in which my father harped on it and used to humiliate me concerning it. He himself was no prodigy of manual skill, but he was interested in farming, gardening, and an outdoor life, and he used his limited skills to the best possible advantage. His discomfiture at my shortcomings was real.

With the inevitable isolation which my father's training gave me, I was a very self-conscious hobbledehoy, subject to alternate moods of conceit when I became aware of my abilities and of great disappointment when I accepted at their face value my father's strictures on my shortcomings, or when I contemplated the long and uncertain road to achievement to which my highly eccentric bringing-up had condemned me. Moreover, I had my father perpetually before me as an example of a certain admissible aggressiveness, although its naturalness and to some extent its justifiability made it less devastating to me than it would otherwise have been.

Superimposed upon this was another source of unsureness which continued to haunt me for a large part of my life. My mother, like my father, was a Jew. Unlike my father, she resented being a Jew. My father and mother concurred, it is true, in assimilationism and in a desire to identify themselves and their children with the general American environment. But while this desire took a defensible form with my father, whose many interests allowed him to view this fact of our origin in something like a proper perspective, anti-Semitism became, in all its starkness, the chief subject of my mother's concern.

On the one hand, we children were brought up not only in ignorance of our origins but under a directly false impression of them. On the other hand, we could at no time fully fail to realize that there was some unexplained Jewish element in our environment. My mother made many uncomplimentary remarks concerning the Jews, which went far to impress on me that she considered her Jewish origin, and consequently our own Jewishness, a source of shame. When, later—at the age of fifteen—I learned from my father that we were unquestionably Jewish, remembered remarks of my mother forced on me a sense of inferiority which contributed greatly to my insecurity so that it was a matter of many years before I could acquire a decent measure of self-confidence. Thus, I was subject to an alternation of awareness of my powers and doubts as to my value; and these made me continue to oscillate between an unlovely conceit and an equally unlovely abjectness.

There were, however, certain important factors in my makeup which made for success in general and for intellectual success in particular. My father's independence had been reflected in both my nature and my habits. His power did not consist merely in a high level of intellectual ability but in a willingness to supplement this ability by hard and unceasing work. I had seen my father bring himself to a breakdown by the herculean task of translating twenty-four volumes of Tolstoy in two years. What Father expected of himself he expected of me as well, and from childhood on I have known no period when I was content to rest on past accomplishments.

From high school I proceeded to Tufts College, near Boston, and later to graduate work at Harvard and at Cornell. I took my bachelor's degree at Tufts at the age of fourteen and my doctor's degree in philosophy at Harvard at eighteen. As I gradually acquired a limited amount—a very limited amount—of independence from my father, I found that the dawning freedom of approaching manhood was largely a freedom to make mistakes and to know failure. Yet even this joyful freedom was limited by my father's proneness to make sudden decisions affecting my whole future which bound me as much as if these decisions had been my own.

After obtaining my Ph.D. I was appointed by Harvard University to a traveling fellowship which I spent at Cambridge, England, and later at Göttingen, in Germany. Although I had been away from home before, this was the first time that I had come to be really competent to live alone and could learn something about the freedom of an independent worker. My chief teacher and mentor was Bertrand Russell, with whom I studied mathematical logic and a good many more general matters concerning the philosophy of science and mathematics. Russell, who looked then, as he does now, like the Mad Hatter, gave beautifully finished discussions largely devoted to Einstein's new theory of relativity. I studied Russell's own mathematical-logic writings with a small group of students who met in his rooms, and I followed other courses which he recommended to me. The chief of these was a higher-mathematics course under G. H. Hardy, later to become a professor both at Oxford and at Cambridge and perhaps the greatest figure of his mathematical generation in England.

My doctor's thesis at Harvard had been on the philosophy of mathematics. Russell impressed upon me that to do competent work in the philosophy of mathematics I should know more than I did about mathematics itself.

Hardy, to whom I turned, was an ideal mentor and model for an ambitious young mathematician. I had first seen him in Russell's rooms when my father took me to Cambridge and left me in an absent-minded way to sink or swim there. At that time, Father and I both took Hardy to be an undergraduate, a shy, self-effacing young man who, as I came to learn later, was an excellent athlete and an authority on every game played with a ball. In his later years he was to become a rather dried-up, wizened figure, in the inevitable unpressed jacket and bags of a Cambridge don: kindly and helpful but detached and desperately afraid of women, and it is in this way that I remember him best.

His course was a delight to me. My previous adventures into higher mathematics had not been completely satisfying, because I sensed gaps in many of the proofs which I was unwilling to disregard—and correctly too, as it later turned out, for the gaps were really there and they should have disturbed not only me but my earlier teachers. Hardy, however, led me through the complicated logic of higher mathematics with such clarity and in such detail that he resolved these difficulties as we came to them and gave me a real sense of what is necessary for a mathematical proof. He also introduced me to the Lebesgue integral, which was to lead directly to the main achievements of my early career.

The Lebesgue integral is not an easy conception for the layman to grasp, but since an awareness of it is fundamental to this book, I shall try to suggest, if not its full complexity, at least its main theme. It is easy enough to measure the length of an interval along a line or the area inside a circle or other smooth, closed curve. Yet when one tries to measure sets of points which are scattered over an infinity of segments or curve-bound areas, or sets of points so irregularly distributed that even this complicated description is not adequate for them, the very simplest notions of area and volume demand high-grade thinking for their definition. The Lebesgue integral is a tool for measuring such complex phenomena.

The measurement of highly irregular regions is indispensable to the theories of probability and statistics; and these two closely related theories seemed to me, even in those remote days before the war, to be on the point of taking over large areas of physics. They stood approximately in the middle ground where physics and mathematics meet, and this middle ground was just where I was eventually to do my best work, for such work seemed to be in harmony with a basic aspect of my own personality.

Even more, they anticipated the main tendencies of modern mathematics, grounded in physics and dependent on the ideas of measure and probability expounded in the statistical mechanics of that great American scientist, Josiah Willard Gibbs. The development of problems surrounding the application of mathematical ideas into problems that arise in the physical world constitutes one of the main themes of this volume.

The theory of the Lebesgue integral leads the student from the measure of intervals to the measure of more complex phenomena obtained by combining sequences of intervals, and then to sets which can be approached by such sequences, while the sets of points excluded from them can be approached in a similar manner. There is nothing in it which can be explained satisfactorily to the layman, but neither is there anything which involves an excessive complication of logic and manipulation. It enabled Lebesgue to extend the notion of length or measure from the single interval to the extreme significant limits at which measure is possible.

Hardy died some years ago, but his younger colleague and research partner, J. E. Littlewood, with whom I also worked, is still alive. At that time, Littlewood seemed to me merely a bright young man among other bright young men, but, as I got to know him later, I learned that he was a great rock climber as well as a mathematician. He had the small, muscular, compact build of the true rock climber, and both as a rock climber and a mathematician he showed an unlimited power and an impeccable technique.

In their long partnership, Hardy and Littlewood occupied easily distinguishable roles, for whereas Hardy was the man of clarity and original ideas, Littlewood was the man of power and indomitable persistence. Curiously enough, Littlewood was the more self-effacing of the two. Later on, when he visited Edmund Landau at Göttingen, that irrepressible, spoiled child of mathematics said to him, "So you do exist! I thought you were merely a name used by Hardy for those papers which he didn't think were quite good enough to publish under his own name."

Landau and David Hilbert became my teachers later the same academic year, when I studied in Göttingen in the spring term of 1914, just as the First World War was about to break out. Landau came from a rich Jewish banking family and had been something of a prodigy himself. He had been brought up with every luxury which his wealthy parents were able to provide. A small, cherubic figure with a bristling little mustache and a completely undisciplined conceit, he always seemed a bit out of place in the real world. When people asked where to find his house in Göttingen he would say quite naïvely, "You will have no difficulty in finding it. It is the finest house in the town."

Hilbert, with whom I also studied, was a very different sort of person, a quiet, peasantlike man from East Prussia. He was conscious of his strength but genuinely modest. He used to say about his son, who certainly lacked the powers of his father, "He has his mathematical ability from his mother, everything else from me."

Hilbert himself had taken up, in succession, most of the difficult problems in every field of modern mathematics, and in each field he had made a major killing. He represented the grand tradition in the mathematics of the beginning of the century. For me as a young man he became the sort of mathematician whom I would have liked to become, combining tremendous abstract power with a down-to-earth sense of physical reality.

At Cambridge, Russell had impressed on me not only the importance of mathematics but the need for a physical sense, and he had suggested that I study the new developments of Rutherford and others concerning the theory of the electron and the nature of matter. At that time I did not make much headway in these subjects, but at least I was given a preview of that theory of the atomic nucleus which has since given rise to the transmutation of the elements and the construction of the atomic bomb. This theory has come to occupy a position alongside Einstein's theory of relativity, the importance of which I also learned from Russell. I found Russell's respect for physics reinforced in the scientific atmosphere of Göttingen.

After Göttingen I came back to New Hampshire for the summer just as World War I was breaking out. I returned to England for the next academic year at Cambridge, but in that atmosphere of calamity and doom I did not find many people with a heart to do serious scientific thinking nor was I myself able to carry on to any very good effect. In the late winter of 1914–1915 the German submarine campaign became threatening, and I was summoned home by my father.

The war took some years to come to America, but it was never out of my thoughts. The present generation, which has been brought up with crisis as its daily associate, can scarcely be aware of the shock with which the war impinged on my contemporaries. We had been brought up to consider the long Victorian peace as normal, and we had hoped for a continued and slow evolution toward better conditions. Not even now, forty years after, have we really been able to accept the prolonged succession of catastrophes about us as normal. I am afraid that we all have from time to time a sneaking hope that we shall wake and find ourselves again in the dull, mild days of the beginning of the century.

During the early period of the First World War I carried on a number of activities, scientific and personal, in a desultory way. I had a certain idea at the back of my mind that the war would be over some day fairly soon and that then we could return to our old settled habits of living and make long plans. I finished the incomplete academic year of 1914–15 at Columbia, listening impatiently and impolitely to various professors who did not impress me after my experience with Hardy and the Harvard philosophers. I was a pest to the older men in the graduate dormitory with my self-assertiveness and bad bridge-playing, and I filled in my abundant but lonely leisure with long walks from Columbia University to the Battery, with an overdose of films and theater in between.

My mathematical work was active but abortive. I made some attempts to apply the abstract way of thought which I had learned from Russell to topology, that strange branch of mathematics dealing with knots and other geometric shapes whose fundamental relations are not changed even by a thorough kneading of space so long as nothing is cut and no two remote points of space are joined. Topology includes the study of such things as the familiar one-sided Möbius's sheet of paper, which you get when you take a long, flat strip, rotate one end of it through half a revolution, and glue the ends. It makes an excellent parlor trick to ask a layman what will happen to such a strip if you start cutting it down the middle until the ends of the cut meet. If you try this, you will find that even after the cut is complete the strip will remain in one piece but will now make a full revolution instead of half a revolution as you proceed around it.

Not many years later topology became *the* fashionable branch of mathematics, particularly in America, under the guidance of Oswald Veblen and J. W. Alexander. But by this time I had grown disappointed in the slowness in which my work led to positive results and had destroyed or lost the manuscript on topology which I had worked on at Columbia.

In the academic year 1915–16 I returned to Harvard as docent lecturer and assistant, and I gave a series of lectures in accordance with the peculiar clause in the University statutes which conveys to every Ph.D. the privilege of giving such lectures strictly on his own responsibility. The subject I chose had to do with the work of Alfred North Whitehead, and my lectures were devoted to showing how mathematics might be based on processes of logical construction. Whitehead had shown by examples how such methods might secure for various mathematical concepts those properties which another mathematical school, that of the postulationalists, had sought as the consequences of rather arbitrary formal assumptions. For example, it was Whitehead who had thought of representing a point as the set of all areas which, according to more usual mathematical language, would be said to contain this point. But my lectures ran into certain logical difficulties which were clearly pointed out to me by Professor G. D. Birkhoff of Harvard, to whom I shall have many occasions to refer later in this book.

He was a lean, tall, Michigan Dutchman, with the drawn face and tight mouth of a rigid Calvinist, and the first important American mathematician to have had all his training in America. He had written a brilliant dissertation on certain branches of dynamics, concerning in particular the mechanics of the planets, a field which had been outlined in France by Henri Poincaré. Birkhoff was fully conscious of his really great powers and was determined to become and to remain the first American mathematician in those classical branches of mathematics known as analysis, which constitute the extension and the elaboration of Newton's calculus and physics.

He was, as I was later to learn, intolerant of possible rivals, and even more intolerant of possible Jewish rivals. He considered that the supposed early maturity of the Jews gave them an unfair advantage at the stage at which young mathematicians were looking for jobs, and he further considered that this advantage was particularly unfair, as he believed that the Jews lacked staying power. At the beginning I was too unimportant a youngster to attract much of his attention, but later on, as I developed more strength and achievement, I became his special antipathy, both as a Jew and, ultimately, as a possible rival.

At the time at which I had first felt Birkhoff's hostility I had not been fully aware of all the forces contributing to it and lying within me and my immediate environment. I have suggested that I was not a very amiable young man. Frankly it was scarcely to have been expected that I should have been amiable. I have spoken at length about the fact of my having been a prodigy, and I need not say much about the distaste which established people felt when they were confronted with a youngster whom they did not know how to place. A career devoted to scientific achievement from the years of childhood leaves very little time for the cultivation of the social graces.

Even with all these things taken into account, I was an aggressive youngster. I felt at the bottom of my consciousness that it would take all the aggressiveness I might have to force success from the very ambiguous situation in which I found myself. Moreover, my father—who, notwithstanding all the elements of conflict between us, was my ideal and my closest mentor—was himself a very aggressive personality.

I had heard many echoes of the somewhat trivial disagreements which my father had had with his colleagues, but I was not fully aware of the fact that in addition to these individual instances he had been regarded as an essentially quarrelsome man. Part of this reputation of his was justified, but an even more important part was due to a misunderstanding of his mercurial temperament by less mobile personalities. Many years after the event, I have come to learn that a not inappreciable element in Birkhoff's antagonism toward me was due to his misunderstanding of my father and to the distaste with which he received my father's somewhat uninhibited boastings concerning myself.

The next year found me at the University of Maine, where I had secured a job through a teachers' agency. I felt humiliated that I could not get a job directly on the prestige of my academic record. The tribulations of this period of what I considered an exile belong to my previous volume and have been discussed there.

At the end of the academic year the United States entered the war. I tried unsuccessfully to join one of the services but was rejected everywhere because of my poor eyesight. I worked for a brief period at the General Electric works at Lynn, from which job I was withdrawn by my father for what he considered the better offer which I received: that of a hack writer on the *Encyclopedia Americana* at Albany.

I left this job to work, together with a mixed bag of civilian and military mathematicians, at the Aberdeen Proving Ground in Maryland. The work concerned problems of the design of range tables for artillery weapons. Here I spent more than half a year, first as a civilian and then as a soldier, where I fared rather poorly since the fact of having been an infant prodigy led me to commit many blunders which were not intended in a vicious spirit but which may have left a disagreeable impression of my whole personality. I was desperately unhappy under barracks conditions and did nothing to endear myself to my companions. I was honorably discharged from the service in February 1919.

After several months of newspaper hack writing, I composed a couple of scientific articles on algebra which were good enough in their own way, but which have remained completely off the beaten track. Then Professor W. F. Osgood of Harvard secured me an appointment as instructor in the department of mathematics of the Massachusetts Institute of Technology.

Osgood was a friend of my father's, and his sons had been to some slight extent my playmates years before. He was perhaps the chief representative of the German tradition in American mathematics, having studied at Göttingen, where he married a German girl, and brought back the determination to live in America the life of a German professor. Perhaps I should say the life of a German privy councilor, because his model in all things was Felix Klein, who had been for years the pope of German mathematics and had enjoyed the exalted title of Geheimrat. Osgood was a sturdy man of ruddy complexion, already getting very bald, with a spreading spade beard after the European manner. He used to pontificate at the meetings of the Harvard Mathematics Club, where he smoked his cigar after a fashion which he had obviously learned from someone else, and which we later found he had got from Felix Klein himself. He would spear it with his penknife and smoke the stub to the bitter end.

He wrote some of his books in a German of a very tolerable quality and had strong quasi-moralistic ideas of what was right and what was wrong in mathematics quite apart from any question of simple logical correctness. Those who worked under him found that he considered them bound by these ideas.

I have perhaps been insufficiently grateful to Professor Osgood for the really good turn he did me in securing me the call to the Massachusetts Institute of Technology—or M.I.T., as it is more frequently known. There were, however, certain offsets to this act of kindness. For one thing, I never felt that I had earned any real esteem from him, nor did I feel that he had made me welcome at Harvard. Furthermore, jobs were plentiful with the resumption of normal life after the war. M.I.T. needed a large number of men for routine teaching. As far as the higher administration of M.I.T. was concerned at the time, routine teaching and nothing but routine teaching was the function of the mathematics department.

There were, it is true, a few devoted spirits in the M.I.T. mathematics department who hoped against hope that the day was to come when M.I.T. would take its place beside Harvard and Princeton as a great center of creative American mathematics. These mathematicians kept bravely defying an unfavorable environment, for M.I.T. was then simply an engineering school, and mathematics was regarded as only a tool to implement engineering training. Nonetheless, they found a certain tolerant sympathy even if not much active co-operation from Professor H. W. Tyler, the head of the department. Professor Tyler, a small, active, bearded man, was not himself a research scholar and was originally quite content with his department as a service department: that is, as one contributing to the education of people whose real interests were in engineering fields. But, like every good administrator, he was ready to seize any opportunity for the advancement of his department, and later, when the chance for a certain research prestige came, he stood behind us.

Many of my colleagues were friendly, but the one who did the most to encourage me was C. L. E. Moore. He had the human gift of affection and a love for mathematics that stimulated others to reach a level to which he could never aspire himself. I wish to pay tribute here to the selflessness and the integrity of this tall, slightly awkward, humorous, and kindly man.

During my first years at M.I.T. I lived at home. My older sister, Constance, had been through Radcliffe and was engaged in graduate study in mathematics at Chicago. The accounts she gave of her rather orthodox mathematical training excited my ambition but left me very much in doubt as to whether I was really aimed toward any large success. My younger sister, Bertha, was studying chemistry, first at Radcliffe and then later at M.I.T.

I tried to develop a certain social life at the time through the Sunday teas which my parents held at their house and among my sisters' friends. In this life I remained clumsy and was still subject to the peremptory demands of my parents. They tried hard to pick my friends for me and to reject those whom they did not find suitable. In fact, they exercised a complete right of veto over the young women to whom I was paying attention. This veto was governed more by what my parents conceived to be the girls' reaction to the rest of the family than by any factor directly concerning me. It left me frustrated and confused and always more determined to make use of my summer vacations to get out from under the burden of family dominance.

The Institute of Technology was at least one place where family pressure could not easily follow me. I taught a heavy schedule, more than twenty hours a week, but I still found time not only to study mathematics but to create it. In the strength of my youth I spent the whole day at M.I.T., from nine in the morning to five in the evening, and even then I could find no greater delight than to pass my Sundays (Saturdays were working days) in my deserted office, my thoughts undisturbed by anyone. One fifth of what I did then would be too much for me now.

As to my amusements, besides the movies and the repertory company at the old Copley Theater, I went for walks in the Blue Hills or the Middlesex fells and undertook the construction of a crude toboggan with which to glide down the slopes behind the Mount Auburn cemetery. I had a few friends among my younger colleagues and the graduate students at Harvard. In the winter I used to walk on the ice to M.I.T. and to Boston from our home on Sparks Street, and in the spring and the autumn I engaged in a little mild and very incompetent tennis.

It was at M.I.T. too that my ever-growing interest in the physical aspects of mathematics began to take definite shape. The school buildings overlook the River Charles and command a never changing skyline of much beauty. The moods of the waters of the river were always delightful to watch. To me, as a mathematician and a physicist, they had another meaning as well. How could one bring to a mathematical regularity the study of the mass of ever shifting ripples and waves, for was not the highest destiny of mathematics the discovery of order among disorder? At one time the waves ran high, flecked with patches of foam, while at another they were barely noticeable ripples. Sometimes the lengths of the waves were to be measured in inches, and again they might be many yards long. What descriptive language could I use that would portray these clearly visible facts without involving me in the inextricable complexity of a complete description of the water surface? This problem of the waves was clearly one for averaging and statistics, and in this way was closely related to the Lebesgue integral, which I was studying at the time. Thus, I came to see that the mathematical tool for which I was seeking was one suitable to the description of nature, and I grew ever more aware that it was within nature itself that I must seek the language and the problems of my mathematical investigations.

Of my many older colleagues at M.I.T., one in particular helped me to become aware of the physical side of mathematics. Henry Bayard Phillips, whose active days are not over yet, is a long, lean, ageless Carolinian who grew up during a period when the bad days after the Civil War were no remote memory. He was and is the complete individualist, with more interest in doing new things than in publishing them. It was from Phillips, more than from anyone else, that I learned the importance to the pure mathematician, of a physical attitude and that I became aware of the great work of Willard Gibbs on statistical mechanics. This was an intellectual landmark in my life.

Willard Gibbs, America's greatest scientist, had himself worked in this middle ground and indeed had pioneered in it. He had lived a quiet, retired life at Yale, where he died in 1903, almost unknown to his students and colleagues. He made many contributions, both to physics and to mathematics, but the part of his work which has always been of the most interest to me was his statistical mechanics. And it is with respect to his work in this area that he exercised such a profound influence on my own career.

The great physical tradition of Newton had necessarily been one of determinism, where a perfect knowledge of the universe at one instant is understood to involve a perfect knowledge of its history throughout all time. It would have been Newton's assumption that to give the present positions and speeds of the particles in a wave moving across the surface of the Charles would allow us to plot the movement of all this wave forever. Unfortunately, no perfect knowledge of the present is available to us with our limited measuring instruments, and the problem that faces the working physicist is to find out how far he can go with the imperfect knowledge available to him.

For this he must work not with a single, fixed universe but with many different universes simultaneously, each having some preassigned probability. He cannot tell you what will always happen but what may happen at certain times, given certain conditions. This new physics of probability had been in the course of development for a considerable period, but it was toward its final clear formulation that the work of Gibbs was unquestionably tending.

When I came to M.I.T., I was intellectually prepared to be influenced by the work of Gibbs. Just before my term work had started, Dr. I. Barnett, of Cincinnati, had turned up in Cambridge and had talked various mathematical and personal matters over with me. As this was the first time that I had taken on a really mature job in mathematics, I was curious as to what problem should form the center of my new work. I asked Barnett to suggest a new and lively problem, and he mentioned to me that a lot was being done on the generalization of the concept of probability to cover probabilities where the various occurrences being studied were not to be represented by points or dots in a plane or in a space but by something of the nature of path curves in space.

For example, a one-point probability problem might concern the distributions of bullet holes in a target and ask what we can say in advance about the way they will bunch in the bull's eye. On the other hand, a curve probability problem will come up if we are asked to characterize the flight of a bee or, even better, the walk of a man who is so drunk that there is no relation at all between the direction of his previous step and his present step. If we put such a man in the center of a square field of given dimensions, how long will it take on the average for him to get out of that field?

This new concern with the probable outcome of irregular behavior has a certain historical significance. The early part of the twentieth century had seen a change in mathematics toward a more complicated world view. The great interest of the nineteenth century had been the study of points and of quantities depending on points. The new concern tried to do for curves what the older analysis had done for points.

The roots of this new emphasis were firm in the work of the nineteenth or even of the eighteenth century, which were concerned with the calculus of variations. The straightforward differential calculus of Newton and Leibnitz had discussed such maximum and minimum problems as those of the summit of a hill or the bottom of a bowl, or the related problem of the shape of a mountain pass. The calculus of variations discussed such problems as that of the quickest way of going from one point to another by a curve through a region in which the possible velocity of travel varies from point to point.

Yet, although the origins of a mathematics of maxima and minima for curves were very old, the full development of the subject was not so old. The world of curves has a richer texture than the world of points. It has been left for the twentieth century to penetrate into this full richness.

As a result of Barnett's suggestions, I spent my first year at the Massachusetts Institute of Technology investigating various extensions of Lebesgue integration to systems more complicated than those discussed by Lebesgue himself. There was a paper already written in this field by a young Frenchman named Gâteaux, who had been killed in the war. But this was only a fragment; and when I investigated it further it seemed to me to lead in the wrong direction.

There were also a number of papers that hinted at the subject by the English scholar P. J. Daniell, who was at that time teaching at the Rice Institute in Houston, Texas. Daniell's work was to me far more satisfying than that of Gâteaux, and I used it as my own model. It did not, however, specifically concern families of curves, and the work in which I followed it up to cover this new field seemed to me at the time artificial and unsatisfying.

I was an avid reader of the journals, and in particular of the *Proceedings of the London Mathematical Society.* There I saw a paper by G. I. Taylor, later to become Sir Geoffrey Taylor, concerning the theory of turbulence. This is a field of essential importance for aerodynamics and aviation, and Sir Geoffrey has for many years been a mainstay of British work in these subjects. The paper was allied to my own interests, inasmuch as the paths of air particles in turbulence are curves and the physical results of Taylor's papers involve averaging or integration over families of such curves.

I may remark that in my later visits to England I got to know Taylor very well. He represents a peculiarly English type in science: the amateur with a professional competence. He is a famous yachtsman and has the open-air appearance which belongs to a yachtsman. Indeed, one of the accomplishments of which he is most proud is that of the invention of a new type of anchor for yachts.

With Taylor's paper behind me, I came to think more and more of the physical possibilities of a theory for averages over curves. The problem of turbulence was too complicated for immediate attack, but there was a related problem which I found to be just right for the theoretical considerations of the field which I had chosen for myself. This was the problem of the Brownian motion, and it was to provide the subject of my first major mathematical work.

To understand the Brownian motion, let us imagine a pushball in a field in which a crowd is milling around. Various people in the crowd will run into the pushball and will move it about. Some will push in one direction and some in another, and the balance of pushes is likely to be tolerably even. Nevertheless, notwithstanding these balanced pushes, the fact remains that they are pushes by individual people and that their balance will be only approximate. Thus, in the course of time, the ball will wander about the field like the drunken man whom we have already mentioned and we shall have a certain irregular motion in which what happens in the future will have very little to do with what has happened in the past.

Now consider the molecules of a fluid, whether gas or liquid. These molecules will not be at rest but will have a random irregular motion like that of the people in the crowd. This motion will become more active as the temperature goes up. Let us suppose that we have introduced into this fluid a small sphere which can be pushed about by the molecules in much the way that the pushball is agitated by the crowd. If this sphere is extremely small we cannot see it, and if it is extremely large and suspended in a fluid, the collisions of the particles of the fluid with the sphere will average out sufficiently well so that no motion is observable. There is an intermediate range in which the sphere is large enough to be visible and small enough to appear under the microscope in a constant irregular motion. This agitation, which indicates the irregular movement of the molecules is known as the Brownian motion. It had first been observed by the microscopists of the eighteenth century as a universal agitation of all sufficiently small particles in the microscopic field.

Here I had a situation in which particles describe not only curves but statistical assemblages of curves. It was an ideal proving ground for my ideas concerning the Lebesgue integral in a space of curves, and it had the abundantly physical texture of the work of Gibbs. It was to this field that I had decided to apply the work that I had already done along the lines of integration theory. I met with a considerable degree of success.

The Brownian motion was nothing new as an object of study by physicists. There were fundamental papers by Einstein and Smoluchowski that covered it, but whereas these papers concerned what was happening to any given particle at a specific time, or the long-time statistics of many particles, they did not concern themselves with the mathematical properties of the curve followed by a single particle.

Here the literature was very scant, but it did include a telling comment by the French physicist Perrin in his book *Les Atomes,* where he said in effect that the very irregular curves followed by particles in the Brownian motion led one to think of the supposed continuous non-differentiable curves of the mathematicians. He called the motion continuous because the particles never jump over a gap and non-differentiable because at no time do they seem to have a well-defined direction of movement.

In the physical Brownian motion, it is of course true that the particle is not subject to an absolutely perpetual influence resulting from the collision of the molecules but that there are short intervals of time between one collision and the next. These, however, are far too short to be observed by any ordinary methods. It therefore becomes natural to idealize the Brownian motion as if the molecules were infinitesimal in size and the collisions continuously described. It was this idealized Brownian motion which I studied, and which I found to be an excellent surrogate for the cruder properties of the true Brownian motion.

To my surprise and delight I found that the Brownian motion as thus conceived had a formal theory of a high degree of perfection and elegance. Under this theory I was able to confirm the conjecture of Perrin and to show that, except for a set of cases of probability o, all the Brownian motions were continuous non-differentiable curves.

The papers which I wrote on this subject were, I believe, the first to disclose anything very new—combining the Lebesgue technique of integration with the physical ideas of Gibbs. They did not solve certain of the problems that were implicit in the technical justification of Gibbs's work, which were later to receive a solution in the sense of Lebesgue at the hands of Bernard Koopman, J. von Neumann, and G. D. Birkhoff. This, however, took place in 1930, when the idea that Gibbs and Lebesgue had something in common was no longer a complete novelty.

While I actually wrote my first papers on the Brownian motion, there was another phenomenon which was then coming into view, and which my work could equally be considered to describe. This was the so-called shot effect, which concerns the conduction of electric currents along a wire or through a vacuum tube in the form of a stream of discrete electrons. There is no way to create a discrete stream which will not at times lump the electrons and at other times leave the stream a little sparse. These irregularities, the shot effect, are very small, but they can be amplified to audible proportions by the use of vacuum-tube amplifiers. This tube noise or conductor noise is an important limiting effect in the use of electrical apparatus when heavily loaded.

In 1920 very little electrical apparatus was loaded to the point at which the shot effect becomes critical. However, the later development—first of broadcasting and then of radar and television—brought the shot effect to the point where it became the immediate concern of every communications engineer. This shot effect not only was similar in its origin to the Brownian motion, for it was a result of the discreteness of the universe, but had essentially the same mathematical theory.

Thus, my work on the Brownian motion became some twenty years later a vital tool for the electrical engineer. However, for a considerable period my work was as if stillborn. When at last I came to write my earliest papers on the Brownian motion—during my summer in Strasbourg, of which I shall speak in the next chapter—I did not make any great stir in the world of mathematics.

The stir that a paper makes depends not only on its inner merits but on the interests of the other workers in the field. As far as American mathematics went, Veblen and Birkhoff were the great names in the period immediately following World War I. Veblen was interested in topology, of which I have already spoken, and believed that it was his destiny to introduce this abstract field as a new American mathematics, in contrast to what he considered the effete and dying European mathematics of analysis, of the differential and integral calculus. He contributed to the birth of a valuable mathematical subject, but his concern with the health of analysis has proved to be at least premature. At any rate, I was too committed to this field to accept the mandates of the new fashion.

Birkhoff was an analyst. As I have said, he had become the unquestioned leader of American analysis and was determined to keep this position. He had persuaded himself that true analysis meant primarily those fields of dynamics in which Poincaré had worked and to which Birkhoff himself had made major contributions. For him, everything else was to be relegated to the limbo of "special problems."

Thus there was no scope for my work in the eyes of the leaders of American mathematics until many years later, when a new generation had arisen, and when the pressing needs of industry and of the war had shown that the problems which I had solved or on which I had thrown light really merited attention.

My reception in Europe was much better than it was here. Maurice Fréchet, with whom I was to spend the summer of 1920, took a mild interest in my work, which in many ways was in the spirit of his own. His younger colleague Paul Lévy had already begun to pursue related directions of thought. Taylor in England was receptive to my ideas.

My old teacher Hardy was kind to one who had been his student and offered me great encouragement at a time when it was not easy to come by that commodity. Even so, I was regarded in Europe as well as in the United States as a young man of a certain peripheral ability rather than as one of the mainstays of the coming generation.

Be that as it may, I was fully convinced of the importance of my new ideas, all the more as they organized themselves quite rapidly into a subject with a neat little formalism of its own. The stuff felt right, and even at that time it would not have surprised me to learn that it would have a considerable future. In order to understand the ramifications of my work, I had to study much more than I had already known concerning waves and vibrations or, in mathematical terms, Fourier series, the Fourier integral, and the like. I began to make myself thoroughly familiar with those branches of mathematics which had already proved to be of physical significance. All this dovetailed into the desire of my colleagues at M.I.T. that I interest myself in applied mathematics. From this time on, my work was never random and desultory but had a definite direction in which I could naturally proceed.

Mathematics is very largely a young man's game. It is the athleticism of the intellect, making demands which can be satisfied to the full only when there is youth and strength. After one or two promising papers, many young mathematicians who have shown signs of ability sink into that very same limbo which surrounds yesterday's sports heroes.

Yet it is not bearable to contemplate a brief distinction and burgeoning of activity which is to be followed by a lifetime of boredom. If the career of a mathematician is to be anything but an anti-climactic one, he must devote this brief springtime of top creative ability to the discovery of new fields and new problems, of such richness and compelling character that he can scarcely exhaust them in his lifetime. It has been my good fortune that the problems which excited me as a youth, and which I did a considerable amount to initiate, still do not seem to have lost their power to make maximum demands on me in my sixtieth year.

Do not think for a moment that my new success made a hero of me in my own home. My father was gratified by my industry and my apparent ability to produce work which at least pleased myself; but at that time my own self-laudatory claims did not receive much of an echo from his colleagues. My father was no longer sufficiently active and interested in mathematics to judge my work on its own merits.

21 The International Mathematical Congress of 1920 at Strasbourg

In the present chapter as well as in those to come I shall have occasion to write about my various visits abroad. These visits represent an essential part of both my personal and my scientific life. They have not by any means been mere junkets, or interludes which have concerned me solely for my amusement, although they have in fact been very gratifying to me. Let me explain something of what they have meant to me, and in particular what was the significance of my 1920 trip.

In this, as well as in all other matters concerning my history, it is necessary to go back to my father. My father's formal education had been entirely European, and although he had gone to the Gymnasium and for a brief period to a medical school in Russian Poland, he was quite aware that his chief educational affiliations had been German. The high level of the German Gymnasium, or classical secondary school, had led to its dominance throughout central and eastern Europe. All educated eastern Europeans had something of a German education.

But in addition, Father had a particular connection with German education. His own father had been a journalist with the Yiddish press but was a great enthusiast for the pure German language, which he vastly preferred to Yiddish. One day he decided to change the language of his journal, which was published in Byelostok, from Yiddish to High German. Naturally this cost him almost all his subscribers, and from that time on my grandfather's career had been uniformly unsuccessful.

Owing to these circumstances, my father had been brought up with literary German as his home language. Later on, he went to Berlin for a few months of engineering training at the old Berlin Institute of Technology, at a time at which this school had not yet moved from the center of Berlin to its famous later site in Charlottenburg. Father's academic training there was brief and stormy. He ultimately left Germany (of which country he was a subject) to engage in a nebulous scheme for founding a humanitarian vegetarian colony somewhere in Central America.

The scheme was never well conceived. The other young man who was to come over with Father soon found that he had no stomach for the adventure. Father was stranded, penniless, in New Orleans, and never got to Central America at all. For some years he led a Huckleberry Finn existence in the West and in the South.

Finally he found it possible to return by a roundabout path to the career for which he was most fitted: that of linguistics. He taught for a period at the Kansas City Central High School and later at the University of Missouri, from which he came on a venture to Harvard and attracted the attention of Professor Francis Child, the editor of *Scottish Ballads*. This led to an instructorship at Harvard and finally, after many years, to the chair of Slavic Languages and Literatures.

Father was an enthusiastic scholar, with interests spreading well beyond the field of Slavic languages. He had fancied himself as a great scholar after the German pattern. This caused a considerable ambivalence in him.

On the one hand, he was essentially a German liberal of a type well known in the middle and later nineteenth century, fully in sympathy with the German intellectual tradition as it had come down from the time when Goethe had been a truer symbol of German aspirations than the Emperor Wilhelm II ever was to become. Separated as my father was from Germany, largely self-educated, and outside the orthodox German academic tradition, he still hoped for many years that by sheer intellectual strength and integrity he could win from Germany the recognition accorded to a great German scholar.

In this expectation Father had never been completely realistic. He was too innocently honest a man to be worldly-wise. It took him many years to learn how the great German intellectual tradition had come to be subservient to a group of vested interests in learning. The really great advances of the German Empire after the Franco-Prussian War had brought with them a not inconsiderable degree of the spirit of the climber and the worshiper of material success. I must admit there were many men in the German universities who were willing to accept new ideas from any source whatever. For all this, as the years went on, the entry to the inner circles of German intellectual life became more and more difficult for the outsider to achieve. This was particularly true in fields like philology and linguistics, in which a definite decision as to the merit of new work is difficult or impossible. The doors of complete recognition were closed to my father.

When Father visited Germany about the beginning of the century, and again later in 1914, he found himself far more of an outsider than he had expected or even feared. This hurt him. He came to resent and indeed to hate Germany, with the sort of hatred which one reserves for those kinsmen who, one feels, have let one down unjustly.

This hatred was reinforced by the political and social changes he had observed in the new Germany.

In addition to that, he did not like what he had seen of German militarism just before the First World War. He came to be one of the chief American supporters of the point of view of the Allies—of France and England. Together with Professor Bierwirth of the German department of Harvard, another ardent anti-militarist, he would walk down Brattle Street every morning berating Germany in the German language. The very intensity of his emotion marked a personal involvement in European matters which was entirely different from the potential isolationism of the average American and even of the average American scholar.

My father was a great enthusiast for America and for most of what was American, but at the same time an enthusiast from outside. He was deeply critical of much in America, and in particular of the shallowness of a great part of American education. The explicit intensity of his love for America and the specific character of that love were perhaps the least American things about him. He loved America, that is, as if it had been his personal discovery, rather than a background so close to him that he could take it for granted.

We had been in the habit of entertaining at our house a number of European scholars, who were largely liberal and disaffected toward the actual state of affairs in Europe. Some of them were among the great reformers of the beginning of the twentieth century. We had as house guests, for instance, Thomas Masaryk, later to become president of Czechoslovakia and the greatest elder statesman of Europe; Paul Milyukov, historian, economist, member of the Russian Duma and finally associate of Kerensky; Father Palmieri of the Propaganda, the chief Catholic authority on the Eastern Church and its various Uniat churches which had drifted into the orbit of Catholicism; and, during the World War itself, Michael Yatsevich, the Siberian engineer to whose lot it had fallen to draw up many contracts for Imperial Russia and to secure the moneys arising from them against the claims of the Communists, for some future non-Communistic and possibly democratic Russia.

Thus it was a familiar part of our life to hear foreign languages spoken in the household. My father, indeed, could speak some forty of them. He was so proficient in linguistic matters that his insistence as a teacher on accuracy and fluency had the somewhat surprising effect of almost completely inhibiting the efforts of my mother and of us children to speak more than one language.

With this background it was inevitable that I should develop a great curiosity about Europe and a profound thirst for the springs of European scholarship. These factors were supplemented by others pertaining more directly to myself and to my own interests. My years in England and, to a slighter extent, my term at Göttingen had constituted almost the first true relief I had found from the intensity of our home life and my parental pressure. My earlier research training had been primarily English and, to a lesser extent, German. The friendly recognition which I had already begun to find in Europe contrasted sharply with the sense of rejection which I had experienced around Harvard.

At the English universities, it is true, there is a certain gentleman's pretense that one is an amateur and not too deeply concerned in the hard and grinding work of scholarship. This was generally understood to be a pose. It did not require any great insight to see that those very men who conformed to the convention of the pococurante were tremendously excited about ideas and more than eager to talk about them. At Harvard, on the other hand, the pose of a lack of interest in creative scholarly work was more than a convention. The typical Harvard man considered it bad taste to talk too much and to think too much about science. The effort of trying to be a gentleman was enough of a strain on his resources.

It is thus easy to understand how the end of the war found me starved for European contacts and only too eager to return to the relative freedom of a European trip. Here the strong hand of the family could scarcely reach me. But there was still another reason why the trip tempted me: the coming International Mathematical Congress at Strasbourg.

Normally, in all fields of science, it has been the custom, say every four years, for those working together in a major subject like mathematics or physics or chemistry to foregather in some central place, read papers, and talk over the problems of their work. The first great war (alas!) interrupted these indications of the universal humanity of science, and the present alignment of the world into two hostile camps has tended to frustrate these meetings still further.

The last International Mathematical Congress before the war had taken place in England in 1912, at Cambridge. The congress which was to have taken place in 1916 was clearly impossible and was allowed to go by the board. The next one, in 1920, did not find any adequate machinery established for its organization. France decided to step into the gap and celebrate an international congress in the newly re-Gallicized city of Strasbourg and at its university, now French. This had become the second university of France and the only provincial university with a great tradition of its own.

In many ways this was an unfortunate decision. It was one which later led me to regret my little share in sanctioning the meeting by my presence. The Germans were excluded as a sort of punitive measure. In my mature, considered opinion, punitive measures are out of place in international scientific relations. Perhaps it would have been impossible to hold a truly international meeting for another couple of years, but this delay would have been preferable to what actually did take place, the nationalization of a truly international institution. All that I can say for myself is that I was young and that I did not feel myself in a position of direct personal responsibility for the course taken by international science. I was avid to seize the opportunity to revisit Europe with a certain small scientific status.

I hoped that I would be able to use the period before the opening of the Congress in September in working with some European scholar in whose field I was interested. The scholar I chose was Maurice Fréchet. It was Fréchet more than anyone else who had seen what was implied in the new mathematics of curves rather than of points, the field of which I have spoken in the last chapter. At that time we all had great expectations that his work was to mark the next great step toward the mathematics of the future.

Let me say that at the present time Fréchet's work has proved to be very important but less central than we had expected. It is written in a spirit of abstract formalism which is fundamentally hostile to any deep physical application. However, hindsight is easier than foresight, and it would have been somewhat difficult when we were at Strasbourg to predict that Fréchet was not to be the absolute leader of the mathematicians of his generation.

One of the specific things which attracted me to Fréchet was that the spirit of his work was closely akin to the work I had tried to do at Columbia on topology. My training with Russell and my later contact with the work of Whitehead had sensitized me to the use of formal logical tools in mathematics, and there was much in Fréchet's work which was suited from the very beginning to be embodied in the peculiar and highly original mathematico-logical language which Whitehead and Russell had devised for the *Principia Mathematica*.

But in order for me to describe the main events at Strasbourg in 1920 I must first write in some detail about the terms "postulationalism" and "constructionalism." This dichotomy of method is one of the central issues of modern mathematics and was of great concern to me at the Strasbourg conference.

The geometry of the Greeks went back to certain initial assumptions, known variously as axioms or postulates, which were conceived to be unbreakable rules of logical and geometrical thought. Some of these were of a predominantly formal and logical character, such as the axiom that quantities equal to the same quantity must be equal to each other. Another, with a more purely spatial content, was that known as the parallel axiom, which asserts that if we have a plane containing a line l and a point P not on that line, then through P and in that plane one and only one line can be drawn that will not intersect l. This will, of course, be the line parallel to l.

This postulate does not have the simple obviousness of the purely logical postulates of mathematics. And generations of mathematicians have sought exceptions to it. In the eighteenth century the Italian mathematician Saccheri spent a considerable amount of effort to ring the changes on the parallel postulate with the hope that any denial of this assumption would have to lead sooner or later to a logical contradiction. He did a brilliant job with the various modifications of the axiom, but his effort was unsuccessful. In fact, the more he tried to draw a contradiction out of the denial of the postulate, the larger was the body of doctrine which he was able to derive from this denial. In fact, this body of doctrine amounted to a geometry which was essentially different from the usual geometry of Euclid, but which was rather grotesque than self-contradictory.

Finally, in the beginning of the nineteenth century, a group of mathematicians including John Bolyai, in Hungary; Lobachevski, in Russia; and ultimately the great Gauss, in Germany, came to the bold conclusion that there was no contradiction in denying the parallel postulate, but merely a new and different, non-Euclidean geometry. From that time on it came to be more and more clearly understood that the so-called postulates of geometry, and indeed of other mathematical sciences, were not undeniable truths. They came to be regarded as assumptions which we could make or refuse to make concerning the particular mathematical systems which we wished to study further.

This tentative attitude in mathematics, in which the postulates became suppositions made for the sake of further work rather than fundamental principles of thought, gradually began to be the standard point of view of mathematicians in all countries. In America one of its earlier exponents, and perhaps its chief early exponent, was Edward Vermilye Huntington, of

Harvard, with whom I had studied in 1912, and who had exerted a great influence on my ways of thought.

Whitehead had been perhaps the chief English postulationalist, but he supplemented a pure postulationalism with the view that the objects of mathematics were logical constructions rather than simply the original concepts described in the postulates. For example, at times he regarded a point as the set of all convex regions which in our ordinary language might be considered to contain this point. As a matter of fact, Huntington has formulated very similar ideas quite independently, and an important essay in this direction had been made by the philosopher Josiah Royce several years earlier. But the classical example of constructionalism in mathematics is the definition of the whole numbers which occurs in the *Principia Mathematica* of Whitehead and Russell.

The distinction between the postulationalist treatment of numbers and the constructionalist treatment described by Whitehead and Russell is that in postulationalism the numbers are undefined objects which are connected by a set of assumed formal relations and specific things which can be built up out of our experience by definite modes of combination of even more elementary experience. A postulationalist treatment of numbers makes them simply objects to be arranged in a before-and-after relation so that if *a* is before *b* and *b* is before *c, a* is before *c,* and every number other than o has another number immediately before it, and so on. These are some of the postulates in such a treatment of the number system.

In the constructionalist treatment of numbers, a unit set is taken as a set of entities all of which are the same. The number 1 then becomes the set of all unit sets. A diad is a set of entities which is not a unit set but which becomes a unit set on the removal of any one of its component entities. Two is now the set of all diads. A triad is a set of entities which is neither a unit set nor a diad but which becomes a diad when any one entity is removed. Three is the set of all triads. In this way, and by the process known as mathematical induction, the complete set of all the natural numbers can be built up.

To the layman all of this will perhaps sound rather like empty logic chopping. Have we not in fact used the numbers 1, 2, and 3 in only a slightly obscured form when we built up these definitions of the earlier whole numbers? But to the logician this objection does not seem cogent for the greater precision of thought given to these definitions furnishes him with a firm ground on which to stand and from which to advance to further mathematical ideas.

The trick of building up more and more complicated mathematical objects as sets of sets and relations between relations had become familiar to me both from the work of Huntington and from that of Russell. I had indeed already written two or three essays in the application of this technique to the construction of certain elementary mathematical situations.

Postulationalism and constructionalism, as I have so far described them, do not represent movements only in mathematics. Postulationalism in particular is shared by physics. The relativity of Einstein and the new quantum mechanics both constitute regions in which physics has burst the frame of classical Euclidean geometry and assumed new definitions which are given rather as sets of explicit axioms than as a rigid and irreplaceable spatial intuition, as the older Kantian theory of space demanded.

It is true of course that the tendency to postulate for the sake of postulating and to write papers for the sake of writing papers characterizes a considerable amount of the newer mathematics. Nevertheless, the cold, hard medium of logic, like the cold hard medium of marble, imposes a certain internal discipline on all but the most vacuous and trivial mathematicians, even when they favor the newer mode of freedom.

As I have said, I was brought up in the postulationalist tradition and shared in the early years of the constructionalist tradition which arose therefrom. When I looked for a French scholar with whom to study, I looked for one whose work might embody one or both of these two directions of thought. As far as these demands were concerned, Fréchet had no rival among French mathematicians.

So far I have spoken of the new direction of thought largely from the Anglo-American point of view. In Germany, too, there had been some early exponents both of postulationalism and of constructionalism, and here the leading and most original names were those of G. Frege and Schröder. France, on the other hand, was rather late to adopt these newer habits of thought, but, as far as France had gone in postulationalism, Fréchet was the unquestioned leader. I myself had made one or two not completely unsuccessful attempts to supplement Fréchet's postulationalism as a tool for the study of new and more complicated spaces of curves by means of a constructionalist attitude. This new effort was, however, outside the inner frame of Fréchet's own work.

I wrote to Fréchet to find out if he was willing to take me on as a disciple during the summer vacation preceding the Strasbourg congress, and I received in return a cordial letter of invitation from him. His first plans had been to spend his holidays in Béarn, on the Spanish border. Later, however, he changed his mind and invited me to come and work with him, first in

Strasbourg, and then in a little country village in Lorraine which has the German name of Dagsburg and the French name of Dabo.

Early in July, I embarked for France in the French Line steamer *La Touraine* with some family acquaintances who had promised my parents to keep an eye on me during the voyage. However, the gin-soaked twenties were already upon us, and I found that my friends' ideas of what was becoming on an ocean voyage were incompatible with my rather puritanical personal habits.

I had never been a teetotaler. I enjoyed the wine we had with our meals on the boat, which I diluted plentifully with water. On the other hand, I did not like distilled liquors, and I protested vigorously against the convention under which my acquaintances pressed me to drink. I find this habit of urging drink upon a person who manifestly does not want it, quite as much an infringement of personal liberty as any blue-nosed prohibitionism can be. Thus, I was not happy on the boat and I made no friends. I was eager to land and to get away from my companions of the voyage.

But before we landed we had an interesting and not altogether pleasant experience of another sort. It had been an overcast voyage, and we were unable to shoot the sun for several days. We were going full speed ahead by dead reckoning, for these were the days in which wireless was simply a means of communication rather than an aid to navigation through an accurate system of cross bearings. We were due to make our landfall at Bishop Rock when suddenly we saw rocks looming from the fog all about us. The engines were set full speed astern, but not before we got into difficult waters.

While backing out we ripped a considerable hole in our stem. Water came in to the steerage compartment. I am told that there were the beginnings of a panic. This was controlled by the ship's officers together with a French prize-fighter passenger whose prestige was enough to keep order.

We were all sent down to our quarters to put on our life belts. It was not pleasant coming up on deck again in the midst of the milling crowd. I felt that I would like to be on deck as quickly as possible, but I also felt that any attempt to rush the matter or to get ahead of other people would not only be an act of cowardice but an act of treason to our common welfare. I forced myself to make way for others and to go up at a measured pace.

When we came on deck we still did not know what awaited us. The ship was taking in water, and the bulkheads threatened to give way. The ship's carpenter did yeoman work reinforcing them so that they did in fact hold throughout our trip across the channel to Le Havre. But still we were ordered to spend the night on deck, sleeping in our life belts by our boat stations. I remember that somebody dropped a bottle by my head as I slept.

We disembarked at Le Havre on the next morning without further incident, but the boat had been damaged far beyond our knowledge and was taken out of service for many months. There was mail awaiting me on shore. I learned that Fréchet was not quite ready to receive me yet, so after a few hours I recrossed the Channel to Southampton and went up to Cambridge.

At Cambridge I found some old friends waiting for me. I stayed with Dr. and Mrs. Bernard Muscio, a couple of Australian psychologists. I had known them in my student days at Cambridge, and they had visited Boston a few years before on an English war mission. I looked up a number of other acquaintances, including Hardy, who was about to leave Cambridge for a chair at Oxford.

In general I found that I had not been forgotten at Cambridge and that my colleagues were glad to give me that cordial reception which I had never received at Harvard. I had not been matriculated at Cambridge, as I had gone there on the basis of a special arrangement with Harvard University, permitting me to take courses without that formality. Years afterward I asked Jessie Whitehead, the daughter of Alfred North Whitehead, whether I could consider myself a Cambridge man. Under the circumstances, she said, the best that I could call myself was an illegitimate son of Alma Mater. At any rate, I have found Alma Mater more than ready to accept her by-blow into the family.

After a few days I left for Paris, where I stayed in a cheap hotel with unbelievably bad sanitary accommodations near the Louvre. I did not find my vegetarianism much in my way in France, for there was a wealth of cheap restaurants with good and appetizing vegetable dishes. I had no friends in Paris, nor was my French at that time more than barely usable. Moreover, the Paris café and street life shocked my youthful puritanism, and I was profoundly homesick and unhappy.

It seemed to me as if the house doors of a great city were a serried wall of fortifications, impregnable to the foreigner. I spent my abundant leisure walking the streets of the city and visiting the museums, particularly one tidbit to which a French-trained friend in America had alerted me: the Museum of the Ecole Centrale des Arts-et-Métiers, in which the remains of the inventions of the nineteenth century and the apparatus used for great scientific experiments were conserved in a peculiarly French sort of dusty disorder.

Fréchet had made an appointment with me, first at a *lycée* on the Boulevard Saint-Michel, where he was grading examinations, and later for lunch at an Alsatian *brasserie* on the same boulevard. He was a mustached, sinewy, athletic man of medium height. He had served in the French army as an interpreter for the British, and he was as enthusiastic for walks and tours as I was. We hit it off well together. He was, however, still not ready to receive me at once in Strasbourg, so I made a little detour into Belgium to visit some friends before I should settle down for work with Fréchet. I found my friends in their fine old house in Louvain, which had just been restored from the disorder and filth in which it had been left by the German officers who occupied it during the war, but my trip was badly timed, as it coincided with a visit of President A. L. Lowell of Harvard and his wife. Thus my entertainment was largely a matter for the children, and in particular for a young son of the family who had just completed a year at the Harvard Law School. He took me about the burned and ruined town and showed me the remains of the library, the town hall, and the nave of the church, half blocked with scaffolding. He also took me on various walks about the countryside and gave me his confidences.

Now that he was free of the Harvard atmosphere, he felt at liberty to protest against certain matters of English and American legal education. He did not like the case system and greatly preferred the a priori way in which those countries deriving their jurisprudence from Roman law search for legal principles rather than for legal precedents.

A few days later I traveled to Strasbourg by way of Luxembourg and the iron country. It was a relief for me to find myself in a region where German was spoken more freely than French, for German was a language at which I was decidedly more proficient. I settled in a boarding house in a new part of the city. Every day or every other day at the least I had a few hours' consultation with Fréchet in the garden of his little house beside the Ill–Rhine Canal.

There were two or three points in Fréchet's work which I tried to extend. Fréchet's treatment of more generalized spaces had made no use of what we know as co-ordinates: that is, he had made no attempt to represent his points by sets of numbers. In the co-ordinate representation of space, any two points at the ends of a line interval are measured by the difference between corresponding numbers at one terminus and at the other. In ordinary geometry of two or of three dimensions, this method of representing a line segment is known as its vector representation. For example, given one point in three-dimensional space, I can locate another with respect to it by saying how far I have to go north from the first point, how far east, and how far up, to get to the second point.

The theory of vectors is not new in mathematics. For well over a century and a half it has been known that an ordinary space of three dimensions contains in itself directed quantities like arrows, which can be added to one another, as for example by traversing the step indicated by one arrow and then the subsequent step indicated by another and by considering this double step as if it were a single step. It is beside the point here to go into the many operations which can be conducted with these directed steps, but it has long been known that similar geometries exist in spaces of more than three dimensions and, in fact, in spaces of an infinity of dimensions.

Fréchet's generalized theory of limits and of differentials applies to many sorts of spaces, including vector spaces, but is not necessarily confined to those spaces in which the elements may be regarded as steps. On the other hand, this geometry of steps constitutes a very important part of Fréchet's general theory and was worth solidifying with an appropriate set of postulates. Fréchet had not done this, nor did he consider these particular vector systems as peculiarly important among those which he had considered. This was the task which I had performed. It was very closely allied to the theory of the combination of successive transformations which is known as group theory, and in fact it constitutes a significant chapter of that theory.

However, I gave a full set of axioms for vector spaces. Fréchet liked it, but did not seem particularly struck with the result. But then, a few weeks later, he became quite excited when he saw an article published by Stefan Banach in a Polish mathematical journal which contained results practically identical with those I had given, neither more nor less general. Banach's conception of his ideas and his publication of them were both a few months earlier than my own. There had, however, been no chance for communication between us, and the degree of originality of the two papers was identical.

Thus the two pieces of work, Banach's and my own, came for a time to be known as the theory of Banach-Wiener spaces. For thirty-four years it has remained a popular direction of work. Although many papers have been written on it, only now is it beginning to develop its full effectiveness as a scientific method.

For a short while I kept publishing a paper or two on this topic, but I gradually left the field. At present these spaces are quite justly named after Banach alone.

There were several motives which led me to abandon this brain child, of whom I was at least one of the parents. The first was that I did not like to be hurried or to watch the literature day by day in order to be sure that neither Banach nor one of his Polish followers had published some important result before me. All mathematical work is done under sufficient pressure, and its increase by such a fortuitous competitive element is intolerable to me.

But the important reasons for my accepting or rejecting any specific piece of work have to do with the much-neglected field of mathematical aesthetics. To ask exactly what I mean by this poses a peculiarly difficult problem, for I must convey to the non-mathematician not only the substance and the texture of the mathematical work I have done but my emotional responses to it. I shall have to tell him my reasons for rejecting certain problems which have proved interesting to other people for a considerable number of years but do not seem to me to offer the same opportunities for my own vein of work and for the display of my own mathematical taste and for my particular individual powers.

This leads to a problem which must be faced in one form or other by any autobiographer who has done significant work in a difficult and private field like mathematics. A composer cannot avoid paying a certain amount of attention to the techniques of composition, and to aspects of harmony and counterpoint which are the very substance of his work but which can be appreciated only in a very limited degree even by the devoted concertgoer who has not himself faced the task of musical composition. The writer or the painter is no less involved in this problem if he goes in for autobiography. He may seem to address himself to the educated layman who can appreciate the results of his creative work. However, he has not completely performed the task of the autobiographer unless at the same time he has managed to express himself concerning those tasks of writing and of painting which can be fully appreciated only by the man who himself has faced them on a high professional level.

But the creative artist and even the creative musician are better able to attract the attention of the lay reader than the mathematician. The layman is easily convinced that, whether he is himself immersed in these creative tasks or not, a degree of information concerning the nature of their creativity belongs to his status as a cultured man. He is further aided by the fact that, even for the reader who makes no claim to the understanding of the full technical means by which a specific emotional effect is reached, there is a direct appreciation of the emotional effects of the arts, sufficient to lead the layman to a sincere effort to understand how the artist arrives in detail at beauties which the appreciative observer may see only as an accomplished whole.

The specific difficulty of the mathematician when he undertakes autobiography is that the layman does not conceive it to be any part of his aesthetic and cultural duty to understand the least thing about mathematics. For him, mathematics is likely to be a dull, dry, and formal subject. If the general public ever thinks of mathematics, it sees it at best as a tool for the physicist and the statistician and at worst as something closely akin to the work of the accountant. Hardly any non-mathematician will admit that mathematics has a cultural and aesthetic appeal, that it has anything to do with beauty or power or emotion.

I categorically deny this cold and rigid concept of mathematics. A piece of mathematics may have the virtues of logic and rigor yet, in the technical opinion of the trained observer, it may be insensitive and purely formal. To other mathematicians, the task of the mathematician is to use a rigid and demanding medium to express a new and significant vision of some aspect of the universe; to express *aperçus* which reveal something new and something exciting. If his medium is strict and confining, so are in fact the media of all creative artists. The counterpoint of the musician does not interfere with his perceptivity, nor is a poet less free because his language has a grammar or his sonnets a form. To be free to do anything whatever is to be free to do nothing.

What differentiates the appeal of the artist-mathematician from the artist-sculptor and the artist-musician is not the unemotionality of his public but the strict discipline necessary to become a connoisseur of mathematics. It is quite possible to imagine a community of musical composers whose primary satisfaction will arise from the interchange of the musical scores they have composed. They might well be relatively indifferent to the performance of these scores at concerts attended by those capable of understanding them only through the vaguer channels of receptive emotional feeling.

That the mathematician displays this aloofness from his public is indeed due not so much to an intellectual-aesthetic snobbery as to the very high degree of training at which it is necessary for the amateur to arrive in order to acquire even an appreciative relation to the content which is presented him and to the fact that, short of this technical appreciation, there seems to be no channel through which the layman can get to feel anything at all, even passively.

This limitation is not as absolute as it may seem at first sight. There is actually a considerable and growing public of trained engineers and natural scientists who, while their first interest may have originally been in the use and application of mathematics to purely utilitarian ends, have nevertheless acquired a sufficient background to appreciate a powerful theory or a clever and elegant proof. At least part of my motive in writing this book is to call the attention of the public at large to the existence of this more limited public of amateur mathematicians. I also mean to give to the reader outside this small circle at least a hint of the thrill of mathematical creation.

It is thus in an aesthetic rather than in any strictly logical sense that, in those years after Strasbourg, Banach space did not seem to have the physical and mathematical texture I wanted for a theory on which I was to stake a large part of my future reputation. Nowadays it seems to me that some aspects of the theory of Banach space are taking on a sufficiently rich texture and have been endowed with a sufficiently unobvious body of theorems to come closer to satisfying me in these respects.

At that time, however, the theory seemed to me to contain for the immediate future nothing but some decades of rather formal and thin work. By this I do not mean to reproach the work of Banach himself but rather that of the many inferior writers, hungry for easy doctors' theses, who were drawn to it. As I foresaw, it was this class of writers that was first attracted to the theory of Banach spaces.

The chief factor which led me to abandon the theory of Banach spaces, after a few desultory papers, was that my work on the Brownian motion was now coming to a head. Differential space, the space of the Brownian motion, is itself in fact a sort of vector space, very close to the Banach spaces, and it presented itself as a successful rival for my attentions because it had a physical character most gratifying to me. In addition, it was wholly mine in its purely mathematical aspects, whereas I was only a junior partner in the theory of Banach spaces.

I do not believe that Fréchet appreciated the importance of differential space when I first mentioned the theory to him. However, he got me in touch with Paul Lévy, of the Ecole Polytechnique, then the most promising young probability-theory man in France, whose work and mine have exerted a mutual influence on each other up to the present time. It cost me a little effort to persuade Lévy that my work was essentially different from that of Gâteaux, but he soon saw the point. He was to become one of my closest friends and supporters.

Curiously enough, another colleague whose work has always been in the closest relation with Lévy's and mine was the Swedish mathematician Cramer, whom I had met during my stay in England as another house guest at the Muscios' that summer.

For some time my mathematical interests made me oblivious to my personal comfort. When I came to sufficiently to think of my environment, I found that I was lonely at my boardinghouse. There was an American there who had come over with me on the same boat and who had no high opinion of me. A young English composer at the same boardinghouse was a friend of his. I wanted this composer to like me, but I had started off on the wrong foot. My fellow passenger of *La Touraine* certainly did nothing to help my standing in our little community.

The musician regarded me as heavy-handed and Philistine. This was partly because of my actual social ineptitude and bad manners, but it was also due to the fact that he considered that mathematics by its own nature stood in direct opposition to the arts. On the other hand, I maintained the thesis of this book: that mathematics is essentially one of the arts; and I ding-donged on this theme far too much for the patience of a man initially disposed to hate mathematics for its own sake. Later on we got into an explicit quarrel, in which we really said the unpleasant things we thought of one another, and this finally cleared up into a certain degree of understanding and even of a limited friendship.

The time came when I was to accompany the Fréchet family to Dabo. They stayed at the best hotel, and I naturally went to the other inn, in order not to infringe upon their privacy. I had ample opportunity for lonely walks over the countryside, climbing up the red sandstone hills of the Vosges and dipping down into the steep valleys, their streams drained for irrigation.

The landlord and landlady of my inn were very considerate of me. I did occasional jobs of wood chopping and the like for them. I felt at home in the countryside, among the crowing of the cocks and the lowing of the cattle. I enjoyed the sound of the water trickling down the village street to the place where the women washed their clothes, and the rhythmic beat of the flails on the threshing floor.

When finally we returned to Strasbourg, the International Congress was near. I had time in plenty to waste, exploring the quaint streets about the cathedral and following the circle of inner canals that surrounds the center of the city.

Three young American friends of mine came for the meeting. We put them up in my boardinghouse, two to a room. One was Forrest Murray of Harvard, vague and amiable, with whom I had often played tennis, and who had been a friend of our family for many years. Joe Walsh accompanied him. Joe, who was about my age, is still a professor in the Harvard mathematics department. He is tall and genial, and in those days his blond hair stood almost erect on his high forehead. He seemed to enjoy his visit to France thoroughly. He intended to stay in France for a year of postdoctoral study. His deep, booming voice was agreeable to hear, and it was pleasant, too, to participate in his eagerness for new experiences.

The fourth of our group was James S. Taylor, then like myself a new postwar section man at M.I.T., and now for many years professor at the University of Pittsburgh. Taylor was a kinsman of Phineas T. Barnum and was himself an enthusiastic showman of parlor tricks. The four of us have gone very different ways since, but at that time we were all united by youth, and we were tasting this youth to the full.

The congress guests began to roll in. From America there came Eisenhart from Princeton, with his beautiful young wife; Leonard Eugene Dickson, the number theorist of Chicago, famous as an enthusiast for France and the French, and a past master at bridge; and Solomon Lefschetz of Kansas, who had conquered the effects of a terrible industrial accident which he had suffered as engineer with the Westinghouse Company at Pittsburgh and had entered upon the new career in mathematics which was to take him to the leadership of the mathematics department at Princeton University and to the presidency of the American Mathematical Society.

There were several of the older people at the meeting whom we felt as links connecting us with the great past of mathematics. Sturdy old Sir George Greenhill represented Woolwich. Camille Jordan, who for all his ninety years accompanied us on our pedestrian excursions, was like a memory from the days of Louis Philippe. His recollections dated back to the great days when Cauchy was lording it over French mathematics and forcing all the younger men to pay tribute. When Jordan died, two years later, we all felt his death as a break in the continuity of the mathematical tradition.

Professor Jacques Hadamard, of Paris, played a great part at the congress. He was then only in his middle fifties, but his reputation had been well established before the end of the nineteenth century, and to us fledglings he was a great historical landmark. Small, bearded, very Jewish-looking in the *fin* French way, he occupied a unique position in the affections of his younger colleagues.

English mathematics belongs to Oxford and Cambridge, where there are ample bonds between the undergraduate and the don; while German mathematics is characterized by the amenity of the *Nachsitzung*. The official discussion of a scientific paper is followed by a procession across the university town to a beer garden, where the great and the little alike talk over the latest results in mathematics as well as the trivial pleasures of life. French mathematics, however, has followed a largely official course, and when the professor has retired to his little office and has signed the daybook which gives a record of the lecture he has just finished, it is customary for him to vanish from the lives of his students and younger colleagues.

To this withdrawn existence, Hadamard forms an exception, for he is genuinely interested in his students and has always been accessible to them. He has considered it an important part of his duty to promote their careers. Under his personal influence, the present generation of French mathematicians, for all the tradition of a barrier between the younger and the older men, has gone far to break down this barrier.

I myself benefited from Hadamard's largeness of outlook. There was no reason why Hadamard should have paid any particular attention to a young barbarian from across the Atlantic, just at the beginning of his career. That is, there was no reason except Hadamard's good nature and his desire to uncover mathematical ability wherever he could find a hint of it.

Many years later, when I was to meet him again at various mathematical congresses and as a fellow lecturer in China, I was surprised and gratified to find that he still remembered me and had an accurate idea of the entire development of my work. Thus one very positive result of the Strasbourg meeting was to bring me together with the long succession of French mathematicians who owe their recognition and their careers to Hadamard.

We congressionists made a number of interesting excursions around the sights of Strasbourg—to Saverne and the great ruined keep of Haut-Barr, to the quaint old city of Colmar, and to a sector of a battlefield of World War I. The French soldiers took us there in army trucks, but on the way back the trucks broke down and we had a long and tedious wait. Thus, when we came to the little wine-growing village of Turckheim, where the meal that had long been waiting for us had got cold and been abandoned, the mayor presented each of us with two glasses of the wine of the country, one old and one new. We had to sip these as a matter of courtesy. I will not deny that the wine was excellent, but two glasses on an empty stomach represent a severe ordeal, and new wine has a potency of its own. As we continued to Strasbourg by truck and by train, some staid souls were a bit less inhibited than was their wont.

Soon the meeting was over and the four of us went back to Paris. Taylor was to return to America with me, but my other two American friends had decided to spend a year studying in Paris. They were eager to immerse themselves in a thoroughly French environment, and they made it quite clear that we should confer on them no favor by continuing to forgather with them.

The remaining two of us found ourselves without return accommodations. *La Touraine,* on which I had come to France and on which we had intended to return, had not yet recovered from its near shipwreck, so all through a very autumnal September and part of October we waited in vain for the resumption of service, believing to the very last that we should be back for the beginning of the M.I.T. term. This however was impossible.

We began to worry what the school might be thinking of our tardiness. Nonetheless, we enjoyed ourselves in Paris and walked over large areas of the town for lack of anything else to do. We haunted the shipping agencies and finally heard of a new American boat just about to be put into transatlantic service. The passenger list was small and interesting and consisted largely of old globe-trotters. The travelers Osa Johnson and her husband were on board, together with a tame orangutan that they had brought from Indonesia. This orangutan contrasted pleasantly in its cultured behavior with a demon child who insisted on scattering the chessmen with which we were playing in the smoking room.

I came back from Europe with renewed and enlarged inspiration. For all of the defects of my French, I had lived in France and had for the first time established a contact with my French colleagues. Both in France and in England I had found that I occupied a more important position than at home, and I had work pending which seemed (at least to myself, if to few others), the promising beginning of a career in mathematics.

I had indeed seen the physical devastation of war, both in Belgium and along the former fighting line in Alsace. I had, however, become aware that the European spirit had greater possibilities of recuperation than I had previously assumed. Notwithstanding the war in the West, the defection of Russia from the European camp, and the news of battles in Poland, it was still possible to hope that World War I was merely an interlude and would be succeeded by another period of peace as long as the great peace of the nineteenth century.

As to Russia and the incipient iron curtain, the great body of Russians of the time were not merely prerevolutionary but prewar. It was not absurd to hope that a return to some sort of equilibrium with the West might still take place, in a manner similar to the return to normal life in France after the Terror and the Napoleonic Wars.

In one way or another, my taste for European contacts had grown with its own satisfaction. I was eagerly awaiting my next opportunity to see something more of the mother continent of our civilization.

22 1920–1925. Years of Consolidation

When I came back from Strasbourg, I found my work well cut out for me. The Brownian motion papers were still in what we call a heuristic stage, which is to say that the general lines of organization of the subject and of the proofs of my theorems were already clear but that much work remained to be done before they could be considered complete. I showed my results to Professor E. B. Wilson, of the Massachusetts Institute of Technology, and at his advice I sent them at once to *The Proceedings of the National Academy of Sciences*.

Professor Wilson, who is now retired from teaching but still active in scientific administration, had been a pupil of Gibbs at Yale and had taught mathematics for some years at M.I.T. There he was teaching physics in 1920, and ultimately he came to be the mathematical specialist at the Harvard School of Public Health. He has always been alert for what was new in the exact sciences, and for many years my enthusiasms have received his staunch support.

Another direction from which I received much encouragement was that of the department of electrical engineering, where Professor Dugald C. Jackson was head. I had known Professor Jackson and his son as neighbors when both of us had spent the summer of 1910 in New Hampshire. Jackson had been looking for some years for an engineer with mathematical sense, or for a mathematician with engineering sense, to resolve some of the problems impeding the theoretical electrical engineering of the time the state of which requires some explanation. Electrical engineering is divided into two more or less clearly separable fields, known in English as power engineering and communication engineering and in German by names which when translated into English become, respectively, "the technique of strong currents" and "the technique of weak currents."

Of these two fields of activity, power engineering had become fairly stabilized by 1920. Most of the types of electrical generators, motors, and transformers which now exist were already thoroughly understood by that date; and the present trend toward the fractional horsepower motor and the separately motored machine were well under way. What progress has been made in power engineering since 1920 belongs not to the tactics of the subject so much as to the strategy of large systems and electric supply. The great power nets in the United States and other countries have been increased, interconnected, and stabilized.

As to communication engineering, the situation was stabilized much later. Wireless had been an established art for twenty years, but for the most part this was wireless in the limited original sense in which Marconi had conceived it. Broadcasting was yet to come on a national scale, and the few preliminary attempts at radiotelephony were of more interest to curious young boy scientists and to hams in general than to the public. The electronic valve had indeed arrived, but there was scarcely a suspicion of the extent to which it was to modify our entire life. Television was not a new concept, for people had talked of television even before the beginning of the century, but it was just emerging from the stage in which it was conceived in terms of the selenium cell into that of practicable and rapid photoelectric apparatus.

The telephone, indeed, was triumphant everywhere, and was extending around the world the tentacles of a tight communication net. In the United States the A.T.&T., the master telephone company, was unrivaled throughout the whole scope of business in its financial magnitude and in its enlightened research policy. Thus, it was a natural time for a forward-looking electrical engineer like Jackson to devote a large part of his attention to problems in communication theory.

The logical basis of communication theory at that time was far from satisfying, and it appeared to be much less satisfying than it really was. It was of course understood that speech is carried on a telephone line by a fluctuating current whose fluctuations map those of the voice input. The great problem was to understand the full implications of the theory of fluctuating currents and voltages.

For several decades, the theory of fluctuating currents and voltages had dominated not only communication engineering but power engineering as well, in the form of the theory of the alternating current. The ordinary direct, continuous current is rather intractable. There are no simple means to step its voltage up or down, and when high-voltage direct-current lines have been used (as in France for instance), it has been necessary to run a number of generators in series against formidable problems of insulation and control.

The man who perhaps contributed more than anyone else to a solution of the problems of generating and using alternating current was Nikola Tesla. This brilliant and eccentric Yugoslav engineer worked for the Westinghouse people and converted them to the policy of generating current not in a continuous stream but as a series of surges back and forth, say at the rate of sixty per second. With the aid of the transformer, this alternating current can be stepped up and down in voltage with the greatest ease, while it can be produced in generators free from many of the other annoying problems associated with direct current. It can be used in various types of motors, including certain sorts of induction motors entirely free from sliding electrical contacts. The only connection between the fixed winding of the motor, which is fed from the outside lines, and the moving winding, which is a part of the rotating apparatus, is the electromagnetic one which also exists between the windings of a transformer. As a matter of fact, in certain forms of the induction motor there is no electrical connection between the fixed winding and the winding of the rotor, or moving part. The electrical current which magnetizes the iron of the rotor is produced by the simultaneous action of the rotor and the fixed part, or stator, as an electromagnetic transformer. Such a piece of apparatus has the great advantage of not having any moving contacts whatever and is simpler, safer, and more troubleproof.

In the early days of the alternating current, there was a battle royal between the Westinghouse people, who owned the alternating current inventions, and the General Electric and Edison people, who had invested heavily in direct-current engineering. This quarrel had as one of its consequences the fact that New York State decided to execute criminals by alternating current. This was the result of a deal put through the legislature in order to give a bad name to the supposedly more dangerous alternating current and to make people unwilling to have this introduced into their houses. Before long, however, the quarrels between the two schools of electrical engineering were patched up, for alternating current became available to the General Electric interests as well as to the Westinghouse Company.

It was in fact at General Electric that the theory of alternating current and alternating-current networks became organized and consolidated by Charles P. Steinmetz. This brilliant little man made great use of the mathematical theory of imaginary or complex numbers (which are quite as genuine and actual as real numbers) to describe alternating currents and voltages and the apparatus operating on them. The reason for introducing complex numbers into the engineering of alternating currents is that each complex number really consists of a pair of real numbers—the so-called real and imaginary parts—while an alternating current of given frequency is also determined by two real numbers, one of which gives its intensity and the other the phase or time at which it passes through zero.

For many years the theory of alternating-current engineering has been pretty complete, at least as far as concerns currents and voltages of fixed frequency, such as sixty-cycle currents and voltages. In telephone and other communication engineering we also deal with a sort of alternating current, but this alternating current is far more complicated because its frequency in oscillations per second is not fixed and because at any given time we must deal with many simultaneous sorts of oscillation. A telephone line carries at the same time frequencies of something like twenty per second and frequencies of three thousand. It is precisely this variability and multiplicity of frequency which makes the telephone line an effective vehicle of information. The line must be able to carry everything from a groan to a squeak.

Here we are concerned with one of the most ancient branches of mathematics, the theory of the vibrating string, which has its roots in the ideas of the Greek mathematician Pythagoras. He and his disciples knew very well that the vibrations of strings produce sounds and that there is a connection between the pitch of the sound generated, the length of the string, and its density and tension. To what extent the Greeks were aware that a single string can vibrate in several modes at the same time I do not know; but this fact was thoroughly familiar to the early modem scholars of the seventeenth and eighteenth centuries.

The fundamental notion in all this is what we call the sinusoid, and to explain this let us suppose that we have a drum of smoked paper turning around and let us further suppose that we have a tuning fork vibrating parallel to the axis of the drum, and that to the end of this tuning fork is attached a straw which will make a white mark on the smoked paper. As the drum revolves at constant speed, the straw will leave an extended mark which we call a sinusoid.

Let us now consider more complicated curves, made up by adding sinusoids. It is possible to add curves to one another by adding their displacements, that is to say, by combining two tuning forks of different rates of oscillation so that they both act on the same straw as it traces its path along a drum of smoked paper. In this motion we can observe two or more rates of oscillation in the same curve at the same time. The study of how to break up various sorts of curves into such sums of sinusoids is called harmonic analysis.

There is a fundamental theorem that says that if we have a curve which repeats the same form indefinitely it can be broken up into an infinite number of separate sinusoids which repeat themselves at different rates. While results of this sort were known in the eighteenth century, the name generally connected with this theorem is that of Fourier, a member of the French Academy of Sciences who accompanied Napoleon on his expedition to Egypt.

Fourier's name is also connected with other ways of adding together sinusoids in which the number of sinusoids to be added is too great to be represented by a first curve, a second curve, a third curve and so on. We may indeed have to add a mass of sinusoids which is entirely too dense to be arranged in one-two-three order.

Two parts of harmonic analysis concern themselves respectively with the analysis of periodic processes, which is given by what is known as Fourier series, and the analysis of processes which come up from zero in the course of time and which go down to zero again. In both cases the mathematician is forced to use the sophisticated methods of adding quantities to which we have already referred under the name of Lebesgue integration.

The really satisfactory theories of the Fourier series and integral were too new in 1920 to have trickled down to the working electrical engineer. Moreover, the sort of phenomenon in which the engineer is chiefly interested had almost entirely escaped the treatment of the pure mathematicians. The Fourier series, which the pure mathematicians had treated, was useful only for the study of those phenomena which repeat themselves after a fixed time. The standard form of the theory of the Fourier integral, as developed by Plancherel and others, concerns curves which are small in the remote past and are destined to become small in the remote future. In other words, the standard theory of the Fourier integral deals with phenomena which in some sense or other both begin and end, and do not keep running indefinitely at about the same scale. The sort of continuing phenomenon that we find in a noise or a beam of light had been completely neglected by the professional mathematician, and had been left to such mathematically-minded physicists as Sir Arthur Schuster of Manchester.

I came to understand that the various demands made on me by Professor Jackson concerning the proper foundation of communication theory were to be fulfilled by a further study of harmonic analysis but that this could not be done solely on the basis of harmonic analysis as it existed at the time. What the communication engineers actually did was to use a formal calculus of communication theory which had been developed some twenty years before by Oliver Heaviside. This Heaviside calculus had not as yet been given a thoroughly rigorous justification, but it had worked for Heaviside and for those of his followers who had absorbed the spirit of his theory sufficiently to use it intelligently.

For several years the chief demand made on me at M.I.T. by the electrical-engineering department was to put the Heaviside calculus on a proper logical foundation. Other people were doing the same thing at the same time in other countries, although I do not think that any of these treatments were more satisfactory than the one which I ultimately gave. In performing this task; I had to study harmonic analysis on an extremely general basis, and I found out that Heaviside's work could be translated word for word into the language of this generalized harmonic analysis.

In all this there was an interplay between what I was doing on the Heaviside theory and what I had done on the Brownian motion. Previous to my work there had been no thoroughly satisfactory example given of the sort of motion that would correspond to sound or light with a continuous spectrum—that is, with energy distributed continuously in frequency instead of being lumped in isolated spectrum lines. The harmonic analysis which had already been given corresponded more closely to what one sees when one examines the light of sodium vapor than what one sees when one examines sunlight. (The light of sodium vapor is concentrated in a number of bright lines, whereas sunlight has a continuous distribution in color and consequently in frequency.)

In the chapter entitled "My Start as a Mathematician" I pointed out my work on the mathematics and physics of the discrete, and in particular of the Brownian motion, in which we study the succession of impacts which a particle in a gas receives from the moving molecules—the shot effect, which is due to the manner in which electric currents are conveyed by a stream of individual electrons. I found that it was possible to generate continuous spectra by means of the Brownian motion or the shot effect and that if a shot-effect generator were allowed to feed into a circuit that could vibrate, the output would be of that continuous character. In other words, I already began to detect a statistical element in the theory of the continuous spectrum and, through that, in communication theory. Now, almost thirty years later, communication theory is thoroughly statistical, and this can be traced directly back to my work of that time.

My interest in harmonic analysis did not exhaust my mathematical activity. There were other problems which occupied me, some intensively and some in a more or less desultory way. The combined-research group of our department had begun to achieve a bulk of publication which made a journal of our own desirable, and we had already embarked upon this project.[1] I was the first acting editor, but the responsibility was soon taken over by Philip Franklin, who had recently come to us from Harvard, and who had been my friend and associate of Aberdeen Proving Ground days.

I used to talk occasionally with Professor O. D. Kellogg of Harvard concerning problems of possible interest on which I might do research. I did not realize at that time how carefully many professors conserve problems for their own graduate students and how sharply they regard proprietary rights in new problems. I had been used to the freer atmosphere of England and to the lavish manner in which my father had scattered the seeds of his ideas before all who would listen. My active and assertive curiosity did me no good in the opinions of those who might have benefited me by their esteem. I was not officially a student of Kellogg's. He was very helpful to me, but I consumed too much of his time, and I think that he considered me as something of a nuisance.

I learned from Kellogg that the old problem of potential distribution was attracting renewed interest. It would be of no point to state the problem here explicitly, but it is quite possible to tell the layman what sort of a problem it is. There are many physical questions which involve measurable quantities that vary over a plane or over space. The temperature in a room is such a quantity, and there are certain other similar quantities related to the flow of a liquid or the diffusion of a gas, which I can measure with a voltmeter, which gives the various electromotive forces between points in a room and the ground or between one point and another in a conductor in which an electric current is flowing.

Here I can dispense with a complete definition of electromotive force; for it is necessary to know only that electromotive force is what we measure in volts. The mathematics of all quantities varying over space and time lies in the field of partial differential equations, which is a mathematician's way of saying that we have various relations among the rates at which these quantities fall off in different directions and the rates at which they change in time. Ever since the time of Leibnitz it has been well known that there are quantities distributed both in time and in space; and that they have space rates of change as well as time rates of change. The temperature may change at so many degrees per hour; but it may also change at so many degrees per hundred miles we go north or at so many degrees per hundred miles we go east. Again, if water is flowing downhill, the steeper the hill, the faster the water flows.

Many quantities thus distributed in space and in time are of great engineering importance. It is the rate at which the local electromotive force falls off as we leave a transmission line which determines whether the line is going to conduct electricity without substantial leaks or will glow at night with a corona effect which represents many dollars out of the pocket of the electric company and its subscribers. The study of the heat-insulating powers of a house wall depends on relations among the flow of heat, the rate of fall-off of temperature, and so on.

Much of the mathematics of these quantities (which are known as potentials) is clear and direct. In that part of a room which is away from the walls, or from any other conductors, the problem of the distribution of electromotive force is relatively simple. However, when we come to the immediate neighborhood of regions of the room with very special electric properties, we get into trouble. Near these regions, which are known as boundaries, the problem of electrostatic potential reaches a new order of complication. Similar difficulties arise in the related theories of temperature and of fluid flow.

In the case of the electrostatic potential, one particular boundary phenomenon is exhibited by the sharp-pointed conductor, such as the lightning rod. Around such a sharply-pointed conductor projecting into a region in which there are electric charges, the rate at which the electromotive force drops off becomes enormous or even infinite. The electric field will not hold such rates of dropping off of potential, or potential gradients as they are called. Around such a sharp point the air is continually breaking down as an insulator, and if the field is large, a distinct corona effect will be seen in the dark. Many sailors have observed the curious effect known as the corposant, where nails and other pointed objects glow with a ghastly light in the electrified atmosphere of a thunderstorm. It is through something like this corona effect that a lightning rod relaxes the potential gradient of the charged atmosphere about it, by a gradual and unspectacular process, before the tensions build up to such a point that they may cause a disastrous stroke.

At places at which the voltage changes rapidly in space, certain media will be strained and will break down as the air does when a stroke of lightning passes or as a piece of glass when a stroke of lightning bursts through a window. The ability to stand up to these stresses is known as the dielectric strength.

So far I have been stating the problem of the pointed conductor in a physical way, depending on the specific dielectric strengths of the different media into which the conductor may be pointed. There are however closely related problems of a more formal and purely mathematical character.

We here come to one of those mathematical situations in which there is a close relation between mathematical and physical ideas but in which the correspondence between the two is not precise. All physically pointed objects are like the point of a needle that is very slightly rounded off at the end. It is possible, however, to imagine a still sharper point—such as one we might obtain by revolving about its middle line the cross section of an infinitely sharp hollow-ground razor—in which the two profiles are tangent to each other. Impossible as it is to realize such a figure physically, its conception offers no mathematical difficulty whatsoever. It is even possible to consider an electrical potential distributed about such a re-entrant spike and to ask how such a potential would behave near the very point of the spike.

It will be found that there are cases where the mathematical behavior of potential around this ideal spike will be strongly suggested by the actual behavior of potential about a very sharp physical spike. In the physical case the strains become so great that the matter in the field breaks down. In the mathematical case there need be no matter in the field to break down, but the field itself may become discontinuous. If this is so, the potential at the point of the spike becomes indeterminate and assumes one value if we reach the point of the spike by one path and another if we reach it by another path. It was this phenomenon which I started to investigate at Kellogg's suggestion. My object was to determine for what spikes this discontinuity could occur.

A Polish mathematician by the name of Zaremba had obtained here certain results which gave some hypotheses of sharpness that were enough to ensure the indeterminacy of the potential, and some other hypotheses of bluntness which were enough to ensure that the potential would not become indeterminate; but these conditions left between them a gap in which our knowledge was incomplete. In this intermediate field Professor Kellogg had done vitally important work, and two of his young friends were writing doctoral dissertations on the subject at Princeton. When Kellogg informed me of the work that was being done in this corner of potential theory, I immediately began to reflect how I myself should attack the problem.

I very soon found that I could make quick headway in this subject and that in fact, within a matter of days, I had got even further than the two Princeton doctoral candidates. When I showed Professor Kellogg my mathematical results, there was a sudden change in his attitude. At the beginning I had found him rather pleased with my interest in potential theory. Now, however, he was chiefly concerned with the effect that my pursuit of the subject might have on the acceptance of the doctoral dissertations of the other two young men.

There is an understanding at many schools which was at that time a general convention—that the publication of a doctoral dissertation is an indispensable condition of its acceptance. It is clearly more difficult to publish a paper which has been anticipated than to publish one when the results are known to be completely new. This I felt to be unjust, and I considered that the only sane criterion of originality from the standpoint of a doctor's degree should be whether the paper was new as compared with the literature reasonably available at the time of its submission. Here, by "reasonably available," I mean reasonably available in view of the actual opportunities of the author.

I am afraid I did not accept with any grace Professor Kellogg's dictum that I was to erase from my mind the work I had already done on the potential problem and to clear the tracks for the two doctoral candidates. I had been aware through Dr. Kellogg's leak, and only through Dr. Kellogg's leak, that other people were working on the problem, but I had no information as to their methods and tools, and my result was genuinely original.

Furthermore, I did not accept with alacrity Kellogg's suggestion that I was now an established mathematician, who did not need these papers and who ought to give them up as a charity to youth and inexperience. Both of the candidates were older than myself, and both of them enjoyed the secure position of being the pupils of men with influence in American mathematics. I had never received favors from those in power, and it was only when it was to my disadvantage that any Harvard man thought of treating me as an established mathematician.

If I had had my full, undivided attention to devote to my scientific work and my scientific position, the situation would have been difficult enough. However, the scientist is at the same time a human being, and his human needs will not wait indefinitely on his scientific career. I was by now in my late twenties and I had begun to look forward to the fuller life of a married man. I was already beginning to pay attention to the young lady who is now my wife.

This young lady who so interested and attracted me was Margaret Engemann. She came from a family in Germany that was close to the land, and which had been gradually emerging from the status of small farmers to that of renters and stewards of great estates, to the clergy, and to professional life in general. Her mother had come over to America after the death of her husband and had lived an active, romantic, outdoor life in the new West. It was the directness, genuineness, and sincerity of the mother that I saw reflected in the daughter, and which gave to me first the promise and then the certainty that this was the girl for me.

I had visited her and her family at about the time of my run-in with Kellogg on a raw and bleak December day. In waiting for the streetcar home, I had got thoroughly soaked and had developed the beginnings of a bad cold. When I talked over the problem of the publication of my paper with Kellogg when we met at a local meeting of the American Mathematical Society, I was already sick and half-delirious with what later turned out to be pneumonia. Instead of acquiescing in his point of view, I felt that it was an unfair assertion of solidarity among the insiders against the outsider and asserted my intention of securing quick publication for my results in our new mathematical journal. This started a storm of antagonism against me, and both Kellogg and Birkhoff thundered at me from an exalted moral elevation.

I felt thoroughly sick and discredited. The next day I commenced a chilly weekend of winter sports at the Groton farm which my parents had bought for their ultimate retirement. I returned home and went immediately to bed, where I found that I was down with a first-rate case of bronchopneumonia. All through the pneumonia, my delirium assumed the form of a peculiar mixture of depression and worry concerning my row with the Harvard mathematicians and of an anxiety about the logical status of my mathematical work. It was impossible for me to distinguish among my pain and difficulty in breathing, the flapping of the window curtain, and certain as yet unresolved parts of the potential problem on which I was working.

I cannot say merely that the pain revealed itself as a mathematical tension, or that the mathematical tension symbolized itself as a pain: for the two were united too closely to make such a separation significant. However, when I reflected on this matter later, I became aware of the possibility that almost any experience may act as a temporary symbol for a mathematical situation which has not yet been organized and cleared up. I also came to see more definitely than I had before that one of the chief motives driving me to mathematics was the discomfort or even the pain of an unresolved mathematical discord. I became more and more conscious of the need to reduce such a discord to semipermanent and recognizable terms before I could release it and pass on to something else.

Indeed, if there is any one quality which marks the competent mathematician more than any other, I think it is the power to operate with temporary emotional symbols and to organize out of them a semipermanent, recallable language. If one is not able to do this, one is likely to find that his ideas evaporate from the sheer difficulty of preserving them in an as yet unformulated shape.

It was at the period of my illness that I became really aware of how much I needed the young lady who was later to become my wife. I will not say that from this time on my courtship followed a direct path, or that I became immediately sure that I wanted marriage; but at any rate I had embarked on the series of emotional changes and ups and downs which ultimately lead to our marriage. I have told the story of this in my earlier book, *Ex-Prodigy.* I shall return to it here only insofar as it is directly relevant to the incidents of my career as a scientist.

It is as a scientist that I am exhibiting myself to the public in the present book, and I shall deliberately play down even those emotional events of my own life and of our later life together which have been most important to us. I wish no reader to draw the conclusion that my emotional life has been restricted to my scientific career, or that I could live with any satisfaction without the loyalty, affection, and continued support of my lifelong companion. What is inhibiting me here is the intensely personal quality of the enduring love and companionship between man and wife. I cannot convey this to my readers without what I feel to be an infringement of what belongs to us two alone and of what, because of its very reality, depth, and lasting endurance, is too free from casual vicissitudes to contain much that we care to impart to anyone but ourselves. My science belongs to the world, my home life and affections to no one but to those dear to me, and to myself.

Let me return to the general question of competitiveness among mathematicians and of the ethics which governs it. I have already said that I was more or less repelled by the high pressure of work which was bound to be competitive from the start, yet I knew very well that I was competitive beyond the run of younger mathematicians, and I knew equally that this was not a very pretty attitude. However, it was not an attitude which I was free to assume or to reject. I was quite aware that I was an out among ins and that I would get no shred of recognition that I did not force. If I was not to be welcomed, well then, let me be too dangerous to be ignored.

I was not alone in my competitiveness. At least one of the greatest American mathematicians, a man whose disapproval was the highest hurdle I should have to leap, was even more intensely competitive than myself. I have always taken in ideas easily and given them out freely, and, although emulation has been a part of my nature, I have not sought to work in the profoundest secrecy and to spring my new results on a world which has not even known that I have been working on them. In this I offer a strong contrast to certain older colleagues, who may have shown less naïve joy in the immediate results of their scientific conquests, and I have never tried to steer other investigators away from my own work so that I could be the beneficiary of the surprise effect of a new paper carefully guarded until I could present it with a maximum impact. I have not been more competitive than many of those about me, but I have been less meticulous in presenting the appearance of lacking competitiveness and I have not been careful to mend my fences.

Note

1. I was lucky to have a journal at my disposal in which I could secure quick publication.

23 The Period of My Travels Abroad—Max Born and Quantum Theory

It was only two years after my trip to Strasbourg that I began to resume my visits to Europe. Part of my eagerness was due to my desire to drink deep at the fountain of European mathematics, of which I had already tasted, but another part came from family considerations.

Soon after the end of the war my parents had bought a farm house and apple orchard in the town of Groton, Massachusetts, with the hope that they might live there after my father's retirement. This farm was indeed at the disposal of the entire family for our recreation; but it would have taken the full efforts of the family to run it, and even in our vacations we were no longer a group of unoccupied youngsters. We needed our free time to recuperate from the very hard work of making our way in our professional careers. It was scarcely reasonable to expect that we should turn our well-earned holidays over to the job of raising vegetables and clearing our wood lot.

My sister Constance was teaching mathematics at Smith during much of the time, and my sister Bertha was studying chemistry at the Massachusetts Institute of Technology. Constance, a small, vigorous, self-confident young woman, was the mainstay of my parents and was supposed to be the one of us with the most *savoir faire*. In those matters in which my point of view came to differ more and more from that of my mother, Constance leaned heavily to my mother's side.

Bertha, seven years younger than I, was perhaps the most independent member of the family. She had been too young to undergo the full educational pressure to which I had been subject or even the more diluted form of it which Constance had experienced. During her secondary-school career, moreover, the education of my younger brother Fritz had been the chief family problem; and Bertha more than the other two of us had been left to her own devices. Thus Bertha has always been able to see the family problems from a somewhat more objective point of view than either Constance or I myself, in my early days at any rate.

I was eager to pass on to my sisters some of the pleasures of my European travels, and they were only too pleased to share these with me. I will not go into the chronology of the successive voyages which I made alone and with my sisters, but the summers of 1922, 1924, and 1925 found me abroad again, looking up friends of the family and various colleagues. I saw more and more of Lévy on these trips and I developed important new contacts not only in England and France but in Germany as well. I witnessed the great inflation in Germany during the summer of 1922 and found it a really terrifying experience.

My work on potential theory continued in two directions. First, I arrived at a new concept of the relations between the electromagnetic potential in the interior of a region and that on the boundaries. As I have already suggested, the earlier concept of the electromagnetic potential in the interior of a region was that it should completely fit the values given on the boundaries. However, I followed out notions akin to those of generalized integration, which I had already studied, and noticed that the interior potential of a region could be regarded as determined by an additive combination of potentials around the boundaries, even when this definition might fail to yield a continuous function as we approach the boundary point. This was a radically new idea, and it led me to significant extensions of many of the notions of potential theory, including those of charge and capacity. In this work I was motivated by concepts which belonged to the generalized theory of integration of P. J. Daniell, of which I have already written. The novelty was that I conceived the relation of the potential of an interior point to the boundary values as a sort of generalized integration rather than as a limiting process by which the internal potentials should be united with those at the boundary point. This was something of an inversion of the existing point of view on boundary problems. Like so many inversions of points of view in mathematics, the reformulation of the problems of potential theory let a breath of fresh air into a situation which had been deadened for years by too conventional a statement.

My old friend and mentor, Professor H. B. Phillips of M.I.T., had already studied quantities analogous to potential on square nets like screens and on cubical structures representing generalizations of these square nets. With the aid of my new generalized concepts in potential theory, I was able to make Professor Phillips's work an important step toward a universal theory of potential.

In this manner I added a considerable number of new and sharply definable concepts to the armament of potential theory. When I applied these to the old problem of Zaremba, which still had not reached a final solution, I found that they fitted. This was at about the time at which the *Comptes Rendus* of the French Academy of Sciences began to be filled with papers on the theme of the Zaremba theory. These were written by Lebesgue himself and by a young pupil of his, G. Bouligand.

In many scientific subjects there comes a time when the sharpness and definiteness of the new papers indicate that an important goal is about to be achieved. So it was with the work of Lebesgue and Bouligand. I knew that if I did not put forth a maximum effort, the whole topic was soon to be crossed off the account books of mathematics as one finally resolved. Accordingly, I put forth a maximum effort, employing those new tools which I had made my own, and I was delighted to find that I had achieved what was from the standpoint of research at that time a complete solution of the problem.

I was aware that I must hurry. I went to a Mexican student of mine, Manuel Sandoval Vallarta, who was much more proficient in French than I was, and got him to help me to rewrite my ideas in acceptable French. I mailed a brief note to Lebesgue for publication in the Comptes Rendus and awaited the outcome.

What followed is a coincidence of a sort much commoner in the history of discovery and invention than one might suppose. While my letter was crossing the ocean, Bouligand obtained some extremely important results which he had not yet had time to polish up. He took council with Lebesgue, who advised him to submit these results to the academy in a sealed envelope, after a custom sanctioned by centuries of academy tradition. The very next day, my paper came in and Bouligand's sealed envelope was opened. The two papers appeared side by side in the next number of the Comptes Rendus, with a preface by Lebesgue covering both of them. While they were expressed in different mathematical language, the main idea of the two was identical. However, the logic of Bouligand's paper was not as complete as mine, owing to the fact that what he had sent in was only a preliminary communication for purposes of record, and not a polished and finished job.

This was even more of a dead heat than my previous double discovery of Banach space. Both the Banach space competition and that with Bouligand turned out to be extremely friendly. Bouligand was even more ready than I was to admit the somewhat greater completeness of my paper, and we made arrangements to meet when I should come to France on a visit.

A piece of work which started out under less competitive pressure but which also ended in a friendly way was stimulated by the research of the Danish mathematician Harald Bohr on what he called “almost periodic functions.” These represent curves which do not quite repeat themselves after the fashion of a pattern on wall paper but come close to doing so. Their discovery represented an important extension of harmonic analysis. I myself, as I have said, was working on extensions of harmonic analysis to which I had been led through an attempt to justify Heaviside’s formal calculus. Again there was nothing for me to do but put on steam and see if I could consolidate my ideas in the field. I did succeed in doing so, and I obtained an adequate theory which covered not only those spectra of light which can be represented as sharp lines, such as we find in parts of the spectra of the elements, but also those where the power is distributed continuously. Bohr’s theory was valid only for line spectra. With the aid of one or two tricks, from my own theory I was able to deduce not only the Bohr theory but a much wider range of results concerning continuous spectra as well.

In this I had to use ideas very closely akin to those which I had used in the study of the Brownian motion. In particular I had to make use of curves which are continuous but which are so crinkly that they can not properly be said to have a direction. I have already pointed out in my discussion of the Brownian motion that these curves had been more or less the stepchildren of mathematics and had been regarded as rather unnatural museum pieces, derived by the mathematician from abstract considerations, and with no true representation in physics. Here I found myself establishing an essentially physical theory in which such curves played an indispensable role.

There was another contact with European mathematics which I made at this time in quite an unanticipated way, though in a personal rather than a scientific matter. I had seen a good deal of the work of Leon Lichtenstein, a German mathematician who had been studying the theory of fluid flow, and who was the editor of what was then the chief international review journal for mathematicians. Father had known of a cousin Leon who had gone to the same institute of technology at Berlin at which he himself had studied, and who had found the atmosphere of industrial research not too agreeable. Father was also aware that Leon had left industry for academic work in applied mathematics but was unaware either of his degree of success or where he was now working.

We received a letter from an aunt of mine in New York, telling us that cousin Leon had turned out to be rather more successful in mathematics than we had anticipated. The letter gave us his full name, which was Leon Lichtenstein. I put two and two together and thought it highly likely that cousin Leon and the famous mathematician were the same person. I wrote to Lichtenstein, asking him directly if he was our cousin, and I received a very friendly reply. He was indeed the Lichtenstein of whom Aunt Charlotte had written, and he was quite aware of my existence and my work. He invited me to look him up the next time I should find myself in Europe. He still lived in Berlin, but his teaching was at the University of Leipzig. Here, as I afterwards learned, he was dean of the Faculty of Sciences.

The summer after I had made my contacts with Bouligand and Lichtenstein I was in Europe with my sister Bertha. I ran down to Poitiers to see Bouligand. He was waiting for me at the station with a copy of one of my articles held up so that I could identify him. He proved to be an unpretentious young Breton, and he took me into his family for the visit. There was much to see in Poitiers, which is a charming, romantic town with most interesting architectural monuments. Bouligand introduced me to a friend of his who was professor at the *lyceé* and an authority on local antiquities, and between them they showed me all the tidbits of interest.

I also went to Germany that summer to see cousin Leon. Like Bouligand, he had to identify himself to me, and I had to identify myself to him, because we never had even seen pictures of each other. He met me at the station, holding up a sheet of paper on which he had written, in my honor, the chief formula in potential theory.

Leon Lichtenstein was bald and bearded and did not have much facial resemblance to my father, but like him he was a short, vigorous man, with brisk motions and strong opinions. He was very anti-American in many of his attitudes, although cordial to me personally. Bertha and Mrs. Lichtenstein, who was the balance wheel of the Lichtenstein family, had considerable difficulty in keeping the arguments between Leon and me from degenerating into quarrels.

Here I ran into one of the minor problems of life in Germany. At our first meeting, Leon had asked me to use *du,* the familiar form of German address, with him. Mrs. Lichtenstein, on the other hand, had made no such suggestion to me, although she was quite as cordial as her husband. Under these circumstances I did not feel that I was entitled to the same degree of familiarity with her as with my cousin, and I stuck to *Sie,* the formal mode of address.

I visited Göttingen in 1924 for old times' sake and found that my new ideas had begun to interest the Göttingers. In 1925, after a little mountain climbing with Alexander, of Princeton, I again visited Göttingen, where my work on generalized harmonic analysis was beginning to attract real attention.

The new head of all Göttingen mathematics was Richard Courant, an industrious, active little man, who was eager to keep all the strings of mathematical administration in his hands. Courant suggested to me that I might find some American sources of funds for a year's study in Göttingen, and for research under continued contact with my Göttingen colleagues. The John Simon Guggenheim Foundation had just been established in New York, and Courant pointed out to me that that was an appropriate benefactor to which I might turn for funds. He promised me the full co-operation of my Göttingen colleagues in making my trip agreeable and in providing me with an assistant to help organize my papers and to take care of my lapses in German.

He sent me over to pay my respects to Felix Klein, who shared with Hilbert the prestige of being one of the two leading Göttingen mathematicians. Klein was in poor health and, as a matter of fact, had only a few months of life ahead of him. I welcomed this opportunity to establish yet another connection with the august past of mathematics.

I began my visit to Klein by a social blunder. When the elderly housekeeper appeared at the door, I asked in my best German, *"Ist der Herr Professor zu Hause?"* She replied in a tone of rebuke, *"Der Herr* Geheimrat *ist zu Hause,"* thus reproaching me for addressing a privy councilor by the lower title of professor. In German science, I may say, the social position of a Geheimrat was like that of a scientist in England who had been knighted; but I have never seen so much fuss made about a knighthood in England, as was made in Germany about the title of Geheimrat.

I went upstairs and found Felix Klein in his great study, a pleasant, high, airy room lined with bookcases and with a large table in the middle covered with an orderly disorder of books and open periodicals. The great man sat in an arm chair behind the table, with a rug about his knees. He was bearded, had a fine, chiseled face, and carried about him an aura of the wisdom of the ages. He spoke with a noble condescension, as if he were a king; and as he spoke the great names of the past ceased to be the mere shadowy authors of papers and became real human beings. There was a timelessness about him which became a man to whom time no longer had a meaning. After I had listened respectfully for a few minutes, I found that I was given my *congé* as I might have been given it at a court.

The talk which I gave the Göttingen people on my work on general harmonic analysis was very well received. Hilbert, in particular, showed great interest in the subject, but what I did not realize at that time was that my talk was closely keyed to the new ideas of physics which were about to burst into bloom at Göttingen in the form of what is now known as quantum mechanics.[1]

Quantum mechanics was a subject in mathematical physics which had originated in 1900 in the work of Max Planck on the equilibrium of radiation in a cavity. In plain language, the subject matter of quantum theory is the study of such light as we find inside of a hot furnace after light and hot matter have come to equilibrium so that if we look into a cavity with heated walls, such as a blast furnace, the light coming from inside the furnace changes in character as the temperature changes. This is a readily observable effect which we all know from the difference between a red-hot piece of metal and a white-hot piece of metal. The spectrum of the light coming from the red-hot furnace ceases somewhere in the red or yellow, but the light coming from the white-hot furnace may go far into the ultraviolet.

The nub of the difficulty in explaining this relation between light and heat, which Planck solved by a brutal new hypothesis, was that the traditional representations of light as a continuous phenomenon was not satisfactory. In light as in matter, he argued, there is a granular rather than continuous texture.

The earlier physics had not been able to conceive any mechanism by which the color distribution of light in a furnace could be determined by the furnace's temperature. Planck's eventual explanation of this easily observable phenomenon was, however, not simple. It was associated with ideas concerning mathematics and thought in general which go back to the end of the seventeenth century, during a period when an important intellectual battle was being fought between the atomists, who believed in the discreteness of matter, and those who believed that matter is continuous. There were various philosophical considerations which made this debate especially critical.

It was not, however, so much the general philosophical climate of the time as a technical innovation which brought the dispute to a head. This innovation was the discovery of the microscope by the Dutchman Leeuwenhoek, who had perfected his device to the point at which he could see something of the teeming life in a drop of stagnant water.

The discovery of a new instrument often leads immediately to a new insight. Before Leeuwenhoek, the study of living organisms had been limited to what could be seen by the naked eye or, at best, with a primitive hand lens. Thus, scientists, while they might have had the Democritean idea that the world exists of extremely small particles or atoms, had made no considerable progress in seeing phenomena smaller than, say, a grain of sand.

Leeuwenhoek's microscope showed by direct observation that a drop of pond water was a teeming mass of life suggestive of a crowded city. The new power lent to the eye engendered a new range of imagination, and everyone's thoughts turned to the fine structure of the world and to the philosophical implications suggested by the process of magnification. One result of this experience, perhaps, was Swift's famous jingle:

So, naturalists observe, a flea
Hath smaller fleas that on him prey;
And these have smaller still to bite 'em;
And so proceed *ad infinitum.*

The background of this little jingle is more interesting than the jingle itself may seem to us at this late date, for among the objects that Leeuwenhoek studied with his new microscope were the spermatozoa of man and the animals, which Leeuwenhoek quite reasonably interpreted as playing a part in conception. Through the imperfect microscopes of Leeuwenhoek and his followers, however, it was easy to imagine that the spermatozoon contained a small, rolled-up fetus. This theory gave a plausible interpretation to the act of conception, for it was believed conception consisted merely in the implantation of the spermatozoon in the womb, in which environment it could grow in size till it became an embryo of the sort which was already familiar to the doctors. The idea that the spermatozoon was the sole antecedent of the embryo led to some very interesting biological speculations.

If the spermatozoon was itself an early stage of the fetus, it was natural to think that it was a human being in miniature, with all the organs of the human being on a smaller scale, distorted but still essentially there. By this token, it should contain smaller spermatozoa, much as Swift's flea carried lesser fleas on a scale far smaller than the microscope of the day could show. These in turn could be thought to contain still smaller spermatozoa, and so on ad infinitum, so that the whole future of the human race actually lies preformed within the bodies of those now existing. This preformationism argued for an infinite subdivisibility of matter, and the philosophical consequences of this were eagerly studied, particularly by the great philosopher, Leibniz.[2]

Leibniz conceived the world after the analogy of the drop of water and the similarly teeming drop of blood as a plenum. That is, he conceived that all the apparent spaces between living beings and within living beings are themselves filled with living beings on a smaller scale. This theory led Leibniz to postulate the infinite subdivisibility of life and, consequently, the continuity of matter.

This opinion, which was generated as we have seen by the microscopic observations of his day as well as by the inner workings of his own philosophy, led Leibniz eventually to a new interpretation of mathematics. He was, we must remember, one of the co-inventors of the calculus, and he originated the notation which we use even now. For him not only are time and space infinitely subdivisible, but quantities distributed in time and space may have rates of change in all their dimensions. For example, one quantity distributed in time and space is temperature. When I say that the thermometer is dropping at the rate of ten degrees an hour, I am speaking of its time rate of change. When I say that it is dropping at the rate of three degrees per mile as I go west, I am giving one of its space rates of change. In discussing quantities which have a distribution both in time and in space, a natural mathematical law is the partial differential equation in which time rates of change and space rates of change are related to one another in a system where time and space are both infinitely subdivisible. Thus, Leibniz, in arguing for the continuity of the physical world, became the spokesman for a view in direct contradiction to atomism.

The development of physics since his time has brought both atomism and the continuistic theory to a perfection and to a sharpness of opposition which they did not possess in his day. The molecule has been all but seen, and the chemical evidence for the existence of the discrete atom is clear. Beyond the atom, new vistas of atomicity have been discovered in the electron, the proton, and the many new fundamental particles discovered in the atomic nucleus; while in the meantime the continuum theory has become a useful and almost indispensable tool for the study of the dynamics of gases, liquids, and solids and for the theory of light and electricity. That these two great directions of thought have come into head-on collision with one another has led to some of the chief problems of modern physics.

This collision began to take shape about a hundred years ago, when Clerk Maxwell developed what is now known as the kinetic theory of gases. A gas consists of particles called molecules which can move in several independent ways. A molecule can move up and down, to the right and to the left, and to and fro as a whole, besides which it can rotate about a vertical axis and two horizontal axes. All these motions belong to it as a rigid body, but it is often more than a rigid body and may have internal vibrations which appertain to it as an elastic system. We can count the number of modes of motion, or, as a physicist calls them, degrees of freedom, of a single particle. By adding up the number of the modes of motion of the different particles forming a gas, we can determine the number of modes of motion or degrees of freedom of the gas as a whole. Maxwell remarked that when a gas has settled down to an internal statistical equilibrium, each mode of motion will have on the average a certain energy and this average energy will be the same for all modes of motion. This is a most important theorem in connecting temperature with the other properties of a gas.

It results at once that the ability of a given volume of a gas to absorb energy depends on the number of degrees of freedom per unit volume. The measure of this ability is called the specific heat. It enables us to ascertain how much energy a body in heat equilibrium will contain at a given temperature. If the number of degrees of freedom per unit volume is infinite, the body will be able to take up an infinite amount of energy with a finite increase in temperature; or what is the same, a finite accretion to its energy will not make it any hotter. If we apply a similar argument to a continuous medium, which will naturally have an infinite number of degrees of freedom per unit volume, then this too will have infinite specific heat, and the notion of temperature will not be applicable to it.

Now, Clerk Maxwell was not only the originator of this theory we have just indicated, which is known as the kinetic theory of gases, but also of the theory that light and electricity are transmitted as oscillations of a continuous medium known as the luminiferous ether. This means that the ether can be heated indefinitely without getting any hotter. Since the motions of the luminiferous ether are known as radiation, taking the form of light, X rays, radiant heat, etc., the Maxwell theory of the ether is inconsistent with the existence of any temperature to radiation. Maxwell's theory of light, satisfactory as it is for free radiation in the absence of matter, makes it impossible for light to come to equilibrium with matter in temperature as it is actually known to do in a furnace. Something more and different from the Maxwell theory was needed for the study of the radiation of light, and this something more was suggested by Planck.

Planck observed not only that there is a temperature to radiation, but that the relation between this temperature and the character of the radiation follows a definite law, which is known by Planck's name. In order to justify this law, he supposed that radiation was emitted according to certain small atomic quantities which he called quanta, and this work of his is the first form of the quantum theory of modern physics.

In general, 1900 represents a critical period in scientific thinking. It had not been many years since the most advanced scientists considered that future centuries would be devoted to determining already existing physical theory to further and further decimal places of accuracy. About 1900, however, the quantum theory was beginning to destroy some of the ideas of continuity in the field of radiation. The Gibbsian statistical mechanics was already well on the way to replacing determinism by a qualified indeterminism, and the optical experiment of Michelson and Morley, which showed the impossibility of measuring the velocity of the earth through the ether, had recently become an essential part of the chain of ideas which was to lead to Einstein's relativity.

Einstein's theory of relativity was formulated in 1905, and in the same year he made a critical contribution to quantum theory. He showed that certain of the constants involved in the photoelectric effect, which connects light absorption or emission and electricity, were numerically and dimensionally the same as a famous constant used by Planck in quantum theory. Seven years later, in 1912, Niels Bohr, of Copenhagen, discovered the same constant in the theory of the radiation of the hydrogen atom.

The theory of radiation which was put forward by Bohr was brilliantly although not perfectly successful. It was a curious hybrid in which features of a discontinuous theory were somewhat unnaturally grafted on to a continuous theory like that of planetary orbits. This quantized mechanics had important numerical successes and rather incomplete theoretical unity. By 1925, the year of my talk in Göttingen, the world was clamoring for a theory of quantum effects which would be a unified whole and not a patchwork.

Without being aware of the way in which interest in Göttingen was already concentrating about the difficulties of the quantum theory, my talk in Göttingen, like quantum theory, dealt with a field in which the laws of ordinary magnitudes do not continue down into the range of the very small. As I have said, my talk concerned harmonic analysis—in other words, the breaking up of complicated motions into sums of simple oscillations. Harmonic analysis, for all its many modern ramifications, has a history going back to Pythagoras and his interest in music and the vibrations of the strings of the lyre. There are many ways in which a string can vibrate, but the most elementary and simplest of all is known as the simple harmonic oscillation. The motion of the string of a musical instrument, if indeed it is not simply harmonic, is well known to be the most elementary sort of combination of simple harmonic motions. In fact, for a first very crude approximation, we can treat such a motion as simply harmonic.

Now, let us see what musical notation really is. The position of a note vertically on the staff gives its pitch or frequency, while the horizontal notation of music divides this pitch in accordance with the time. The time notation contains the indication of the rate of the metronome, the subdivision of sound into whole notes, half notes, quarter notes, etc., the various rests, and much else besides. Thus musical notation at first sight seems to deal with a system in which vibrations can be characterized in two independent ways, namely, according to frequency, and according to duration in time.

A finer assumption of the nature of musical notation was that things are not as simple as all this. The number of oscillations per second involved in a note, while it is a statement concerning frequency, is also a statement concerning something distributed in time. In fact, the frequency of a note and its timing interact in a very complicated manner.

Ideally, a simple harmonic motion is something that extends unaltered in time from the remote past to the remote future. In a certain sense it exists *sub specie aeternitatis*. To start and to stop a note involves an alteration of its frequency composition which may be small, but which is very real. A note lasting only over a finite time is to be analyzed as a band of simple harmonic motions, no one of which can be taken as the only simple harmonic motion present. Precision in time means a certain vagueness in pitch, just as precision in pitch involves an indifference to time.

The considerations are not only theoretically important but correspond to a real limitation of what the musician can do. You can't play a jig on the lowest register of the organ. If you take a note oscillating at a rate of sixteen times a second, and continue it only for one twentieth of a second, what you will get is essentially a single push of air without any marked or even noticeable periodic character. It will not sound to the ear like a note but rather like a blow on the eardrum. Actually, the complicated mechanism of the reflection of impulses which is necessary to make an organ pipe speak in a musical manner will not have a fair chance to get started. A fast jig on the lowest register of the organ is, in fact, not so much bad music as no music at all.

It was this paradox of harmonic analysis which formed an important element of my talk at Göttingen in 1925. At that time, I had clearly in mind the possibility that the laws of physics are like musical notation, things that are real and important provided that we do not take them too seriously and push the time scale down beyond a certain level. In other words, I meant to emphasize that, just as in quantum theory, there is in music a difference of behavior between those things belonging to very small intervals of time (or space) and what we accept on the normal scale of every day, and that the infinite divisibility of the universe is a concept which modern physics cannot any longer accept without serious qualification.

To see the relevance of my ideas to the actual development of quantum theory, we must step ahead a few years, to the time when Werner Heisenberg formulated his principle of duality or indeterminism. The classical physics of Newton is one in which a particle may have at the same time a position and a momentum—or, what is not very different, a position and a velocity. Heisenberg eventually observed that under the conditions under which a position can be measured with high precision, a momentum or velocity can be measured only with low precision, and vice versa. This duality is of exactly the same nature as the duality between pitch and time in music, and in fact Heisenberg came to explain it through the same harmonic analysis which I had already presented to the Göttingers at least five years before.

The two main figures of the early days of quantum mechanics in Göttingen were Max Born and Heisenberg. Max Born was much the older of the two; and, although it is unquestionably his line of thought which led to the origination of the new quantum mechanics, the actual initiation of the theory as a separate entity belongs to his younger colleague. Born has always been a calm, gentle, musical soul, whose chief enthusiasm in life has been to play two-piano music with his wife. He has been the most modest of scholars, and it was only in 1954, after grooming other scholars for work which led them to the Nobel Prize, that he himself was awarded it.

Heisenberg, who was at that time in his early twenties, had a less retiring personality than Born and was able to taste success at the very beginning of his life. It must have been a great disappointment for Born to find that his favorite student was drifting off in the direction of German nationalism. The tragedy was made more acute because Born himself was of Jewish origin and Heisenberg ultimately joined the Nazis. This was enough of a tragedy for any one normal life, but we must add to it that later, when Born had retired to Great Britain after the war, his most brilliant student was—Klaus Fuchs.

As I have said before, my Göttingen paper had attracted a certain amount of attention, and hints were dropped to me, emanating from Hilbert, Courant, and Born, that I might be the recipient of an invitation to work at Göttingen some time the next year. In the meantime, Born was coming over to lecture at the Massachusetts Institute of Technology, and I prepared myself to work with him through the intervening months.

When Professor Born came to the United States he was enormously excited about the new basis Heisenberg had just given for the quantum theory of the atom. This theory was an essentially discrete one, and the tools for its study consisted in certain square arrays of numbers known as matrices. The separateness of the lines and columns of these matrices was associated with the separateness of the radiation lines in the spectrum of an atom. However, not all parts of the spectrum of an atom are made of discrete lines, and Born wanted a theory which would generalize these matrices or grids of numbers into something with a continuity comparable to that of the continuous part of the spectrum. The job was a highly technical one, and he counted on me for aid.

There is no point in my going into the technique of a piece of work which not only is highly abstract but was to a certain extent a transitory stage of quantum theory. Suffice it to say that I had the generalization of matrices already at hand in the form of what is known as operators. Born had a good many qualms about the soundness of my method and kept wondering if Hilbert would approve of my mathematics. Hilbert did, in fact, approve of it, and operators have since remained an essential part of quantum theory. They were introduced about the same time by the independent work of Paul Dirac in England. Moreover, they turned out to be useful in tying up another form of quantum mechanics just being invented in Vienna by Erwin Schrödinger with the Heisenberg form of the theory.

From this time on, quantum mechanics went into an active phase of growth in which young men like Heisenberg himself, Dirac, Wolfgang Pauli, and John von Neumann were making new discoveries almost every day. This feverish atmosphere is not one in which I function well, nor did I feel the need of intervening in a subject which was already so competently handled. I did have an idea that the philosophy behind my old paper on Brownian motion could be exploited in quantum mechanics; but the type of difficulty which bothered me and the type of problem where my method was useful were not to become actual for another twenty years. I have returned to this field in the last decade, together with Armand Siegel of Boston University, and I am hopeful that now at last I shall have something useful to say which has not been said by other people.

In all this work, past and to come, I must remember—and the reader must remember—that the task of physics at the present day is not that of carrying out into further and further refinements an existing theory where essentials are well understood. Physics is at present a mass of partial theories which no man has yet been able to render truly and clearly consistent. It has been well said that the modern physicist is a quantum theorist on Monday, Wednesday, and Friday and a student of gravitational relativity theory on Tuesday, Thursday, and Saturday. On Sunday the physicist is neither but is praying to his God that someone, preferably himself, will find the reconciliation between these two views.

Notes

1. In this chapter I must face the problem of trying to explain in nontechnical language what the technical meaning of some very difficult work has been. The reader who is not interested in a detailed documentation of my work at this time is advised to skip over the technical passages.

2. The step from Leibniz to Swift involves certain aspects of the history of the early eighteenth century which deserve comment. Leibniz was a great philosopher and physicist by avocation, but his official position had been that of archivist to the court of Hanover. In this position he showed himself to be not only a librarian but a diplomat of the first rank, eager for the welfare and the aggrandizement of his ruler. There is much to be said for the conjecture that he was active in the negotiations which put the house of Hanover on the throne of England. Since it was the Whig party in England which desired the coming of the Hanoverians, in order to terminate the reign of the unpopular Stuarts, Leibniz became identified with the Whig intrigues. His contact with England was greatly facilitated by his membership and his active share in the Royal Society.

Swift, on the other hand, was a Tory supporter of the Stuarts, and he took an active share in the attempted *coup d'état* which tried to put the Old Pretender on the throne after the death of Queen Anne. Thus, Leibniz and Swift were key figures, respectively, of the two conflicting parties in the English politics of the day. It is no wonder that a great antagonism grew up between them.

This antagonism is shown in the third of the four books of *Gulliver's Travels,* the voyage to Laputa. Many people have wondered at the virulence with which Swift lashes scientists, these impractical projectors who measure a man with a sextant to fit him with a suit of clothes, who extract sunbeams from cucumbers, and who attempt to attain all the learning of the ages by a process equivalent to Eddington's monkeys and typewriters. In fact, they represent nothing but the Royal Society and in particular the Leibnizian influence in the Royal Society. It is thus not astonishing that one of the targets of Swift's wit should be the essentially Leibnizian situation of the fleas upon the fleas and so on ad infinitum.

This was not the only place at which Swift showed himself fascinated with the problem of the variable scale of nature and of what would happen to the world and the individuals in it under a contraction or an expansion. It is likewise the theme of the first two books of *Gulliver's Travels,* the voyage to Lilliput, where the inhabitants are one twelfth the height of a normal man, and the voyage to Brobdingnag, where the inhabitants are giants seventy feet tall.

In both cases, Swift's imagination concerning the effect of change of size is keen but limited. It applies to the physical dimensions but not to their powers of motion. He is not aware that the Lilliputians, if they were made of human flesh and blood, should be able to jump a height several times greater than their own, nor of the similar fact that his Brobdingnagians would be so slothful and earth-bound that they would be scarcely able to stand up.

24 To Europe as a Guggenheim Fellow with My Bride

For the last few years, the meetings between Margaret and me had been a bit too intermittent to suit us. Her teaching and her continued obligations to her own family kept her fully occupied. For my part, my position was not yet sufficiently secure for me to take on the obligations of a married man. Yet the recognition I was receiving from Germany, together with an improved economic status at M.I.T. consequent upon it, now for the first time made it possible for me to look the responsibilities of marriage in the face. Margaret came down to see me at Christmas. I proposed again and was accepted, and we decided to get married and to take the European trip together as our honeymoon.

However, there were difficulties of detail. It was planned that I should arrive in Göttingen in April for the summer semester, at which time Margaret would still be teaching modern languages at Juniata College, in Pennsylvania. She did not wish to resign from the job two months before the end of the school year. For a while we thought of getting married in Europe, but we found that the red tape attendant upon this made it practically impossible. We played with the idea of an embassy marriage or of a marriage at sea by the captain of an American ship. These courses of action also ran into serious difficulties. Finally, we had to admit that the practical and sensible thing was to get married in the United States just before my trip, for Margaret to go back to her teaching work, and for her to join me in Europe during the summer.

Margaret left Boston again for her work at Juniata. In the meantime I found myself very busy and hardly able to think of the new problems of marriage and of the trip. I had an active social life at the time, and on one occasion soon after Christmas the Borns invited a group of us to their apartment to show off a new electric train they had bought for their children in Germany. There was quite a group of scientists and electrical engineers present to witness this occasion—Vannevar Bush, now head of the Carnegie

Institution of Washington; Manuel Sandoval Vallarta—the young man who had helped me to translate my article into French—at present vice-minister of education in Mexico, formerly professor of physics at M.I.T., and many others whose names have become household words among those dealing with electricity. When the train was assembled for display and the switch was thrown, the transformer flashed and burned out. It was a considerable time before our combined engineering talents were able to diagnose the trouble. That part of Boston was on direct current, on which no transformer could function.

I was closely associated with Vannevar Bush in my work during this time. Bush was already developing some of the various forms of electrical computing machines which were later to make him famous. From time to time he would call on me for advice, and I tried to do what I could in designing computational apparatus on my own account.

I have already spoken of my work on harmonic analysis, which even at that time seemed to me to be pointed directly toward important practical realizations. Since then these applied realizations have been made; and, as I shall show later, generalized harmonic analysis is an important part of my work even to the present day.

One time when I was visiting the show at the old Copley Theater, an idea came into my mind which simply distracted all my attention from the performance. It was the notion of an optical computing machine for harmonic analysis. I had already learned not to disregard these stray ideas, no matter when they came to my attention, and I promptly left the theater to work out some of the details of my new plan. The next day I consulted Bush.

The idea was valid, and we made a couple of attempts to put it into working form. In these, my contribution was wholly intellectual, for I am among the clumsiest of men and it is utterly beyond me even to put two wires together so they will make a satisfactory contact. Bush is, among other things, one of the greatest apparatus men that America has ever seen, and he thinks with his hands as well as with his brain. Thus, our attempts in a new sort of harmonic analyzer were quite reasonably successful, and since then they have led to work even more successful.

Ultimately the spring came, and I was about to leave for Germany. I was in a very exulted mood at what I conceived to be the first wholehearted recognition that had come my way, and I am afraid that I talked more of it to the newspapers than was strictly becoming. I felt that I had now got from under the pressure and the indifference of Birkhoff and Veblen. I was more eager to begin the duties of my new position, and I must have been an insufferable young man in my boasting and gloating.

Margaret and I were married in the parish house of a Lutheran Church in Philadelphia. We left at once for a few days' advance honeymoon in Atlantic City, to separate again for the months until Margaret should have discharged her duties at Juniata. She saw me off at the boat in New York. The hotel to which I took her in New York was the old Murray Hill Hotel, which had been for many years the special headquarters of the meetings of the American Mathematical Society, and which was a gloomy old-fashioned marble and porphyry mausoleum inhabited almost exclusively by elderly ladies, around whom flitted the spirits of the not-so-gay nineties.

After this depressing incident I took Margaret to the theater. As luck would have it, we went to see Ibsen's *Ghosts*, the gloomiest of all plays by that most gloomy playwright. These things would not have mattered much if they had only been incidents in a prolonged honeymoon, but as a prelude to a separation of many months they must have been devastating to Margaret's peace of mind.

I arrived in England to see the Devonshire spring already established and the primroses in full bloom. After visiting Hardy at Oxford, where he had now become professor, I went to Göttingen and took up quarters with the landlady of my student days.

I have already mentioned Richard Courant, the young mathematician on whom the administrative mantle of Felix Klein had fallen, as the pope of Göttingen. Courant, who had been amiable on my last Göttingen visit, now turned out to be somewhat hostile. The list of Guggenheim fellows had appeared in the American newspapers, and, as I have said, I had been a bit loquacious concerning my prospective trip. I gave an interview which came to the attention of the all-seeing eye of the Amerika-Institut in Berlin, which went on to dig up the fact of my father's bitter opposition to Germany in the First World War.

While Nazism did not become official in Germany until 1932, there was a strong and bitter nationalist element which had already assumed great power, and which had begun to terrify the more liberal elements in the universities. These universities were of course government institutions, and thus subject to nationalistic pressure. This was also the precise moment at which Courant wanted very much to gain the good will of the United States. The Rockefeller Foundation was deeply interested in European reconstruction. As far as mathematics was concerned, it had picked out the University of Göttingen as the first object of its benefactions. This was entirely natural and right, because Göttingen at that time held an unquestioned first rank as the great center of world mathematics. Later on that year, as I then learned, Birkhoff was to visit Göttingen and to make a detailed report on the project for an improved and enlarged mathematical institute.

I do not envy Courant his difficult position between the upper and the nether millstones. However, it was myself rather than Courant who felt them grind the hardest. Courant's attitude to me became quite cold, and the favors which had been promised me were either denied or granted in such a grudging way that they were not acceptable.

Courant scolded me for my newspaper publicity and was disposed to deny me the assistant and the complete official recognition which he had promised me. Nevertheless, he allowed me to continue in Göttingen in an unofficial way, and after some persuasion found an able young mathematician to aid in the preparation of my lectures, provided I should pay him out of my own pocket.

I was left at Göttingen with a position that belonged neither to flesh, to fowl, nor to good red herring. The humiliation brought me to the edge of a nervous breakdown. Partly as a consequence of this, my lectures were less successful than I could have wished, both as examples of mathematical research and as lectures in the German language. I have no doubt that I would have broken down if it had not been for the loyalty of a few American and English friends, who cheered me up in my blues, went for long walks with me, and attended my classes at a time at which almost all the German students and docents had given them up as a bad job.

Chief among the Americans was J. R. Kline, a Pennsylvania German who was many years later to become the secretary of the American Mathematical Society and the head of the mathematics department at the University of Pennsylvania. He was there with his wife and little boy, and they took me in almost as a member of the family until Margaret came over and lightened their burden for them.

Of my English friends the chief was A. E. Ingham, then a don at the University of Leeds and later a fellow of King's College, Cambridge. Ingham was a shy, almost timid man who had already begun to do distinguished work in the theory of numbers. It is to Ingham that I owe a scientific lead which has carried me to much of my best work.

There were places in my theory of generalized harmonic analysis which I was nearly but not quite able to bring to a definite close. I needed certain theorems, and I found myself proving similar but not identical ones. Ingham pointed out that many similar problems had been solved by Hardy and Littlewood by what they called the method of Tauberian theorems. The study of these is a job belonging to the technique of the mathematician rather than to his repertory of ideas, and I do not intend to try to expound it to the layman. It is enough to say that I made a new attack on this field which was thoroughly successful, and that I not only closed the gap in my earlier work but was able to go on to the simplification of large areas in the theory of whole numbers.

With Ingham and Kline as my friends, I turned my attention to a premature idea of reviving the two old Göttingen clubs, the American and the British Colony which had been the center of my life there in my student days. Kline and I had hoped to improve German-American relations by reestablishing the American colony. Accordingly, we turned for aid to one of the subadministrators of the university.

This subadministrator, who turned out to be a very questionable character, backed us to the limit. He introduced us to a group of young German students, whom I later found to have all the marks of the Nazi about them. Our administrator friend saw to it that our plans got a certain publicity in a local newspaper.

This came to Courant's attention, and he was furious. He squelched the subadministrator with all the fund of contempt which the German professor has for the underlings of the university office. We ourselves got the backlash of his anger, and my very weak position at Göttingen became even weaker.

I had expected that my Göttingen recognition would be a way of getting out from under the continued hostile pressure of Birkhoff in the United States, but now Birkhoff had himself come to Göttingen. He represented the American whose support Courant most wanted.

Courant approached me as an avenue through which he might win Birkhoff's good will. I told him that I had no influence whatever with Birkhoff and that Birkhoff's entire reaction to me was hostile. I kept away from Birkhoff on his visit. I felt that the relations between Birkhoff and Courant were their own business.

Soon after school had closed in the United States, Margaret came over to join me in Göttingen. I fetched her from the boat at Le Havre, and after a few days together in Paris and a short trip to Holland we arrived in Göttingen. It was a pretty sorry and confused state of things into which I introduced her, and it must have been a great shock for a newly wedded wife as yet imperfectly acquainted with her husband. Besides consoling me, she had serious work to do in bringing my landlady to a proper sense of her responsibilities and to a halfway fair treatment of our business relations. The difficult situation in which I found myself as far as my relations with

Courant were concerned had gone too far for any repair, but Margaret did her best to help me mend my fences.

Soon after our arrival we threw a belated wedding party for our friends at a well-known Göttingen wine restaurant, where the wine steward did everything in his power to see that the wines we ordered were adequate without being overly expensive. He pointed out that after the first bottle our guests would no longer be interested in the superlative excellence of the wines we ordered and suggested that we order a cheaper wine for the succeeding bottles. Our guests brought us as a wedding present a beautiful tablecloth and set of napkins.

It was not long after that that my parents decided to visit Europe, partly to share in my supposed success and partly to keep a supervising eye over the newly-married couple. This more than doubled my already unsolvable problems. Was I to tell my parents of the rebuff I had received and of the reason for it? As I have said, this lay partly in my father's opinions and in a confusion which the Germans had made of the two of us. It has always been harder for me to be safely wise than to blurt things out, and I told my father what had happened. Naturally, he was much more interested in his personal rebuff than in extricating me from my impossible situation. It was not a very happy week that we spent together in Göttingen, nor was it possible to keep my father and mother from going over my head and attempting to deal directly with the Göttingen people and the German educational authorities.

Margaret and I decided to spend our summer vacation in Switzerland. We went to Bönigen, a suburb of Interlaken, to a little hotel which my sister Bertha and I had already visited on an earlier trip to Europe. Later on the Klines came down from Göttingen to join us at the same hotel. Part of the time we wandered over the foothills of the Alps and some of the time I would play chess with the proprietor, a friendly wine merchant with whom we were on the best of terms. But, suddenly, our stay in Bönigen was terminated by a peremptory summons from my parents, who were passing their holidays in Innsbruck, in Tyrol.

Margaret and I needed this time for those adjustments in marriage which consist primarily in getting acquainted with one another, and which are rendered infinitely more difficult by any attempt at surveillance. On the other hand, through the course of the years, I had become too emotionally dependent on my parents to ignore their summons.

We found Innsbruck delightful, with its walks, its little theater, and its scenery, but my parents were in an irreconcilable mood. Father insisted on my writing an immediate and unconditional protest to the Prussian minister of education. This I knew to be futile, for it was perfectly clear that the minister of education was the real source of all my difficulties. It was foolish and weak of me to submit, but the habits of years are not easily overcome. It took a long time, even after that, for Margaret to build up in me a certain degree of independence from my parents as an individual and as the head of a family in my own right.

Finally we went to Italy for three weeks of a real honeymoon. We first spent a little time in Bolzano, which had but recently been ltalianized from its previous South Tyrolean status as Bozen, and which was not happy under the change. Then came a brief stay among the dust-covered olive groves of the Lago di Carda.

There followed a visit to the magic city of Venice, with its fabulous watery streets, its treasures of architecture, and the delightful Lido. This Venetian visit would have been pure fairyland if it had not been for the black depression caused by my Göttingen experience.

It was no pleasant experience for Margaret to become involved with the problems of a neurotic husband at his very lowest emotional level. I had become even more of a problem, because my parents had made a policy of glossing over my emotional difficulties, instead of confronting Margaret with the real task she had undertaken in marrying me.

From Venice we went on to Florence and to Rome. Florence in particular seemed to us a city of unbelievable beauty and distinction, which we could appreciate even through the veil of our emotional confusion.

However, the time came when we had to decide what to do with my remaining half-year abroad. For the immediate future, the meeting of the German Association for the Advancement of Science, in Düsseldorf, awaited us. After that, we felt that we had had our fill of Göttingen, and we decided to spend our remaining time in Europe, until January 1927, in the more genial atmosphere of Copenhagen. I had received Harald Bohr's permission to work with him, and I was determined to make up for the blight of my Göttingen visit.

We made a hurried and fatiguing journey to Düsseldorf by way of Switzerland and the Rhine. In Düsseldorf I gave a paper, and I made many new and agreeable contacts with German scientists. In particular, I met a young mathematician named Robert Schmidt, who was an instructor at the University of Kiel. Schmidt had done some important work on Tauberian theorems which, as I saw, was closely related to my own new ideas. We decided to pool our efforts. He pointed out to me in particular that a Tauberian theorem of the comprehensive character, the kind that I had some hope of proving, would be most valuable in number theory, and in particular in the problem of the distribution of prime numbers: such numbers as 2, 3, 5, 7, 11, which have no other factor besides one and themselves.

In the late nineties, two great mathematicians, Hadamard and De La Vallée Poussin, of Louvain, succeeded in proving that the number of primes less than a larger number *n* was approximately $\frac{n}{\log n}$. Their proofs were thoroughly rigorous and satisfying, but somewhat complicated. Their theorem had been on the point of being proved for many years before they had succeeded in demonstrating it, and the great German mathematician Riemann had come near to establishing it in the third quarter of the nineteenth century. Riemann had made a certain conjecture which he had not been able to establish, but which, if it was true, would lead to a much finer estimate of the distribution of the primes.

To make a long story short, I found my way clear to using my methods to give a much simpler proof of the prime-number theorem and ultimately several much simpler proofs. It was Schmidt who directed my efforts toward this problem, and Schmidt also suggested to me that I might be able to establish or refute the Riemann hypothesis. In this more difficult problem, however, I have always found my efforts completely inadequate.

During my later stay in Copenhagen I made a couple of visits to Schmidt at Kiel. At first he was enthusiastic about my new method, but he gradually began to lose confidence in what I was doing. He threw the work entirely back on my own hands. There were, in fact, some gaps in my proof at that early stage; but they were the sort of gaps which were easily filled up. The repudiation of my work by Schmidt proved a blessing in disguise, for it gave back to me the full control of a piece of research which, if it was not the best that I was ever to do, was certainly close to my best, and which gave me a reputation incomparably greater than that which any of my earlier work had given me.

Courant was at the Düsseldorf meeting, and he tried to get me back again for another term in Göttingen. I told him that a further visit to him would have no point. Margaret and I made a brief trip to nearby Belgium, from which we took an unbelievably fatiguing train voyage to Copenhagen.

To go to Copenhagen by train one had then to take the ferry from Warnemünde to Gjedser. We traveled third class and spent an unhappy night in the red-painted, roughly-beamed, third-class dining saloon on the train ferry. It is a place to make one contemplate all one's past sins and all one's wasted opportunities. The passengers were huddled against one another in an uneasy sleep, and the swinging lanterns cast their oscillating shadows on the floor to the tune of the rocking of the boat and the creaking of the timbers.

When we got to Copenhagen we were nearly dead, and we slept for a whole day. Then I looked up Harald Bohr and prepared for some months of research. We saw a good deal of the brothers Bohr. I remember that at the apartment of one of them, I believe it was Niels, there was a plaque of the two as children which gave them an undeniable peasantlike appearance, which they had lost in the course of the years. One of the other guests, a lady who was professor of classics at the University of Copenhagen, and who smoked big black cigars incessantly, told us that some friend had commiserated with their mother for having two such dull boys for children. In view of the fact that Niels Bohr has become the national hero of Denmark because of his scientific work and lives in a palace donated by one of the great Copenhagen breweries and that Harald was certainly the greatest mathematician ever to live in Denmark, this story now seems more than a little ridiculous.

Copenhagen was a delightful city which combined the intellectual amenities of a world capital with the hominess of a small town. In the intellectual world everyone knew everyone else, and there was an atmosphere of friendliness pervading the whole of life.

The Bohrs were charming to us, as was their colleague, Professor Nørlund, whom I had already met in Strasbourg. Nørlund was a tall, handsome, bearded man, who had gone from pure mathematical work to the headship of the Geodetic Survey of Greenland, and whose house was frequented by bluff Arctic sea captains. Mrs. Nørlund retained the beauty which had so impressed me at Strasbourg. She was most cordial to us. We had already decided to learn Danish, and were taking Danish lessons with a high-school teacher who had spent some time in the United States. Mrs. Nørlund supplemented our Danish instruction by reading with us Andersen's *Fairy Tales* in the original. The beauty of these fairy tales in the sweet, intimate Danish language was brought out to the full by the charming way in which she read them.

Besides my work on Tauberian theorems and number theory, I made one or two new starts in Copenhagen on important points in harmonic analysis. Copenhagen was a rest and a refuge after the turmoil of Göttingen.

I have said that we left Copenhagen for a brief trip to Germany, where my wife visited her relatives and I worked with Robert Schmidt. After that we returned to the extended festivities of the Danish Christmas and New Year season. Two weeks are devoted to nothing but parties, and the crowds take delight in milling up and down the narrow business street of Strøget.

The time had now come to go back to the States. We returned by way of England. Margaret and I took over the room of some American friends of hers who had been studying in London. I found the mild winter a fine occasion to talk over my work with Hardy and take advantage of his criticisms. Then we returned to the States after a calm winter voyage and spent a day or two with my sister Constance before we started house hunting.

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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.

26 An Unofficial Cambridge Don

We spent the summer as usual, resting and hiking. I took rather longer walks with the children of the valley than I had before, since they were now old enough to stand the severe trip over the Presidential Range. Shortly before we left to take the boat at Montreal, I went with a few of my young friends for a quick trip to Mount Chocorua, where one of them had the misfortune to turn his ankle. The problem of getting him down was considerable, since he had to stand behind me and rest his weight on my shoulders for two grueling hours. The result was that I was tired and had a bad cold coming on when the time came for us to make the trip.

This was not the worst of it. Peggy, who was about one and a half years old at this time, had managed to develop a slight fever; and although the local doctor thought that a sea trip would quickly put her in shape, we were none too confident. Thus, we were rather under the weather when we took the train at Meredith for Montreal and our embarkation.

After a difficult night at a hotel, we just managed to embark in the morning. Then our illnesses really gripped us. Fortunately, the Canadian Pacific boat on which we traveled had an excellent doctor, who had himself taken to the sea to recover from a complicated and crippling illness, as well as a first-rate Scottish trained nurse and a couple of ex-N.C.O.'s of the Royal Army Medical Corps. Peggy and I went immediately to bed, and while the nurse took care of her in our cabin, I went through a bout of septic sore throat under the care of one of the medical stewards.

When I had recovered enough to drag myself around the deck, I found that Peggy was still no better. The doctor came and listened to her lungs with the stethoscope. As soon as I saw that he had settled down to the detailed examination of a particular spot on her chest I knew we were in for trouble. It was bronchopneumonia all right, and for about half of the voyage we were not at all certain of Peggy's recovery.

The doctor consulted with another doctor who was aboard as a passenger, and between them they hit on a course of treatment. By the time the voyage was half over the worst was passed. We wirelessed ahead for an ambulance to meet the boat at Tilbury docks to take Peggy to the local hospital, and settled down to enjoy the remainder of the trip as much as we could with a considerable burden of worry still on our minds.

In due course we landed, and Peggy was taken to the hospital, where it was considered advisable for her morale and for that of the hospital that we should see as little as possible of her until we had found quarters at Cambridge and were prepared to take her back with us.

We stayed for a few days at an excellent hotel run by the Port of London Authority, where I had the pleasure of meeting the personal representative of that body—the captain of the port and harbourmaster of London. He was much interested in engineering, and especially meteorology. He told me what a lonely profession his was, with a small group of perhaps less than ten men in the whole world with a comparable job; and how badly he had felt during World War I to have been shut off from his opposite numbers at Hamburg and at Antwerp. He said that, in fact, under normal conditions, London, Antwerp, Rotterdam, Hamburg, and Bremen all constituted one great port which had to be governed by the closest cooperative work of all the harbor masters concerned. This was largely due to the unbalanced import trade of London, which so greatly exceeds its export trade that ships leaving their cargoes at London have to pick up cargoes elsewhere.

I made one or two visits to London and to my friends there before Margaret, Barbara, and I went up to Cambridge. We stayed at a delightful hotel near the river and immediately began to inquire of my friends to find out the proper way to get ourselves housed for the winter. Finally, with the aid of an estate agent, we were able to rent a typical English middle-class cottage in New Chesterton, to the northeast of Cambridge proper and not more than a block from where the housing developments began to peter out into open country and farms.

We found a nursery school for Barbara, and thus we were to some extent free from the burden which had been heavy on my wife on the ship—nursing a very sick child and at the same time keeping a very well and lively child in some sort of discipline. Peggy had recovered from her acute illness but was still in delicate health. We secured a Yorkshire woman who had worked at our hotel to take care of the house and hired a car for the long cross-country journey to Tilbury and back.

This was my first auto trip of any length in England, and I marveled at the abrupt turns of the narrow roads and the way that the main street of each village seemed unfailingly to be on the route taken by our car.

In a few weeks Peggy had recovered most of her strength. There was, however, a stubborn low-grade infection of the middle ear with which we still had to contend, and indeed it was years before Peggy again showed the full vigor which she had had before the trip. Now that they are both grown women, it is still a little surprising to us to see that Peggy is the really vigorous one, having thrown off all her youthful difficulties, while Barbara is somewhat slighter and frailer.

The Yorkshire nurse soon gave Peggy the native accent, no trace of which, happily, remains, though Peggy used to say to me, "Ah coom to Daddy," in the best manner of the West Riding.

Our house had a long narrow garden, with a garden house at the back that could be turned to suit the direction of the sun and the rain. The very friendly lady who rented us the furnished house, and who lived only a door or two from us, secured us the services of a gardener.

Not only was this lady most cordial, but the neighbors in general began to drop in on us from the very first day, with a quicker and readier cordiality than that which I had found in any Boston suburb. This certainly belied the legend of British unapproachability, but perhaps our peculiar position in the community had something to do with it. Most of our neighbors were university people. Not only was I a university man, but I had been at Cambridge many years before and had a certain connection with the place. As Jessie Whitehead has said to me, I must wear the arms of Trinity College with the difference of a bar sinister. Yet, although I am a Cambridge man only on the left hand, the Cambridge family has at least acknowledged me. At any rate, the reader in Italian, who lived two doors from us, and the lecturer in tropical agriculture, who lived next door, visited us at the earliest possible moment and had us over repeatedly to informal teas. This was precisely the easy hospitality which was most to our taste.

Later on we made the acquaintance of the reader in Hebrew, who lived a few hundred yards down the street, and who was official rabbi simultaneously for Oxford, Cambridge, and one of His Majesty's jails. He was a fellow of Queen's College, where he had grown up together with a covey of High Church parsons. He was indeed very much the Jewish equivalent of a High Church parson. He was a ritualist to the last degree, and the door posts of his house had the traditional Hebrew texts fastened to them.

Like so many High Church parsons, he was extremely liberal in his opinions. When it was necessary to raise funds for the repair of the old Norman round church, it was on his lawn that the garden party was held. He had a most interesting collection of seventeenth- and eighteenth-century prints of synagogues and Jewish ceremonies in Holland, among both the Ashkenazic and the Sephardic groups. His particular hobby was to find some of the sources of the Gregorian chants and of Christian ritual in Jewish music and rites.

During my stay in Cambridge, I would run down to London to try to sell the English rights to the filter inventions which Lee and I had made. However, it was no go, largely because a communication engineering did not then seem to be a truly indigenous British industry but rather the reflection of more active interests in the United States and Germany. Furthermore, it became clear to me on a little investigation that a British patent—or, for that matter, almost any sort of European patent—was not something to be taken out as lightheartedly as an American patent (which is in effect nothing but a license to litigation) but involves a considerably greater amount of research and conveys a much greater expectation that the courts will uphold it. It guarantees greater rights and is proportionally more expensive to obtain. In most countries, the laws are less favorable to paper patents than they are in the United States and make very explicit demands that a valid patent be exploited in industry in order for it to remain alive.

While I was in London I met Miss Cartwright, who has since become Mistress of Girton College. She was then, as she is now, a delightfully sincere and unpretentious mathematician in the first rank of English mathematicians, men or women. She invited me to tea to meet a young Trinity don named Paley, of whom I was to see much in the future.

Soon after I arrived in Cambridge, I made the acquaintance of the Youngs, brother and sister. Young was then a don at Peterhouse, I believe, and had done some interesting work which we continued to develop together. Miss Young, a Girton don, was particularly kind to my daughters and she invited all four of us to tea in her rooms.

But of all my scientific contacts in Cambridge, the most important remained my teacher Hardy and his other self, Professor Littlewood. By now, Hardy had become an aged and shriveled replica of the young man whom I had met in Russell's rooms in my student days. Yet he continued to be a redoubtable tennis player and an enthusiast for cricket who knew every finesse of the game. Later on, during his many visits to the United States, he developed an almost equal interest in baseball, and Babe Ruth became a name as familiar in his mouth as that of the cricketer Hobbs.

Littlewood was near the peak of his rock-climbing career. He used to invite me to his rooms at Trinity and demonstrate some interesting rock climbing maneuvers by taking traverses about the base of the pillars in Neville's Court. Hardy and Littlewood took me to see a cricket match. They also showed me a rugger game, in which the players on the two sides managed to pile themselves into unbelievable heaps after a scrummage. I am afraid I did not understand the finer points of either game.

Hardy was lecturing on the elementary theory of numbers, but I did not follow any of Littlewood's lectures. However, I did go occasionally to the mathematical seminar which he held in his rooms.

Unlike Hardy, who had a hatred for the applications of mathematics, particularly those to engineering and warfare, Littlewood has a very considerable physical sense, and played an important part among the mathematical advisers to the military of both the First and Second World Wars. In the First World War he discovered a very brilliant way of computing trajectories of missiles and in particular of making use of a few computed trajectories to obtain a whole range table by interpolation. In the Second World War he and Miss Cartwright left their own chosen field of abstract mathematics for a very useful study of differential equations.

I was invited almost every week to dine at high table either at Trinity or at some other college, and indeed Trinity treated me almost like a supernumerary don. I found the table talk most interesting and less a formal game of high-pressure wit than I had been led to expect. Indeed, when I later visited my academic friends at Oxford, it became clear to me that there was a great gulf between the two schools and that the high-pressure wit which had not been too greatly in evidence in Cambridge was in fact the order of the day at Oxford.

One of the delightful experiences which I had at Cambridge was to go out after the port and a cigar in the Trinity common room following high table and participate with the dons in a game of bowls. I was as poor at this as in all sports of skill, but it was a pleasure to relax with my friends in this cozy and almost secret little garden as the long English dusk came on. It was bowls and not bowling for these two sports must be carefully distinguished. The English sport, which dates back far beyond the time at which Drake played it on the Hoe at Plymouth while waiting for the Spanish Armada, has nothing to do with bowling alleys or tenpins. It is a much closer relative of curling, which, in fact, is nothing but a variety of bowls played on the ice.

The make-up both of the undergraduate body and of the dons at the two universities had greatly changed since the war. No longer were universities the privileged property of the ruling classes. They were now full of brilliant young men who would not have been there at all if they had not been subsidized. Indeed, most of the honors men were subsidized, and the undergraduate who came up for a pass degree was no longer looked on with favor.

Some of these subsidized students came from very poor families and bore the marks of earlier poverty and malnutrition in their stunted forms and poor teeth. Many of them, in addition, had a social hurdle to overcome, which in most cases was more in their own consciousness than in the minds of those about them. I have had young friends from the two universities—and especially from Oxford—tell me how carefully they worked to learn to play the conversational game at the high table. The background of this problem will be clear to all who know D. H. Lawrence and his work, for on the literary side he represents the precise analogue of many of the young mathematicians whom I met in England and, later, as Commonwealth Fellows in America.

The year I was at Cambridge was a great one in the annals of physics, for Cockcroft and Walton had just split the atom for the first time. I saw their apparatus—a pile of glass cylinders and glass plates with holes cut through them, joined together with De Kautinisky cement, which is a glorified form of lowvapor-pressure sealing wax. I was impressed then and I am impressed now with the way in which the English physicists, and indeed European physicists in general, instead of waiting (as do so many American scientists of the present day) for an enormous appropriation, use the material at their disposal and make ingenuity do what one might have thought only money could accomplish.

Yet there was one laboratory at Cambridge which involved great expenditures. This was the magnetic laboratory of the Russian physicist, P. L. Kapitza, who designed powerful generators to be short-circuited and thus put enormous currents through great leads which threshed around like angry snakes under the tremendous magnetic fields thus created. Later on Kapitza was to go back on a visit to Soviet Russia, which has kept a tight hold on him ever since—whether with or without his consent no one will know. At any rate, the Russians sent to England for the whole equipment of the laboratory. He became the pioneer in Russia of that large-scale, factorylike type of laboratory which had first been employed by Kammerlingh Onnes in the Netherlands for low temperature research, and which is now the standard means of exploring the nucleus and of designing atomic bombs. Thus, I felt sure, as soon as I heard of the atomic bomb and of our use of it, that with Kapitza to train the Russians in the technique of this sort of laboratory it would not be many years before they would have mastered for themselves the principles and technique of nuclear research, whether or not they might capture our secrets by means of espionage or persuade a group of malcontents to serve their purposes.

I found a couple of American friends at Cambridge. One of them was a young lady who had done a thesis under me on coherency matrices and was taking a year off from her academic job to combine research with leisure at Cambridge. The other had been an instructor in the Mathematics Department the second year I was at M.I.T. Later on he had gone to study at Munich, where he had managed to get himself embroiled in a challenge to a duel with an army officer while helping an American lady onto a streetcar. He escaped the duel by the ingenious device of using his right to name the weapons. He proposed that these should be bows and arrows and then had his friends let it out that he was an archer of great prowess. Some of us at Tech had seen the story in the Paris edition of the New York Times and had written to him a letter purporting to come from some archery association and offering him the highest honor it had the capacity to bestow. He had always been a bit gullible, and he swallowed the bait hook, line, and sinker. But we could not help admiring the genuine courage of the man, and I think his honest and sincere letter turned the laugh rather against us. At any rate, we found him in Cambridge as a great deal of a companion and a little of a butt.

I tried to walk my young American friend down on a hiking expedition around Cambridge. This time, though, the joke was on me, for he walked me down instead. In fact, he spread the tale of my injudicious challenge to his ability so far and wide that later, when another American went for a hike with me in the Lake Country, I was hard put to it to keep up with his challenge.

As the autumn term drew to a close, Professor Hardy broached two matters to me. One concerned the possibility of my having a book on the Fourier integral accepted by the Cambridge University Press, another the curious Cambridge custom by which a professor may give his lectures by deputy. Professor Hardy had the right to authorize any person to give a course of lectures on his behalf, and these lectures were to be regarded by his College, the University, and the Board of Studies exactly as if he had given them himself. Thus, I was to be a quasi don at Cambridge and to lecture during the second term on my own work on the Fourier integral, even as I had many years before been a quasi undergraduate and attended courses without immatriculation, on the basis of my Harvard connections.

As the term drew to an end and the Christmas holidays came near, I began to receive invitations from my Continental colleagues to lecture at their universities, some of which were still in session. Professor Wilhelm Blaschke invited me to speak at Hamburg; Professor Karl Menger, of Vienna, offered to put me up for a couple of weeks in his rooms; and Professor Philipp Frank, of the German University at Prague, asked me to come there for a few lectures.

Merely to think of these names brings to my mind the vicissitudes which these men have suffered since. Blaschke became, during the Second World War, if not an ardent Nazi, at least an ardent supporter of the Nazis, and he wrote articles in ridicule of American mathematics. In particular, he showed his contempt for the mathematical school of Princeton, which he called "a little Negro village." Menger later on came to the United States as a refugee. I helped him find a position at Notre Dame. I believe he is now in the Illinois Institute of Technology. Frank also came over as a refugee from Hitler and has recently retired from Harvard.

Many of the other mathematicians whom we met on this Continental trip—in fact, most of them—are in the United States or have died since. Hahn, of Vienna, is dead; Artin, of Hamburg, is a professor at Princeton; Gödel, who was Menger's assistant in Vienna, is also at Princeton, where he has done much of his great work on the basis of mathematical logic. Von Mises, of Berlin, whom I met later on the trip, was another Harvard professor until his recent death. In fact, the mathematical schools that were then dominating the Continent have either been transferred bodily across the seas or have gone out of existence completely, and only a very small proportion of the younger men remain to take on the desperate task of rebuilding.

We had a delightful time at Hamburg, where we were taken into the life of the Mathematical Institute. Margaret, the children, and I then entrained for Berlin. There Margaret left me and took the children to see her kinsfolk in Breslau while I continued alone to Vienna.

I had earlier decided to give a few lectures at Prague, and in the course of my travels I wrote to President Masaryk, my father's old friend, hoping to visit him while I was in his country. I therefore took the liberty of writing to him, identifying myself as the small boy whom he had so often seen when he visited my father's house and saying that I was soon to be in Prague. I received a prompt answer inviting Margaret and me to call on him in his palace at Lana. We crossed the Giant Mountains at exactly the best skiing season, and I wished as we passed through that we might visit them in some future winter.

Our friends met us in Prague, saw to it that we found a suitable hotel, and extended to us every cordial hospitality. I was much touched when some of the faculty of the Czech university forgot their traditional feud with the faculty of the German university and came to my lecture.

Finally the day came when we were to drive out to Lana. A state automobile called for us at our hotel and drove us to the palace, through some very bad roads across what seemed quite prosperous farming country. There Masaryk's daughter was waiting for us in a cheerful room with a tall Christmas tree and a blazing fire, until her father should come back from the horseback ride that was part of his daily regimen. It was clear from the informality of our reception that we were being received not as guests of state but as family friends.

The hearty, bearded, old President came in in his riding clothes. He remembered very well his visits to our house on Medford Hillside and cautioned me that I was getting too fat and that I should take more exercise; that I should go in for horseback riding like himself. He expressed great worry at the advances of the Nazis and showed very little hope for the future of Europe. Then he left to take his rest; and after a few minutes' more conversation with his half-American daughter, we left too.

Margaret soon returned to Breslau, and I took the train to Leipzig for a brief visit to Cousin Leon and a talk or two at the University. I think that this was the time I met Koebe, a ponderous, pompous man–"the great expert in the theory of functions," as he was said to have been called by the passers-by in his native town in Brandenburg. There were many tales current about him. On one occasion, when he had visited Da Vinci's horribly mutilated painting of the Last Supper, he is supposed to have said: "How sad! This painting will pass away, while my theorem concerning the uniformization of analytic functions will endure forever!"

Shortly thereafter, we took the train for Holland and the cross-channel boat. By now the children were quite well, but we were exhausted. Luckily, they slept through the crossing, which was as rough as only a winter crossing of the North Sea can be, but we two parents spent the night groveling on the floor of the cabin in an agony of seasickness.

We arrived in Cambridge the next day to find that the frost had burst the plumbing in our bathroom, and we went through two days of annoyance until the plumbers could repair it. However, during this time I had to get my course started, Barbara had to go back to school, and we had to make the best of things.

My course and my book went on very well, and the term continued to follow the pattern of the previous one. I did a large part of my reading in the Philosophical Library, the library of the Cambridge Philosophical Society, in the publications of which some of my earliest work had appeared. It was in this library that I made one of my closest friendships.

I used to read a lot of popular stuff for recreation: particularly detective stories, and the English popular periodicals such as *The Strand* and *Pearson's*. One day I saw in *The Strand* a first-rate thriller called "The Gold-Makers." It was science fiction, with some very plausible science and economics in it, and it had an excellent plot, with conspiracy, pursuit, and escape. It was written by Professor J. B. S. Haldane, of Trinity College, Cambridge. There on the cover stood the photograph of a tall, powerfully built, beetle-browed man, whom I had repeatedly seen in the Philosophical Library.

The next time I saw Haldane at the library, I summoned up my courage to speak to him, to introduce myself, and to express my appreciation of his story. There was one little point in his story, however, which I called to his attention. He had used a Danish name for a character supposed to be an Icelander.

Haldane welcomed this impertinent suggestion of mine, and in a few weeks he invited us to his charming house in Old Chesterton. He first appeared at our house, however, during the Easter vacation, when I was away with an American friend on a hiking-plus-bus trip to the Lake Country. Margaret had never seen Haldane, and he was a little too shy to make clear who he was. However, she recognized him from my description. Haldane invited Margaret to his house to meet Mrs. Haldane. She happened to have gone to London by car and was rather late in returning through the traffic. Hour by hour went on, and still no Mrs. Haldane. Haldane began to be embarrassed, but Margaret was not, and at a late hour Charlotte Haldane turned up. She was very grateful to Margaret for taking the situation with *savoir-faire*.

She was a brilliant young Jewish journalist and novelist, and very charming to Margaret. It was agreed that as soon as I was back from the North, the two families would have a get-together.

Meanwhile, I was hiking over a region which both reminded me of my beloved New Hampshire and contrasted with it. Windermere made me think of Winnipesaukee, but is narrower and less irregular in shape; and the hills behind it are lower, more barren, and more rugged than those behind our New England lakes. There are groves and woods; but the general picture is one of woods surrounded by moor and farmland rather than one of clearings surrounded by woods. There was snow on the higher ground—not as much as there would be in New Hampshire so early in April but still enough to give an atmosphere of bleakness. The houses are of rough stone instead of the wood to which I had been accustomed; the stone walls between the fields are higher and a bit neater than those at home; and the weather, when I went up Scafell, for instance, was cold, wet, and stormy.

When we came back from the trip, much refreshed, Margaret took me over to visit the Haldanes. I remember that we played a lot of bridge with them—family against family, men against women, or Jews against Gentiles. We also had a chance to converse a good deal, and I have never met a man with better conversation or more varied knowledge than J. B. S. Haldane.

It was not long after I came back from my hike in the Lake Country than I began one day to feel rather ill. The doctor came over and diagnosed my case as scarlet fever. We were terribly grieved to have exposed our friends, the Bisonettes, with whom we had spent the previous day, to this infection, and particularly their young boy. However, there was nothing to be done about it, and the ambulance came to take me to the Cambridge Hospital for Contagious Diseases on the outskirts of the city.

I had a pleasant room opening onto a veranda and warmed by a stove, which was necessary as the weather was still occasionally raw and inclement, though it was well along in May. The partitions between the different rooms were of glass, and as soon as I began to be on the mend, which was in a very few days, I found the chance of playing a non-contagious game of five-in-a-row with the occupant of the next room on a piece of paper pressed against this glass partition. I also had plenty of visitors from the university—my friend Paley came particularly often—and it was not impossible for me to do a little work and to correct a little proof. What I missed most was that I could not participate in the jollities and gaieties of the May Week, which came early in June (when I was already a convalescent), but some of the nurses furnished me with the complete gossip and scandals of the town.

When I got out of the hospital, Term was already over and life in Cambridge had the usual dull flavor of life in any academic town during the summer vacation. However, we continued to see a lot of the Haldanes, and I used to go swimming with him in a stretch of the River Cam, which passed by his lawn. Haldane used to take his pipe in swimming. Following his example, I smoked a cigar and, as has always been my habit, wore my glasses. We must have appeared to boaters on the river like a couple of great water animals, a long and a short walrus, let us say, bobbing up and down in the stream.

Later in the summer I was to participate in the Zurich Mathematical Congress, so Margaret, the girls, and I left for Switzerland. We returned to the Hotel Belle-Rive at Bönigen. Our good friends, the proprietors, were still running the place, although it was about to fold up.

We found a local farm girl who agreed to come with us to Zurich as nursemaid, to take care of the children while we were busy at the meeting. Even while we were on the train to Lucerne and eventually to Zurich, we found the clan of mathematicians beginning to gather. Our old friend Emmy Noether—probably the best woman mathematician there has ever been—whom we had known in our Göttingen days, was on the train, looking as always like an energetic and very nearsighted washerwoman. She was, however, a very warm personality, and her many students flocked around her like a clutch of ducklings about a kind, motherly hen.

We took an inexpensive lodging at a Christliches Hospiz on the hills behind Zurich. This was the Swiss equivalent of a YWCA or YMCA hotel. The place was a bit sanctimonious, but the meals were good, the surroundings delightful, and best of all there was a little zoo in the neighborhood where the children could amuse themselves among a collection primarily of baby animals of all sorts.

As is usual at such congresses, we had a very active scientific and social life, with excursions and entertainments, public and private, at which the university and the Federal Institute of Technology put themselves out in a pleasant competition to do their best by us. The place was full of our old friends, as well as of those who were to become our new friends. By this time I had acquired sufficient prestige to be asked to preside at one of the polyglot section meetings. It was not an easy problem to judge a dispute between a quarrelsome Italian speaking very bad French and an equally quarrelsome German with almost no French at all.

On one of the excursions—I believe it was a steamer trip up the lake—a couple of the Italian mathematicians broached to me the subject of an invitation to lecture in Italy. I had no sympathy with Fascism and resented the completely Fascist and official auspices of this invitation. I talked the matter over with Leon Lichtenstein, who was also a participant in the meeting, and he told me to forget the politics and accept the offer. However, I heard nothing more of the offer, and they must have come to the conclusion that my opinions would not go well in Fascist Italy.

Paley was at the meeting, and he told me that he had made arrangements to come to the United States in the fall and work with me. I was a little alarmed, however, at the way in which he radiated British superiority and railed at the shortcomings of the unfortunate Swiss. I took this opportunity to take him down a peg or two, for I felt that it was easier to criticize his excessive boyish nationalism here, on ground, where we were both foreigners, than it would be later in the United States, where I should owe him the courtesy of a host.

At the end of the congress, tired and let down, we took the train across Germany to Hamburg, where we embarked in a North German Lloyd boat. We were eager to get back from what had been a holiday, if not a vacation, and to resume a reasonably routine life.

27 Back Home

1932–1933

We came back to Boston well and happy. Margaret busied herself in finding a new house, but even before we had a chance to get settled there, I had a hurried call from my parents. I was used to these unexplained summonses, but I frankly could not see anything in my recent conduct to account for the emergency nature of the call and the tension that my parents' voices expressed.

I found them in a high dudgeon. Father had received a very insulting letter from a philologist colleague in Germany with whom he had tried to correspond, and both my parents considered that my contacts with German mathematicians, who had not the slightest connection with this philologist, must be interpreted as disloyalty to the family.

My father's feelings for Germany were, of course, ambivalent. He had come to hate the tradition in which he grew up and felt personally rebuffed by the very sources from which he would most have welcomed approval. With my mother, the feeling was partly a transference of my father's emotions and partly a desire to assert her solidarity with the prevailing opinions of the Harvard academic group and thus to underline her ultra-Americanism.

Margaret and I had no real idea of what we might have done to merit the stream of invective which bore down upon us. Moreover, it was not the usual more or less good-natured invective to which we had been accustomed; it had in it a certain ominous stridency which suggested that Father was in a critical state. Something was wrong, something more than a mere ordinary fit of anger.

My parents went out for the evening, leaving us alone in the house. They soon returned in a state of deep alarm. We learned that Father, while crossing the street, had been knocked down by an automobile. He did not seem to be injured seriously, and although one leg was put out of action, it appeared to be more the pain of the bruise that bothered him than any deeper injury. We called the family doctor—an elderly gentleman who belonged to my parents' generation and had the courtly manners which meant so much more to them than the brusque efficiency of the doctors of my own generation. He decided to leave Father at home for the night.

The next day found Father no better, and some twenty-four hours after the accident he was sent to the X-ray room of the Mount Auburn Hospital. There was a fracture of the neck of the femur, and we all knew that Father was in for a bad time. Before much of a surgical nature could be done, it was necessary to allay the pain and to calm his excitement and forebodings of doom.

The drug chosen was paraldehyde, which is generally one of the more innocent sedatives, but in Father's case it proved to have most undesirable reactions. Much of the time he was in delirium. Excited patients are not welcomed in general hospitals, and it was necessary to send my father to a special institution for cases of this sort. By this time, however, a surgeon had set the hip.

Thus, at the very beginning of my return, I was faced by the need of daily visits to my father at the hospital and of further visits to a second hospital to take him out in our car for an airing in the country. Gradually his hip healed and his state of confusion faded away. I took him back to his apartment, from which he ultimately emerged each day to continue his researches at the Harvard Library, but the almost boyish exuberance which had characterized him before the accident was gone, never to return.

It was during this very trying time that I learned that my English colleague, Paley, was arriving to take up a year of work with me on a Commonwealth Fellowship. I crossed the dismal wastes of East Boston to meet him at the pier. There he was, with a couple of big bags which only as sturdy a man as Paley could carry with ease and with another great bundle of skiing equipment.

The ensuing few weeks were for me an alternation between visits to my father and the most active possible joint scientific work with Paley.

Paley had an unlimited admiration for Littlewood, and I imagine that this had been greatly increased by Littlewood's accomplishments as an Alpinist. It was from Littlewood as well as from his own impetuous, untamable nature that he drew the *élan* which enabled him to drive through any problem that he could not turn. He was the leader of the young generation of British mathematicians, and if he had not come to an untimely end he would be the mainstay of British mathematics at the present moment.

Paley and I used to work together on the great blackboard of a dusty, half-lighted, abandoned classroom which had been turned into the lumber room of the M.I.T. Mathematics Department. We decided to continue some work that I had already been doing on the design of electric circuits, and we attacked the problem with every tool in our joint repertory. My role was primarily that of suggesting problems and the broad lines on which they might be attacked, and it was generally left to Paley to draw the strings tight.

He brought me a superb mastery of mathematics as a game and a vast number of tricks that added up to an armament by which almost any problem could be attacked, yet he had almost no sense of the orientation of mathematics among the other sciences. In many problems which we undertook, I saw, as was my habit, a physical and even an engineering application, and my sense of this often determined the images I formed and the tools by which I sought to solve my problems. Paley was eager to learn my ways, as I was eager to learn his, but my applied point of view did not come easily to him, nor I think, did he regard it as fully sportsmanlike. I must have shocked him and my other English friends by my willingness to shoot a mathematical fox if I could not follow it with the hunt.

One interesting problem which we attacked together was that of the conditions restricting the Fourier transform of a function vanishing on the half line. This is a sound mathematical problem on its own merits, and Paley attacked it with vigor, but what helped me and did not help Paley was that it is essentially a problem in electrical engineering. It had been known for many years that there is a certain limitation on the sharpness with which an electric wave filter cuts a frequency band off, but the physicists and engineers had been quite unaware of the deep mathematical grounds for these limitations. In solving what was for Paley a beautiful and difficult chess problem, completely contained within itself, I showed at the same time that the limitations under which the electrical engineers were working were precisely those which prevent the future from influencing the past.

The difference between Paley and me was essentially the same as the difference between a great but traditional English classical scholar and my father. I loved my father, and, for all his severity, I knew his softer sides; but he was no Mr. Chips. What was for a Mr. Chips a subtle and difficult game was for my father a struggle in dead earnest to use one's ideas as tools in the world of life outside. I respect and I understand English scholarship, but my roots are Continental.

Paley was a skiing enthusiast but not a particularly skillful skier. He had the habit of going deliberately over forbidden slopes, and he would stamp his heavy weight on his great long skis in a way that would make the local ski master cock his eye in alarm. In fact, Paley's whole policy of life was to live dangerously. For him, any concession to danger and to self-preservation was a confession of weakness which he dared not make in view of his desire for the integrity of a sportsman.

Paley visited us in New Hampshire. He showed a remarkable lack of understanding of the life there and of the way to get things done. After my experience of his tactlessness in Switzerland, I was not surprised at this. When his car gave him some difficulty on the way up to New Hampshire he had tried to pay the passers-by who had helped us to get going again, even though I told him that these were our neighbors, who were helping us out of friendliness, and who would bitterly resent any attempt to pay them off.

Yet for all this, as he came to learn what life was really like in America and in the American countryside, he began to respect our people for their friendliness and independence. The fact is, Paley remained a schoolboy until his death, and he would have remained a schoolboy still if he had lived to be eighty.

We were to learn later that the front of boldness and danger seeking which Paley put on was part of a true death impulse. At any rate two cousins met as violent ends as Paley himself was later to do. One of them died in a motor smash and the other in a rock climbing accident.

During the Christmas vacation, Paley did a little skiing in the Adirondacks with an Irish friend who, I think, was another Commonwealth Fellow, and after the skiing was over they continued to Montreal. I believe they almost wrecked their car in the Adirondacks, and they got entangled with a group of New York gangsters who had moved to Montreal during prohibition. Paley came home to Boston rather thrilled than chastened. At that time I already knew that the best thing that could happen to him would be some really dangerous and scaring experience—some accident short of death.

In April he went with some Boston friends for a skiing party in the Canadian Rockies. There were safe and interesting ski slopes near where the party was encamped, but the ski master had put some neighboring slopes out of bounds as too likely to have avalanches. To forbid a thing to Paley was to make quite sure that he would do it.

I shortly received a telegram from his companions that he had been killed in a ski accident. He had gone across one of these forbidden slopes at an angle. Apparently he had stamped with his heavy skiis at a place where an avalanche was just ready to start. His body was found a day or two later three thousand feet down the mountain, with one leg torn off. He was buried at Banff, and it was my sad duty to inform his mother and his English friends. It took me some time to come back to a mental equilibrium sufficient to permit my further work and my proper attention to the environment about me.

After this, I turned to the group of my friends centering about the Mexican physicist Manuel Sandoval Vallarta. He introduced me to a Mexican physiologist named Arturo Rosenblueth, the right-hand man of the great Harvard physiologist, Walter Cannon, whom I remembered meeting when I was a boy of about eight. They had worked together in many fields of physiology, in particular neurophysiology. By that time, it was already clear that whatever Arturo's academic fate at Harvard might be, it was his scientific destiny to carry on the great Cannon tradition.

Arturo is a burly, vigorous man of middle height, quick in his actions and in his speech, who paces rapidly up and down the room when he is thinking. No one who sees him in the Mexican environment can doubt that he is a true Mexican, though the greater part of his genetic heritage comes from other countries, particularly Hungary.

Arturo and I hit it off well together from the very beginning, though to hit it off well with Arturo means not that one has no disagreements with him, but rather that one enjoys these disagreements. One point that we shared in common was an intense interest in scientific methodology; another, that we believed that the divisions between the sciences were convenient administrative lines for the apportionment of money and effort, which each working scientist should be willing to cross whenever his studies should appear to demand it. Science, we both felt, should be a collaborative effort.

Arturo carried out at least part of his ideas of what science should be by running a private seminar on scientific method at the Harvard Medical School. Not all those who attended were medical men, however. Manuel Vallarta as well as some other people from M.I.T., including myself, became regular attendants. A few from the branches of Harvard on the Cambridge side of the river also frequently sat in on the meetings. Naturally, it was Arturo who was the chief host in these feasts, which belonged specifically to him, but if there was any other person who took the reins into his hands, I think that by general recognition that person was myself. Thus, the seminar represented some years of our preliminary cooperation before the final and definitive co-operation which has taken place within the last twenty years.

While our seminar was at no time a part of the official teaching of the Harvard Medical School or of any other institution, its many alumni will give it the credit of founding in them a wide interest in the philosophy of science and even of initiating them in quite specific lines of thought and research. Since Arturo has left the Harvard Medical School for Mexico, the two of us, either jointly or singly, have continued similar meetings in Mexico City or at M.I.T. We have managed to recreate much of the atmosphere of those earlier gatherings, although it has perhaps not been possible for us to bring back the full flavor of active enthusiasm which belonged to our earlier days, when our main successes were ahead of us. In its later days, the seminar established a scientific reputation which may have tempted certain eager beavers to try to take over the prestige of the group for their own advantage.

Ultimately the theme of the many discussions which Rosenblueth and I had privately and in our seminar came to be the application of mathematics, and in particular of communication theory, to physiological method. We laid out a policy of joint effort in these fields for a future when we might work closer together.

28 Voices Prophesying War

1933–1935

During my stay in the English Cambridge, the depression had reached its height. When we had come back we found that it had entered a new phase, and that the dangerous possibilities already implicit in it were well on the way to being realized. England had devalued gold at about the time Paley arrived, and he assured me that this was a move of extreme cleverness which would ultimately give England a decisive economic advantage. He told me quite correctly that it was a move that we should be forced to follow ourselves, but that the first nation to act gained an advantage thereby that could never be made up by those who followed suit. It was clear that the world was going to be subjected to shocks which the patched-up economic system that came into being after the First World War, and the patched-up social system that went with it, might not be able to sustain.

When I had at last been in contact with Leon Lichtenstein, at Zurich, I had found him in a depressed mood, owing to the political ground that had been gained by Adolf Hitler and the Nazi party in Germany. He knew that the *Putsch* meant trouble, and trouble was not long in coming. We read in the papers of the anti-Jewish measures, and even as these appeared in the foreign news bulletins Leon sent us a pathetic and desperate letter. He had not waited for the *Putsch* to flee Germany for Poland. He wrote me from a hotel in Zakopane in the High Tatra to find a job for him in the States.

I set out at once to look over the ground, but before I could get started we received another communication—I think from Lichtenstein's wife—that Leon was dead of heart failure. Then I knew that the work of us American mathematicians was cut out for us and that we should have to get together and make a systematic effort to find jobs and a possibility of life for many a displaced scholar.

About this time I received a very tragic letter from Mrs. Szász, the wife of a lovable little Hungarian who had taken me under his wing in my student days in Göttingen. Luckily there was a Mathematical Society meeting not far off, and I had a chance to talk with a Cincinnati colleague, Irving Barnett, the very same man who had introduced me to the theory of generalized integration. Barnett offered to place Szász in Cincinnati in the future, but the Massachusetts Institute of Technology was ready to accept him for a year or two until he could become more proficient in the English language.

Soon Szász turned up, and later he brought his daughter and his brother over to this country. He had a career of some twenty years in America, where he ultimately received recognition more appropriate to his really very considerable talents than he had found in Germany.

Szász was but the first of a great stream of *émigrés,* some of whom went through my hands. I had at least some share in finding positions for Rademacher, Pólya, Szegö, and many others.

Rademacher, of Breslau, was invited to the University of Pennsylvania by Professor J. R. Kline. His field in the analytic theory of numbers was one which had not been too strongly represented in the United States before his arrival; but he formed a school here, and much of his best work has been done in the United States.

Pólya and Szegö are two Hungarians who are particularly strong in the classical field of analysis. Between them they had already written a very interesting sort of textbook which consists in a large repertory of research problems at just about the level at which the novice in research can undertake them with profit. Both of them were received by Leland Stanford University, and Pólya has just retired there.

Emmy Noether, the greatest woman mathematician whom the world has ever seen, was cordially received by Bryn Mawr. Unfortunately, after all too few years she died there from causes presumably unconnected with the Nazi regime.

Menger wrote to me from Vienna to see if he could find a refuge in the States before the Austrian crash came, and we secured him an invitation from Notre Dame University. He has since moved on to the Illinois Institute of Technology.

Von Neumann, who was something of an infant prodigy, received a cordial welcome at Princeton, together with Einstein and Hermann Weyl—who is without doubt the greatest German mathematician since Hilbert. They all later moved to the new Institute for Advanced Study, founded at Princeton with the advice and under the administration of Veblen. Veblen and Kline were perhaps the two chief American almoners for the European refugees, but I am proud to believe that I did at least my share in the matter.

From the very beginning of the problem, I tried to get in contact with Jewish charities and individual Jewish sources of funds for the tremendous task of salvaging as much as possible out of the Nazi mess. Here I found a mixed reception. The Jewish sources of charity had very frequently decided that Jewish displaced scholars were in most cases too far from Judaism to be their special responsibility. Furthermore, it was the time of the height of the Zionist movement, and the Zionists considered that part of their charitable bounty to be spent abroad or on foreigners should go in the first instance to Zionist undertakings, and only in the second instance—if there was a second instance—to other causes.

Even so, we found a considerable amount of Jewish money at our disposal, but I think that at least half of the funds we needed came directly from our already overloaded universities, and from our large scientific fund sources such as the Rockefeller Foundation and the Guggenheim Foundation.

It was about the time of the *émigré* influx that I received an invitation to join the National Academy of Sciences. This is the organization which was entrusted during the Civil War with the task of putting the services of the scientists of the United States at the command of the American government. In the course of the years, its governmental importance had gradually given way to the secondary function of naming those American scientists who might be considered to have arrived. There has always been a great deal of internal politics about science, and this has been distasteful to me. The building of the National Academy was for me a fit symbol of smug pretentiousness, of scholarship in shapely frock coat and striped trousers. After a brief period, during which my inquisitiveness concerning the nature of the high brass of science was amply satisfied, I got out.

It was during the period after my visit to England and before my visit to China that I was perhaps the most active as a participant in Mathematical Society meetings. The very young man cannot afford to go to too many of these. The older man, as I now regretfully find myself to be, does not have the energy to go through too much of the excitement of the meetings and cannot, in fact, attend them without finding claims made on him to act as a presiding officer or to take some part or other in the councils of the group. Moreover, his prestige enables him to let others come to him without the necessity of his going out to meet them. It is therefore the middle period of a man's life which is the most suitable for extensive meeting going.

Among the most delightful meetings were those that usually took place during the spring holidays in the South. To leave half-wintry Boston and cross the cold stretches of Massachusetts and Connecticut, where the merest haze of green foliage was beginning to decorate the bare trees, and to meet in Pennsylvania and Maryland the white of the dogwood and the purple of the Judas tree, was a pleasure indeed. We generally took one of the children along with us, and I think they enjoyed the trips as much as we did.

It was not at one of these delightful spring meetings, but on a bitter Cambridge winter day when the temperature went down to eighteen below and the inadequate steam hissed futilely into the chilly Radcliffe classroom where we held our meeting, that Professor Morse, of the Institute for Advanced Study, and I received the Bôcher prize in analysis together. It was a pleasant thing to be recognized, and even more pleasant to receive this recognition from the mathematician whose opinion I valued above that of all others: Hermann Weyl.

One of the honors that normally comes to the successful American mathematician is a request to write a book for the so-called Colloquium Series. As far as the personal satisfaction of this invitation goes, it is quite equivalent to the award of a prize. The possibility of an invitation of this sort had been hovering about me for some years, and Tamarkin was my most enthusiastic supporter. The invitation came after Paley's death. If he had lived, we would have divided our joint efforts to the writing of this book; and now that he was dead, I felt it to be a work of piety to incorporate into one book the various pieces of research we had done together and the other investigations I had made along similar lines.

The title of the Colloquium was *Fourier Transforms in the Complex Domain,* and it appeared with the two of us as joint authors. I presented the material of the book at the summer meeting of 1933, which was held in the very delightful college town of Williamstown.

At about this time in the academic year of 1933–34 an interesting group of young undergraduates was coming up in the mathematics department at M.I.T. Claude E. Shannon was a very striking member of this group. He hit on an idea which even then showed a profound originality and which has since been justified by the vast amount of work on switching devices, computing machines, and information theory to which it has led.

Let me point out the content of this idea, whose implications are of major importance in current scientific work. All of you are familiar with the wall switches which turn on and off the lights in a room. In their simplest application, each switch operates one light alone. However, more complicated arrangements occur in almost every house. For example, you can turn the hall light on from the foot of the stairs and turn it out from the head. It is possible to find arrangements by which the same light can be turned on and off from four or five different wall switches. It was Shannon's discovery that the mode of designing these switching circuits with greatest economy is in fact a branch of the algebra of logic.

Switching circuits lie at the heart of automatic telephone central exchanges, and Shannon's special talents proved to be exactly what the Bell Telephone Laboratories needed. Shannon has gone on from triumph to triumph as an employee of those laboratories. His interests have come to embrace the general measure of information, the electrical mouse, which can learn its way around a maze; the automatic chess player; the problem of coding and decoding messages; and in fact the entire scope of modern information theory. In all of this he has been true to his first intellectual love for problems of a sharp yes-and-no nature, such as those of the wall switch, in preference to problems which seem to suggest the continuous or roughly continuous flow of electricity.

Thus, Shannon is one of the major spirits behind the present age of the electronic computer and the automatic factory. Moreover, it is through his work that a training in symbolic logic, that most formal of all disciplines, has come to be one recognized mode of introduction into the great complex of scientific work of the Bell Telephone Laboratories.

While Shannon was an M.I.T. man, and while Bush was among the first of our staff to understand him and to value him, Shannon and I had relatively little contact during his stay here as a student. Since then, the two of us have developed along parallel if different directions, and our scientific relations have greatly broadened and deepened.

Two younger men who made their appearance in our department during the early thirties were W. T. Martin and Robert Cameron. Martin left us to head the mathematics department at Syracuse, and his splendid work in building up that department led to our calling him back as head of the M.I.T. mathematics department. His sincerity and understanding friendship with the other members of the staff have been an enormous asset in bringing our department up to its present status. He and Cameron did a good deal of work together along the lines of my Brownian-motion papers, and they organized the field into a generally recognized branch of mathematical work. Cameron has since left us for the University of Minnesota, but for many years he has continued to work together from time to time with Martin.

There were three young men studying mathematics at that time who became more definitely attached to my line of work. These were Norman Levinson, Henry Malin, and Samuel Saslaw. They were Jewish boys who had felt, in differing degrees, the frustration of prejudice without being so deeply wounded by it as to be completely ruined.

Saslaw was the oldest of the three and acted as a good-natured older brother to the group. Levinson was decidedly the strongest mathematician, and he has been one of my successors as a recipient of the Bôcher prize. He is now at M.I.T. and he is a mainstay of the middle generation of research men. Malin filled a very useful part in encouraging the *esprit de corps* which such a group of young men needs to carry it through the difficult work of mathematical research.

Levinson had done research for me at a very early age, and, even before he had completed his bachelor's work, he had taken a job of mine which was already an extension of Fourier Series theory and pushed it to the extreme limit. I felt that the boy had obtained about everything he could from me at that time and that he needed the broadening of contact with other scholars new to him. Hardy was willing to accept him at Cambridge, and we secured for him a Redfield Proctor scholarship from M.I.T. This was in 1934, Levinson's performance at Cambridge was eminently satisfactory, and it was on the basis of the impression that he left that future American mathematicians in Cambridge have been judged and assessed.

By the time at which these young men had come to work with me, Lee, my Chinese friend, and Ikehara, my friend from Japan, were already gone. I have told in an earlier chapter how we learned of Ikehara's desperate straits and had returned him to his own country.

Lee also had gone back to China, on his own initiative, to seek a job there. He had tried both government and commercial work but had found that in neither was it possible for a man to live up to the demands of his own probity. Luckily he had been appointed a professor of electrical engineering at Tsing Hua University, which was just developing from a receiving institution for those boys destined to be Boxer Indemnity scholars in the United States into a full-fledged and autonomous university. Here he was finally in a congenial environment.

Lee had not forgotten me. In the course of the academic year 1934–35 I received an invitation from Tsing Hua to come there for a year and lecture on mathematics and electrical engineering. The invitation came, of course, from the Tsing Hua authorities, and particularly from President Mei and Dean Ku, who was later to become vice-minister of education in China. But the invitation had certainly been inspired by Lee. After a fair amount of negotiation, during which I was always eager to go but had certain qualms about the possibility of going and the stability of affairs in China, I accepted. By then my two daughters were seven and five years old, and we decided to take them along, even though we were somewhat worried about the safety of the trip and its effect on their health.

My prospective trip to China filled me with great enthusiasm. Not only have I always loved travel for its own sake, but my father had brought me up to consider the intellectual world a whole and each country, however exalted its position might be, as a mere province in that world. I had actually been a witness and a participant in the rise of American science from a provincial reflection of that of Europe into a relatively important and autonomous position, and I was sure that what had happened here could happen in any country, or at least in any country which had already shown in action an aptitude for intellectual and cultural innovation. I have never felt the advantage of European culture over any of the great cultures of the Orient as anything more than a temporary episode in history, and I was eager to see these extra-European countries with my own eyes and to observe their modes of life and thought by direct inspection. In this I was thoroughly seconded by my wife, to whom national and racial prejudice have always been as foreign as they have been to me. Even my daughters, small children as they were, had been brought up outside of the usual run of prejudices.

We spent the beginning of the summer as usual in our home in Sandwich, and I participated in hikes with my young friends, who were now beginning to grow up. We took the train from Meredith, our usual jumping-off place, and went north by way of Montreal to Chicago. We left for California, where I had accepted invitations from Stanford University for a few lectures. There we saw our friends the Szegös and the Pólyas, together with other even older acquaintances. We left for Japan by a boat of the Dollar line.

29 China and Around the World

On the boat for Japan we found ourselves in the company of a mixed group of returning missionaries, Japanese army officers, and conducted tourists, but with two young children with us we led a very self-centered life on board.

At Yokohama we were met by our friend Ikehara, who was teaching at the University of Osaka, and he served as our mentor for the two weeks we were to spend in Japan.

Rooms were waiting for us at the Imperial Hotel in Tokyo, that fantastic structure of the American architect Frank Lloyd Wright, and Ikehara had made arrangements that a Japanese lady who had been in America should invite our children. Lo and behold! This lady had lived in Boston, where we had known her well, for her daughter had been a friend of Barbara's at kindergarten. Thus, my children got along splendidly with their hostess, and we had no difficulties at all in this respect.

The hotel was excellent and the food very good, but in those days the guests were subject to a continuous surveillance. For all we knew, the American lady who sold dolls and souvenirs might have been under orders to report our every word or attitude to the management and eventually to the police. Perhaps the waiters were a little more than waiters. Still, we had a very good time, and Margaret enjoyed shopping for Japanese souvenirs.

During the next few days, in hot August weather, I met the Tokyo university people and gave a few talks. I found the academic standard at the universities quite high, but I also felt that Tokyo in those days had begun to be affected by the rigidity that so often taints a university which is sure of its position as the first in the land. The Tokyo professors looked slightly down their noses at their associates at the lesser universities.

Ikehara accompanied us to Osaka, which was even muggier than Tokyo. I found the mathematics club at Osaka University very much to my liking. It is from this group that many of the best Japanese mathematicians have come, such as Yoshida and Kakutani, who stand among the best mathematicians anywhere.

We visited the great keep of Osaka. There is no European building more suggestive of strength and military impregnability than this cyclopean structure. The battered-back massive stone walls could even now hold an army at bay. It seemed to us a comment in stone on the old Japan of the *bushi* and *ronin*.

We left Kobe for the China coast by a small Japanese steamer. It was a sail of several days through the beautiful inland sea of Japan and then past the forbidding and bare hills of Korea, to the starboard, and Shantung, to the port. The food was dull and inferior, and the company more interesting as types than as companions.

After a few days we saw fishing boats which had put out from the low muddy coast, and soon we docked at the pier by the railway station at Tangku. We were surprised to see how much taller the Chinese porters on the shore were than the run of the Japanese we had met, even though we had seen a few tall and even burly Japanese here and there.

Soon the Chinese custom house officer came on board, inquired courteously for us, and told us that Dr. Lee was waiting for us on shore. Within a few minutes Lee met us and we were sitting together in the great, empty, waiting room of the railway station, which was adorned with pictures of the summer resort at Pehtaiho. Lee told us that he was married, but he would not tell us from what part of China his wife came. He immediately started discussing with me both the work we had already done and our prospects of work to come.

Soon the train came in, and we entered a second-class compartment for Peiping. The train had cars which looked American from the outside, although they followed the European compartment system. Instead of a water cooler there was a little stove at one end of the car, where the attendant kept a pot of tea always on the boil for the passengers. Indeed, we found that boiling tea is as much a necessity of life in China as cold water is with us.

We traveled through flat country reminiscent of the cornfields of Kansas, where we saw corn as well as the tall Chinese sorghum. The farm houses were of adobe with somewhat less curved roofs than we had been led to expect from the pictures we had seen of southern China. Soon the barren Western Hills came into sight, and before we knew it we were at the main railway station of Peiping. A seven-miles taxi trip took us to the South Compound. This was a large aggregation of modern bungalows which, however, instead of facing one another along streets of the Western type, all faced to the south and looked into one another's kitchen compounds.

There we found Mrs. Lee, who was not a Chinese lady at all but a tall beautiful Canadian, who had been working in New York for the same firm as Dr. Lee himself, and for whom Dr. Lee had sent once he was established in a permanent position. She was in charge of the Lee household of servants, who all appeared at our house to enable us to get started, and who had a delicious meal ready for us.

It took us a few days to get used to Tsing Hua and our new life. It was a bilingual place, where much of the teaching both in the Western humanities and in science was in English. Although there were some Western professors on the faculty, the majority were Chinese, trained for the most part in the United States but some of them in England, Germany, and France, as well.

It was interesting to see how this foreign training had reacted on the members of the faculty. There was a Chinese lady who had studied in Paris and whose walk looked French even at the distance of a couple of blocks. There was an assertive little German-trained professor—nothing but minor details of complexion distinguished his appearance from that of the perfect Nazi. Many of the professors were as American as my colleagues at home, and there was a tweedy professor of English who had Oxford written all over him, and inside his soul as well.

Professor Hiong, the head of the mathematics department at Tsing Hua, whom I had already met in France, was as much at home in Paris as on the edge of the Western Hills, and his eleven-year-old boy was an artist in the Western style. The boy was able to write with ease a French composition for a *lycée* in Paris or a discussion of Confucian virtue in classical Chinese.

The first things we had to attend to concerned the bread-and-butter necessities of life. The Lees found for us an establishment of servants. There was an elderly butler, or number-one boy, who had worked for people at the French Embassy. He was supposed to know a little French, which consisted mostly of the word *oui*. There was an amah, or maid; an elderly Mohammedan rickshaw boy to take our children to the American school at Yenching University; and a cook (or "great expert," to translate the Chinese expression). He did not live up to his name, but he certainly could produce acceptable meals.

One of the most important things to find was a Chinese teacher to come to our house and instruct us in the language. Lee found for us a tall, dignified, elderly gentlemen in a long gown, who knew little English and spoke none with his charges, but who brought with him a textbook in English with the aid of which he could supplement his oral lessons. We did not find progress too slow, and both my wife and I took a great deal of interest in learning a language so new and unfamiliar to us. Our teacher proved to be more than a teacher, for he was the vehicle of all the local gossip both to and from our house. We always knew if one neighbor had an extra big banquet on for the night, or if another neighbor had servant troubles, or if the Christian congregation was to meet that Sunday in the home of a third.

We sent the children to an elementary school carried on under the auspices of nearby Yenching University. Before school had started, they had managed to make friends with the Ni children, who lived nearby and whose mother was American. The two broods raided the neighbors' vegetable gardens, invaded the privacy of the neighbors' houses, and, in short, got into all the sorts of trouble into which active children can get.

The American school at Yenching was a one-room structure containing all grades from the first up to the high school. The teachers came mostly from the faculty of Yenching University. All the children except mine were so fluent in Chinese that my own made very little effort to bridge the gap between their attainments and those of their schoolmates.

Occasionally my children would be greeted with abusive cries from the village children, with the result that my daughters learned Chinese abuse with an alacrity with which we could have well dispensed. They hurled back the cries of the village children without understanding what they meant. When my daughter asked the meaning of one of these phrases, the Chinese teacher stood her in a corner.

The school was very good for my children. The informal arrangement made it possible for Peggy to advance far more quickly than in any graded school. She was the only beginner, but before the year was half over she had really started to read.

It was quite early in our stay at Tsing Hua that Mr. and Mrs. Hiong were hosts to the whole mathematics department for a picnic on the grounds of the New Summer Palace. This was a rococo assemblage of passageways and temples. At every turn there were windows of fantastic shape in the form of vases, lutes, hearts, or lozenges. There were long winding walks and gingko trees, and staircase upon staircase running up the hills to little summerhouses.

Mrs. Hiong had prepared a collection of tidbits in Chinese style. The mathematics department servant carried the basket and participated as an equal in the feast. The Hiong boys made sketches of passers-by in which they showed a sense of humor at once Chinese and Western.

Everybody could talk English, and all but ourselves talked Chinese. The atmosphere was one of democracy and free interplay of personality with personality. The servants were neither actively snubbed nor tacitly left out of the conversation.

We had later invitations to the Hiongs' house, which was almost a museum of delightful paintings in the modern Chinese style. There were many panels covered with delicate and deft representations of fish and shrimps and crabs and all the other little animals of the water. The Chinese, like their pupils, the Japanese, show a tenderness and taste which is never cloyed with the sentimentalism that spoils so many Western nature paintings. To the Buddhist, God is not separate from nature but is manifest in all nature. Thus, nature contains God himself as a part of its very being and not as part of a quasi-human personality imposed on the world from without.

To go from Chinese painting to Chinese banquets is a short step, for cookery is quite as much a Chinese art as painting. For me, a vegetarian, there was a wealth of dishes—and indeed an entire school of cookery—especially concocted for the vegetarian Chinese Buddhist monks. Yet even my taste for Chinese food scarcely mitigated for us the feasts of twenty or thirty courses. I am afraid that the cookery we asked for at home shocked the professional sensibilities of our "great expert," for we preferred plebeian food to the dainties which gave him a chance to display his skill.

My lectures at Tsing Hua were in English, which all the students could easily understand. More than one of these students later turned to pure mathematics or to electrical engineering. They are now scattered around the universities of China, and of the United States as well. In my spare hours around the classroom building I used to sip the tea that always was kept prepared by the department servant and play innumerable games of chess or five-in-a-row or go with my colleagues. I never mastered the game of go, however, and would now prove a mere child at Princeton among the experts of Fine and Fuld Halls.

I continued to work with Dr. Lee on problems of electric-circuit design. We attempted to do some laboratory work, which came to nothing because it involved technical problems which we were not then in a position to handle adequately.

What Lee and I had really tried to do was to follow in the footsteps of Bush in making an analogy-computing machine, but to gear it to the high speed of electrical circuits instead of to the much lower one of mechanical shafts and integrators. The principle was sound enough, and in fact has been followed out by other people later. What was lacking in our work was a thorough understanding of the problems of designing an apparatus in which part of the output motion is fed back again to the beginning of the process as a new input. This sort of apparatus we shall know here and later as a feedback mechanism.

Feedback mechanisms were already used by Bush in his computing machines, but they have certain serious dangers of their own. Too intense a feedback will make a machine oscillate so that it cannot come into equilibrium. It is not too difficult to avoid this with the relatively light feedbacks used in the machine of the Bush type, but with the heavier feedbacks of the purely electrical machines this difficulty is hard to overcome. What I should have done was to attack the problem from the beginning and develop on my own initiative a fairly comprehensive theory of feedback mechanisms. I did not do this at the time, and failure was the consequence.

My main work, however, was lecturing on generalized harmonic analysis and on the material Paley and I had included in our book. I also was engaged in new pure-mathematics research on the so-called problem of quasi-analytic functions.

In the last hundred and fifty years, analysis, which represents the modern extension of the infinitesimal calculus, has split into two main parts. On the one hand we have what is known as the series of functions of a complex variable, which is a continuation of the eighteenth-century theory of series proceeding in the powers of a variable such as 1, x, x^2, etc. This theory is particularly applicable to quantities which change smoothly and gradually. At one time it was supposed that all important mathematical quantities changed smoothly and gradually; but toward the end of the eighteenth century, the study of harmonic analysis, which is analysis of vibrating systems, showed that curves pieced together out of parts which had nothing to do with one another were subject to an analysis of their own. This point of view first led to the theory of Fourier series and then to the general branch of study known as the theory of functions of a real variable.

The theory of functions of a real variable and the theory of functions of a complex variable thus represent two separate but related subjects, which do not succeed one another like a freshman and a sophomore course but represent radically different insights into the nature of quantity and of the dependency of quantities upon one another. They have had a great deal of interplay in the course of the last hundred and fifty years. However, it is only recently that it has come to the attention of mathematicians that there are certain intermediate fields of work which share the methodology of both. There are certain curves which are smooth enough so that the whole course of these curves is known from any one part, but which are not smooth enough to be treated by the classical theory of the functions of a complex variable. The study of these curves is known as the theory of quasi-analytic functions. The French school of mathematicians has contributed greatly to this field, and here some of the best work is that of Szolem Mandelbrojt. However, the book by Paley and myself in this field has also led to results which I was pursuing during my stay in China.

I had hoped to meet Mandelbrojt sometime soon—as, for example, at the scientific congress to take place in Oslo in 1936. When I heard that Hadamard, Mandelbrojt's patron, was also coming to Tsing Hua University, I hoped that he could arrange for a meeting between us in France while I was on my way to the congress.

I have already pointed out that my work with Paley had direct applications to the study of electric circuits. These were related to the same issues which also came up in the theory of quasi-analytic functions. Thus, here again, as so often in my work, the motivation which has led me to the study of a practical problem has also induced me to go into one of the most abstract branches of pure mathematics.

This represents an attitude of mine which is in harmony with the greater part of the work of the eighteenth and nineteenth centuries, and which since then has found its representation in the writings of Hilbert and Poincaré. It is not the prevailing attitude at the present time either in America or elsewhere. The two mathematicians now or recently active in America who have adopted a similar point of view areand I believe not by coincidence—two of the greatest forces in modern mathematics, namely, Hermann Weyl and John von Neumann.

This mathematical work on quasi-analytic functions was, of course, not the whole of my life in Peiping but was interspersed with periods when my chief interest was to behold the rich and unfamiliar panorama of the life about me.

We used to go to the city quite often by bus or taxi or even rickshaw. Leaving for the city and returning by rickshaw was an interesting process, though one felt ashamed at being pulled along by the strength of another man. At the northwest gate of the city there was a teahouse which was a rickshaw man's exchange. The guild of rickshaw pullers outside the walls was separate from that functioning within the walls, and no man from inside the city was permitted to ply his trade further out than this teahouse. Thus the rickshaw boy who took you to the teahouse in either direction made a bargain with his opposite number from the other guild and was paid by the new man for the work he had already done. Later on the passenger payed the new rickshaw man what was owing to both.

When we first went into the city we were beset by a gang of rickshaw men, who almost tore us to pieces in their eagerness to make us their employers. Acting on Lee's advice, Margaret and I each picked one, and we had no further difficulty in this respect. Our steady rickshaw man was almost always ready for us when we came in and got off the bus, and if he was not there he would have assigned somebody else to take his place. These bargains were generally respected by the whole guild.

When we dined in town, my wife would send what was left on the serving dishes down to the rickshaw man for his meal. My rickshaw man was a Moslem and would not accept non-Moslem food, so I would pay him off with a small extra sum for his lunch. Both our rickshaw men were extremely loyal to us, particularly after Margaret's man had damaged his eye in a fight with another rickshaw man and she had sent him a sum of money to help him take care of himself while convalescing. When we finally left Peiping he gave my children Chinese hats to wear, and my rickshaw man gave us a can of fine tea scented with jasmine.

My rickshaw man was both intelligent and enterprising. He read his newspaper, and later in the year when Professor Hadamard came to Tsing Hua University, my puller asked me whether it was true that Ha-Ta-Ma Hsien-Sheng was as great a mathematician as the newspaper said he was. I have no doubt that part of my rickshaw boy's interest lay in trying to get a permanent job with Hadamard, but at least he was both as enterprising and as well-informed as one would expect a very intelligent cab driver to be in the States.

In town, Margaret would shop at the Clock Shop, the Big Bell Shop, and at many other such places. In these higher-class Chinese shops the service was often excellent and even modem, but the décor and the costumes were thoroughly Chinese. The subordinate clerks wore the cotton gown of their class, and the proprietor and his immediate underlings were dressed in gowns of gray or blue silk damask. The language of the shops was either good English or good Chinese. Pidgin English was almost unknown in Peiping. There was a great deal of snobbery and mutual belittlement between Shanghai, the New York, and Peiping, the Boston of China. The Shanghai ladies were quite as eager to keep in the latest style as the ladies of New York, and their Chinese-cut gowns often showed bold and attractive adaptations of Western fashions. The Peiping ladies, on the other hand, dressed with some of the aristocratic intellectual dowdiness which I have seen on the banks of the Charles.

Peiping was an ancient capital with a long tradition of art and culture. Here and there one would see simple farmers of Manchu blood, the descendants of the courtiers of the Ching dynasty, who spoke the beautiful Chinese of the palace.

The city was a mixture of glamor and squalor. It was intriguing to walk down ill-paved alleys which seemed to lead from one slum to another, but where the vermilion moon-gates often opened into a little gem of a courtyard and garden surrounded by pavilions of taste and beauty.

My friends were both American and Chinese. So far as concerned the serious intellectuals on both sides, the relations between the two groups were excellent. On the other hand, the rag, tag, and bobtail of Englishmen abroad, which Thackeray knew so well in the Paris of the Restoration, were to be found in 1935 throughout the treaty ports of China.

The Japanese were moving in. They had already taken over the administration of most of Pei-ho Province, either directly or by satellites. The town was full of Japanese soldiers whose drill would carry them brutally into the midst of a Chinese crowd.

I went down with Lee to Tientsin to collect part of the pay for our inventions from an agent of the American Telephone and Telegraph Company. Tientsin was interesting. With its several foreign concessions, one would go in one block from Russia to France, and in another from France to England. There was something unreal in this finely subdivided macédoine of different nationalities.

The student life was greatly disorganized. The students put on a strike that lasted for months. They marched into the city to protest the coming of the Japanese and the supineness of the Chinese. When the students were not allowed in at the railroad gate, a little wisp of a girl rolled under it and opened it. Inside the city, the police force used clubs and fire hoses on the students, and the hospitals and jails were full. However, many of the students came from the leading Peiping families, so their parents found a way to compromise.

All this time Lee and I spent in work. We would sit over the drafting board in Lee's den while our wives would talk and read in the next room. After they thought we had had a sufficient dose of work, they would call us out for a snack and a cup of tea, and finally we would finish the day with a game of bridge.

One of our big problems was to get our patent application through the American Patent Office. For this we had only one course to follow: to work together with the consular secretary of the American Embassy. In his book of instructions, there were directions as to how to take care of the proper documents and drawings, but our diplomatic friend knew nothing of engineering or of patent requirements, and Dr. Lee and I had to interpret his own books to him.

We learned to know the embassy people very well and were genuinely pleased by the quality of the attachés we found there. The requirement of knowing something specific and difficult, such as an oriental language, definitely weeds out the playboys and the incompetents in the diplomatic service. Indeed, I think that this service has always been at its best on its periphery.

For a long time, the frost held off, but at last the ponds and ditches froze. Notwithstanding the short winter, there were days quite as severe as any in Boston. It was too dry for snow, and the favorite sport of the good Peiping citizen was to go skating on Japanese-made skates on the ponds in the Forbidden City. It was quite a sight to see a sparsely-bearded elderly gentleman in a long fur-lined gown glide over the lake, doing a figure or two.

Of course, the working people, both men and women, wore the short gown and trousers, thin in summer and wadded in winter. The poor students and people of sedentary life but of humble status wore the long blue or gray cotton gown. The modern men of the university wore European costume in spring and in autumn, but in the summer they preferred the greater coolness of the thin silk gown, often worn over European trousers and shoes. In the winter, the chillness of the corridors and classrooms made the fur-lined gown almost a necessity.

My daughter Peggy had another attack of the middle-ear infection which had so often made her a victim in her childhood. There was nothing to do but to send her to the American hospital in town and leave her there with a very competent Chinese ear doctor and excellent Chinese nurses.

Meanwhile we had a chance to appraise the faithfulness of our number-one boy. It was Chinese New Year's, and of course we had given all of the servants the day off, as one would at Christmas at home. The number-one boy refused to take the day off because a member of the household was sick, even though that member was under adequate care seven miles away and he could do nothing about it. I think he liked us, but I think it was even more a sense of honor and professional duty which constrained him to this course. He was very angry at the maid for taking the day off and never quite forgave her, which caused an eruption of the quarrel, traditional in China, between butlers and maids.

Among my Chinese friends were scientific skeptics in the modern sense. Some were Christians, but almost none were Taoists or Buddhists of any devoutness. Yet there was common to almost all that love of the whole world rather than of any specific humanity, which is so characteristic of

Buddhism. Equally characteristic of China is the light, epicurean play of thought which has attached itself to the rather quaint and formless body of Taoist legends.

All the good Chinese I have known carried on the Confucian traditions, and they were not the less Confucians when they were Christians; for the Chinese have a religious tradition of syncretism, and for them the appreciation of one religion does not mean the rejection of others. Behind all Chinese who respect any religious tradition whatever is the Confucian concept of the gentleman-scholar-statesman: of a rather austere and courteous personality, tempered by a sense of humor, with the welfare of the community as his goal and dignified scholarship as his means.

There are many ways to be evil, but there are also many sources from which a good life may spring. The Confucian character is a very interesting and tempting source of the good life, and few of the more sensitive and intelligent missionaries have been able to return from China without a deep understanding and participation in the Confucian point of view. China converts its converters.

Even those Christian Chinese who are fully aware of the fineness of the better missionary and of the manifold things he has done for education, medical help, and social improvement do not welcome the missionary as an institution. Their ideas of a Christian church in China is that of a Chinese Christian church, rooted in the people, with a Chinese clergy. They resent the protected extraterritorial position which the missionary has held until recently, and they resent equally the tendency of the conventional missionary as an institution to talk down from above.

I think that Yenching was one of the finest monuments of an enlightened missionary movement, which was already departing from the purely missionary standpoint and taking on that of a native Chinese Christianity.

There was a broad and durable companionship between many of the Westerners and many of the Chinese. Yet the Chinese all felt strongly that they should be masters in their own house and that this transfer of authority should involve no avoidable delay.

Even those Westerners who loved China and wished to help it were appalled by the impossibility of raising the Chinese standard of living to a Western level without making its price beyond anything which a bankrupt country could afford. Studies made at the Peiping Union Medical College had shown that the pitifully inadequate diet of the Chinese peasant, which was shown in the lean stomachs and the spiderlike limbs of the rickshaw boys and the peasants, could not be improved into something more nourishing at any price which the peasants could pay.

Corruption and graft, which seemed an irremovable part of Chinese life, were not to be put aside by any arrangement which appeared practicable during the time of transition. If you destroyed the customs by which the Chinese favored his family and his clan against the demands of political and business honesty, millions of people would have been doomed to starve before any new order could come to replace the old. It is quite understandable why the Chinese should look longingly toward any shortcut to modernization, industrialization, and a higher standard of living which might be offered them.

We went on immersing ourselves deeper and deeper in the life in China, and sometimes were a little eager for variation in the small circle of our companions. My friend Professor Hadamard, of Paris, came at the beginning of the second term. He was installed near us in the Old South Compound, but he soon moved to an apartment in the city in or near the legation quarter. The Hadamards were much happier in this livelier environment. Professor Hadamard was already well along in years, and the discomforts of the isolated life on the grounds of the university rather terrified him. It was a pleasure to see him again. He was a great mine of reminiscence of the good old days of French mathematics. His wife was also a mine of anecdotes concerning French academic life, and she had known Pasteur when she was a child.

Hadamard told us a delightful story about his own youth, when he felt a certain fear of disfavor from his more conservative colleagues because of his kinship with the wife of Colonel Dreyfus. With the Dreyfus affair exciting all France to great emotional heights, everyone was either an ardent Dreyfusard or an ardent anti-Dreyfusard. Among the latter was the great mathematician Hermite, who was to examine the young Hadamard for his doctorate. Hadamard approached this occasion with fear and trembling, and this embarrassment was not relieved when the old gentleman said to him, "M. Hadamard, you are a traitor!" Hadamard mumbled something in confusion, and Hermite went on to say, "You have deserted geometry for analysis."

We used to go to town to visit the Hadamards and sometimes Margaret and I, or Lee and the two of us, used to go down into the tangled, squalid streets of the so-called Chinese city (as opposed to the rectangular Tatar city) to rummage in the antique shops. There we would often come across ancestor portraits which show dignified Chinese gentlemen or ladies, in stiff poses, with hands on the knees, dressed in marvelous silken gowns, which for the men were robes of office, civil or military. For all their pomp and stiffness, it was common for the faces in these pictures to be of a remarkable fineness, humor, and sensitivity.

We found one such ancestor portrait which was so like Professor Hadamard himself, with his somewhat sparse, stringy beard, his hooked nose, and his fine, sensitive features, that it would have been completely adequate to identify him and to pick him out of a large assembly of people. There was, it is true, a very slight slant to the eyes, and a very slight sallowness of complexion, but not enough to confuse the identification. We bought this picture and gave it to its likeness. He appreciated it very much, but I don't think that Mme. Hadamard cared for it. She did not wish to think of her husband as a magot: the conventional nodding figure of a mandarin which one finds in French tea shops, and which occupies a conspicuous position as the emblem of the famous Café des Deux Magots.

At any rate, in their later wanderings the Hadamards managed to lose or to misplace this portrait, and thus my more recent visits to the Hadamards in New York and in Paris have not enabled me to verify again from direct observation the resemblance between Hadamard and the Chinese picture.

As I have said earlier, I had long wished to meet Mandelbrojt face to face and to discuss with him the relation of our researches. Hadamard told me that he too was eager to talk these matters over with me. This was all the more important as he had taken an active part in the organization of the International Mathematical Congress at Oslo, which I was to attend that summer. Hadamard wrote to Mandelbrojt and made arrangements for me to visit him on my trip to Oslo and possibly put in a few days of joint work with him.

This habit of joint work is almost the peculiar property of mathematicians and mathematical physicists. Most other scientists are hampered by the fact that they are dependent not only on a laboratory but on a very special laboratory with their own materials and equipment. Historical and philological workers usually find themselves in so controversial a field that a joint paper would scarcely be possible, unless by an extraordinary accident they should happen to share not merely the same general views but even the same detailed opinions. In the arts such as literature and music there is not enough common ground to enable a group of artists to achieve together that unity of personal point of view which is essential for true creative work. Mathematics, however, for all the real, personal individuality of point of view which characterizes the aesthetic side of that discipline, is sufficiently factual for collaboration to be possible and for differences of opinion to be judged and dissolved on a non-personal basis.

Eventually the time began to draw near for us to leave China, and we began to consider how we should do so. My main motive for going home by way of Europe was that I wished to participate in the Congress in Oslo. We had thought of going by the Trans-Siberian Railroad. I took council with the officials of the Soviet Embassy to see if it would be possible. They said it was, but Margaret and I eventually came to the conclusion that the two weeks' trip on a train would be too hard on the children and we decided to take the longer sea trip by the Indian Ocean. We found a Japanese boat which was nominally first class but actually second and was not too expensive. We made plans to embark in Shanghai.

We took the train from Peiping, accompanied by Dean Ku, of the Tsing-Hua Engineering School, and a Chinese friend of his. The fact that the cars were divided up in European fashion made it much easier to take care of the children. The dining car offered us a choice of two styles of meals, Western and Chinese, and we unhesitatingly chose to eat Chinese. The next evening we arrived at the train ferry on the north bank of the river opposite Nanking.

Ku and I left my family on the train and took the swifter passenger ferry to the city, where we drove over to the house of my old friend Y. R. Chao and found not only a family party but a number of interesting officials from the university and some officials of the government. The two American-born Chao girls were now making their appearance as individuals, and there were two younger daughters, who had been born in China.

In a few days our boat was in—the *Haruna Maru* of the Nippon Yusen Kaisha. The captain was a very amiable gentleman who spoke excellent English and who, I believe, had spent some time in America. He was very solicitous for our interest. Indeed, although the ship was small and crowded and not particularly well equipped for the tropical service in which it found itself, the spirit on board was very pleasant and we rather enjoyed the trip. We found ourselves immersed for six weeks in a world of which we had heard only through the writings of Somerset Maugham.

One new passenger of great interest came aboard at Hong Kong. It was Father Renou, a French Lazarist missionary priest from the interior of Yunnan, who was on his way back to the headquarters of his order in France to report at first hand on the needs of the China service. We found him a charming and intelligent companion. He felt that there had been sent out too many priests who were French peasant boys full of good will and sanctity but ill-trained to compete with even the fragmentary scientific learning of the Chinese village school teacher or to fulfill the magistrative duties which the treaties had conferred upon them.

Politically, Renou was a liberal who had very little sympathy with the Fascist reaction which had become so strong in Italy and had met with a certain sympathy among a sector of the clergy. He was a historian, and was greatly interested in the way the Church had lost a magnificent opportunity to embrace the whole of China toward the end of the seventeenth century. This was, he told me, the Affair of the Rites, when a great many Chinese would have agreed to embrace Catholicism if only a Christian interpretation could have been found for their traditional ceremonies.

Professor Fujiwara, of Sendai, was on the boat, bound as we were for the International Congress at Oslo. There was also a Chinese lady on her way to marry her fiancé, at the embassy at London. The Chinese lady was very shy and almost a myth to us on board, but Father Renou tried to help her in her loneliness by having Margaret come down and talk Chinese with her. Apparently the lady had received instructions not to confide in anybody until the boat landed, and all my wife's overtures were in vain. However, after I had left the boat at Marseille and they had passed Gibraltar, the lady had to come on deck to fill out the documents necessary for landing. There she attached herself to Margaret and remained close by her side until the boat landed and her betrothed appeared to take her off.

We played a great deal of bridge on board, and I showed myself to be a habitual overbidder, thus incurring the wrath of some of the crustier experts. I also played some chess. Here I initiated into European chess one of the Japanese officers who was manifestly an expert at the very different chess of his own country, in which the pieces are wedge-shaped and their direction rather than their color indicates the side to which they belong. It is worth noticing that in Japanese chess, captured pieces may be used by the captor—a fact which may not be without importance in considering the Korean War. How else can we interpret the use of the Korean prisoners of war as pieces still in action against us, or as our prisoners of war as pieces to be turned to the disadvantage of our enemies? At any rate, my Japanese friend learned Western chess in two or three sittings and proceeded to demolish me with an absolute consistency.

Beside these amusements, I made a first attempt at writing a novel, based on some of my own experiences in the academic world and on some characters I had known. The novel was very amateurish, but it helped fill in the interstices of boredom in the long trip. In addition, I got the practice of at least trying to write on fictional and human material, and this practice has stood me in good stead ever since.

While the boat was tied up in the Suez Canal, Margaret and I made a brief trip to Cairo. In a few hours after our return to Port Said, we were drinking in the cool breezes of the Mediterranean. The next day we passed through the Strait of Messina, between Sicily and the mainland of Italy, the home of the magic Fata Morgana.

We emerged into the Tyrrhenian Sea, and soon the active volcano of Stromboli loomed before us. This was the captain's first trip to the Mediterranean and he was as interested as any of us at seeing the sights, so he had the kindness to swing the ship all around the mountainous island before continuing on his course to Marseille.

I debarked at Marseille alone, leaving the family to continue the boat trip by way of Gibraltar and the Bay of Biscay to London. I took the night train to Clermont-Ferrand. Mandelbrojt met me at five in the morning.

He was exceedingly friendly and cordial, even though this was our first meeting. He was a Polish Jew who had come to study and live in Paris and had attracted the attention of dear old Hadamard. Hadamard's accolade was enough to make a young mathematician, and Mandelbrojt was well launched on a fine career. We put in four days of hard work on a joint paper which we were later to present at the Oslo congress.

There was a general strike in France at the time, and Mandelbrojt was, on the whole, sympathetic with the strikers. Nothing happened to molest us. I lived at a small hotel but passed the day in Mandelbrojt's house, working with him and dining with his family. Mandelbrojt took me around to see the sights. Finally I left for England a few days ahead of the arrival of my family.

In London I looked up the Haldanes, who were now living in an interesting quarter of town near Regent's Park and the Zoological Gardens, in which Haldane took a great interest. Mrs. Haldane welcomed me cordially, but I could not see the family that evening as they were giving a party for H. G. Wells. However, the next day I visited them, and Haldane showed me some of the particularly interesting specimens at the zoo which were not generally shown to the public.

In a couple of days I went down to the docks to pick up my family. We began at once to plan how we should settle the children for the duration of our visit to Oslo. We found a sort of vacation school at Bexhill, on the south coast, where we left them in good hands for a few weeks.

Margaret and I took the rough and fatiguing North Sea trip to Denmark and spent a day or two in Copenhagen visiting friends. The city had become much more a place of rush and of automobiles than we had found it on our last visit, and its quaint and homelike flavor, though it still remained, was now considerably diluted. We took the train for Oslo, crossing the sound by train ferry from beside the jutting castle of Elsinore to the Swedish city of Hälsingborg. We found many American and European mathematical friends on the train.

The southern part of Sweden, through which the train ran, made me think very much of the Maine coast. Even the houses were wooden. The rocks themselves had the familiar rounded, glaciated appearance we so well knew at home, and we felt as familiar with the country as if we had been there before.

Our Danish stood us in good stead and enabled us to converse even with our Swedish fellow passengers. When we crossed into Norway the language difference was still smaller, and at no period did we feel the helplessness of the tongue-tied monoglot.

The meeting was delightfully arranged, and as exciting and overstimulating as such meetings always are. We found ourselves among a host of friends of all nationalities. We joined up particularly with our colleagues the Vallartas (for Vallarta had married since our early days, and Mrs. Vallarta became a good friend of my wife). Canon Lemaître, of Louvain, also belonged to our group. He had worked for years at the Harvard Observatory. We found him in Oslo, as we had known him before, a delightful friend and table companion. We would go with him on excursions and walks during the twilit midnight of an Oslo summer.

Margaret left after the congress to see her kinsfolk in Germany. I went back to England, for I had planned to spend a couple of weeks with the Haldanes in the charming county of Wiltshire. The Haldanes had found a delightful old stone house among the rounded hills and steep valleys of that region. Haldane had ascertained that his wife loved the downs of Sussex, but the downs of Sussex had become too expensive for a university professor. He took the geographical map of England and followed the chalk formation of these downs around until he came to a somewhat less popular region where he could count on a similar landscape. The rest was easy, and the Haldanes had settled in a country of delightful walks and views.

Both Haldane and myself were grieved and depressed at the new blow to freedom in Spain. Later on, Haldane offered his services to the Spanish republic and left to fight there. In Spain he was appalled by the bumbling inefficiency which accompanied the good will of most of the anti-Franco parties, and more and more he began to lean toward the Communists. According to him, they at least had a purpose and a policy.

The English Communists soon recognized in him one of their greatest assets. He continued to have an editorial post on the *Daily Worker* until his break with Communism on the basis of the dogmatic biology of Lysenko and the Czechoslovak trials.

However, this political rapprochement with Communism was yet to be. For the most part our talks on our long tramps about the bare-topped hills were scientific or literary. I showed him the fragment of the novel that I had already begun to write, and he showed me the manuscript of a series of children's stories which he was soon to publish under the title *My Friend Mr. Leakey.*

Looking back on my China trip and on the European visits which ensued, I see now how much progress I had made since my earlier years at M.I.T. I had a successful family life even if I was not the easiest of husbands and fathers, and Margaret and I now had a large stock of common experiences to enjoy together. My children were more than babies and had begun to be companions. They were starting their lives with the enormous moral advantage of seeing the world as a whole and not merely as an interplay of master races and servant races. My scientific career had reached the point where my accomplishments were unquestioned even if they were not yet welcomed in certain quarters near home. I had begun to see the fruition of my work not merely in a number of important independent papers but as a standpoint and as a body of learning which could no longer be ignored. If I were to take any specific boundary point in my career as a journeyman in science and as in some degree an independent master of the craft, I should pick out 1935, the year of my China trip, as that point.

30 The Days before the War

1936–1939

During my last few days in England, while I was visiting the Haldanes in the West Country, Margaret was still over in Germany. When she returned, we picked up the children at the south-coast summer resort where they were staying and left for home. One year's absence had made it necessary for us to take up anew many old threads in our M.I.T. life.

I came back to find the situation in the mathematics department rather confused. Besides Eberhard Hopf, who had been a member of the mathematics department for several years, we had another young man, Jesse Douglas, working with us. Douglas had just completed a brilliant piece of work on the form of minimal surfaces such as are generated by soap films. This is a classical problem and Douglas had so advanced it that he had won the Bôcher prize, which I had previously received for my work on Tauberian theorems.

It must be borne in mind that the depression had frustrated some of President Compton's attempts to raise M.I.T. salaries to the level of those of the greatest universities. We were thus faced by a dilemma. Either we could stay within our means and obtain mediocre mathematicians for mediocre salaries, or we could deliberately seek for those undervalued in the general market, with the hope that as economic conditions improved, and after Compton's program gathered momentum, we could ultimately bring their salaries up to a proper sum.

As a matter of fact, it was not many years before salaries began to go up again, and those of us who had confidence in the bounty of M.I.T. have not been disappointed. Still there was a period when those brilliant men who were undervalued in the general market and had been obtained by us at a discount felt exploited.

Thus, it was no surprise that our two brilliant young men felt aggrieved. So long as I was about, I could talk frankly on the matter and help build up hope for a better future. Once I was away in China, the two of them worked on one another's nerves. The scholar tends to have the sensitivity and, with that, the excitability of the artist. By the time I returned from China both Hopf and Douglas were so far set in their emotional attitudes that they were permanently lost to M.I.T.

It is Hopf's case that was the most interesting. He was a German of sufficiently correct racial origin to be acceptable even in Nazi Germany. Originally he was hostile to Hitler, or at least sympathetic to those on whom Hitler had wreaked his ill will. However, there were strong family influences pulling him toward the Nazi side.

When my cousin, Leon Lichtenstein, had died as an indirect result of the coming of Hitler, the Germans looked around for a successor. At that time good mathematicians were leaving Germany en masse, and a successor was not easy to find. Finally Hopf's name came up, and he was offered the position.

It must be borne in mind that a university position in the Germany of the good old days had a prestige both social and intellectual beyond any comparison with that of a similar position in America. A full professor at a German university was socially superior to the most successful industrialist. What the Nazis offered Hopf, if it could have been taken at its face value, was financially out of the class of anything we could hope to do in the immediate future and considerably greater in prestige than anything we ever could offer in the remote future.

I will say that Hopf consulted with a number of German refugees from Hitlerism and that they did not oppose his acceptance of the offer as vehemently as one might have expected. In the first place, while they were irreconcilably opposed to Nazism, Hopf was obviously willing to temporize with the movement, and they could not argue with him as if he had had any very strong feeling in the matter. Moreover, it was certainly better for Germany to have a man who, although not a vehement anti-Nazi, was at least not a vehement pro-Nazi. Many of the German refugees believed that Germany would either be defeated or by an intrinsic revulsion would sooner or later cast off Nazism, and all their opposition to Nazism had not affected their pride in Germany as such. Hopf would form part of an element in the new Germany which would be at least a possible basis for the re-establishment of academic sanity after the war.

The authorities at M.I.T. did not like to have a pistol presented to their heads in the form of Hopf's claims to an immediate promotion over the heads of older men. If the German offer was to be taken at its face value, and if all moral issues were to be ignored, Hopf was right to accept it. Of course, all of us hoped for an ultimate defeat of Germany, and we were more than suspicious that in that defeat the whole academic system and Hopf himself would go down in ruin. That was a matter for Hopf to decide and for no one else.

Hopf accepted the German offer. In his delight at his sudden rise, he was most condescending to his colleagues at M.I.T. To me he expressed his feelings that I was not getting my full deserts, and he wished that I could find such an advancement as he had found in Germany. I need not say that this condescension was not welcomed.

It is interesting to note that the refugee scientists who stayed in America have made great contributions to American science both for war purposes and for peace. More than half of the leading figures in nuclear science came from within the Axis. Here I need only mention Einstein, Fermi, Szilard, and von Neumann. Von Mises also came over later and made important contributions to statistical theory, as Courant and several members of his school did toward the introduction of the prevailing European techniques of applied mathematics.

My former student, Norman Levinson, was back from England where he had gone on a National Research Fellowship. I did everything I could to secure him for the department, but I found very mixed support for him in high quarters. There were those who helped me to the limit in backing up my judgment, but there were others who felt very definitely that we had enough Jews already. Among these was at least one Jewish colleague, who believed that the welcome he had received in the department might be damaged if more Jews came in, and who treated this welcome as if it were his personal property. As to myself, while I was quite willing to recognize that it might be good policy to distribute people rather widely by racial and cultural origin, I believed then as I believe now, that all such considerations are purely matters of convenience and should be subordinated whenever one has the chance to get a man outstanding on his own merits. Good men are too rare to allow a school to pick and choose on the basis of secondary considerations.

The Harvard tercentenary occurred in 1936, and a good many scholars gathered from all the world for the occasion. Hardy was there from England, and I appealed to him to back Levinson. His effort was successful, though his task was not easy. Levinson has remained a member of the department ever since, and continues to be a tower of strength there.

Thus this was a period in which I was subject to a great number of separate emotional strains. The fact that Nazism threatened to dominate the world was a continual nightmare to every man of liberal feelings, and in particular to every liberal scientist. I was able to take part of my internal storm out in active measures to help the refugees, but this was not enough to give me anything like peace of mind.

The old strains and stresses of my education as a prodigy came back to bedevil me. For all my love for my father, those near to me were not slow in reminding me that I was after all nothing more than my father's son. The fact that I was a Jew rendered my emotional situation somewhat ambivalent. In America there was a reaction in our favor from the atrocity and terror of the German situation, but this did not completely compensate for the knowledge that somewhere in the world we were being threatened with extermination, and that Nazi anti-Semitism had provoked an echoed anti-Semitism in some American quarters.

I had not only to suffer the direct stresses and strains of my origin and of my early education but the subsidiary strains as well, which originated from my entering academic life from a rather unusual angle, without sufficient social maturity to know just what I was and where I was going. These strains had been much eased in the course of time by my marriage to Margaret, but I am afraid that I had merely transferred to her the impact of the conflicts already implicit in my own nature.

As the years went on, some of my difficulties decreased, for people will forgive in an older man what they will not tolerate in a youngster. Nevertheless, the epoch which would naturally have been the time of my emotional release was complicated by the strains of the depression, of Nazism, and of the threat of war, so that there was no period during which I could recover in peace from my earlier conflicts, and in which I could taste a few years of true serenity.

What with the Jesse Douglas problem, the Eberhard Hopf problem, and the problem of securing for Levinson his just deserts, all added to the tenseness and suspense of the prewar period, I was in a state of confusion. When I came back from China I was forty-two years old, and I had already begun to feel that I was no longer a youngster. The burden of many years of hard life had started to tell on me. Following the advice of my wife, I consulted a doctor friend who had gone from internal medicine to psychoanalysis.

Under the circumstances it is scarcely astonishing that I needed psychoanalytic help. Indeed, notwithstanding a deep skepticism concerning the intellectual organization of psychoanalysis, I would have sought this long before—if I had known the right quarter to which I could turn. I made some abortive attempts to undergo psychoanalysis during my stay in China. Even then, however, I began to learn that the more individual a man's background is, the harder it is for him to find the right practitioner.

Even as a child I had read on psychiatric topics, and I was familiar with some of the writings of Charcot and Janet. Moreover, my own experience had convinced me long before I had heard of Freud that there were dark lacunae and concealed urges in my soul that offered a deep resistance to being brought to light. My studies in philosophy had made the notion of the unconscious no novelty to me, and I was aware of the cruel and almost unspeakable impulses which this unconscious hides as well as of the nearly irresistible tendency to gloss over them and to bury them beneath a layer of rationalisation.

Thus, when I learned about Freud and his ideas, I was quite prepared to see in them a new revelation with a real measure of validity. Nevertheless, I was repelled by the internal rationalizations of the psychiatrists themselves. Their answers to all human problems and to me were too glib and too pat. Without denying in any way the therapeutic validity of much that they did, it did not seem to me that the intellectual roots of psychoanalysis had yet reached that degree of convincingness and scientific organization which carries with it full conviction. Furthermore, the maxims of the need for submission and for financial sacrifice on the part of the subject of psychoanalysis appeared to have too much in them that is professionally and financially advantageous to the psychoanalyst to seem fully objective.

Freud himself had obviously carried out on his own soul a large degree of psychoanalysis, without putting himself in that classical passive attitude which he himself later defined, and I had seen in my own person the beginnings of a psychoanalytical consciousness which was not imposed from the outside. I therefore was hardly ready to throw myself in the recommended state of full submission.

Neither was I ready to accept without question the orthodox psychoanalyst's valuation of personality, and the goals set by him as a result of a successful psychoanalysis. I never have valued contentment and even happiness as the prime objects of my life, and I began to fear that one of the aims of the conventional psychoanalyst was to remake his patient into a contented cow.

I performed the usual analytical reporting on the psychiatric couch and tried to supplement it by all that my insight could furnish concerning my own motivations and my internal set of values. I let the analyst know how deep I found the impulse to creative work and how much the satisfaction of success in this work was of an aesthetic nature. I also told him what my tastes in literature were, particularly in poetry. There are passages from Heine, especially in his *Disputation* and his *Prinzessin Sabbath,* that relate and express the religious exaltation of the Jew, which I cannot recite without tears. I told him, moreover, how the sudden shift in Heine's attitude between awareness of the degradation and baseness in daily life and the exaltation of declaring the glory of God and the dignity of the despised Jew, create in me a deep sense of awe.

All these things my psychoanalyst rejected as not coming from the true depths of my subconscious. For him they represented merely the things that I had learned at conscious levels and were of no importance in comparison with the slightest tag of a hint got from a half-remembered dream. Conscious they may have been, but their ability to move me did not come from any superficial level of my consciousness.

My analyst regarded them as a sort of contraband which had not paid the duty of a psychoanalyst's couch. He refused to consider them to any extent, and he left me with a deep feeling of having been misunderstood and misrepresented. He accused me of that cardinal sin of the psychiatric patient known as resistance. I certainly did resist, but the very fact of this resistance was a clue to much that I had experienced, and to much that was at the bottom of my spiritual make-up. Finally we parted, after a most futile half year of trying to get something from a man who, as I am convinced, did not have very much of a notion of what made me tick.

Later on, I have turned to other psychoanalysts who did not go so much by the dream book, and who made a far greater effort to get in rapport with me as a human being. These more sophisticated and sympathetic friends do not make so much of a fetish of the ritual of the couch. They do not omit to record my dreams and contradictions. However, they treat me far more as an individual than do their ritually orthodox Freudian colleagues. For them the psychoanalyst's couch is not a bed of Procrustes. They accept such differences of opinion as I have with them without labeling them at once with the damning epithet of resistance.

Naturally I was not in a position to confine my attention to my inner problems and reserve all my efforts for the baring of my soul. I still had a certain responsibility left in the matter of placing refugee scientists. However, this responsibility had become less arduous with the years, and their problems were assuming a new nature.

More countries, such as Finland and even China, were furnishing their quota to American science. The early arrivals were gradually depending less and less on their native language, which was generally German, and were taking up the American way of life as something normal. The older people were bringing up their children within the American tradition, and it was obvious that whatever might come, few of them would return to their homes in Europe on anything but a temporary basis. The younger immigrants were marrying into American families.

The final result of the great immigration of Hitler times is not yet to be seen, but it is certain that the contribution to our mathematical life of new individuals and new stock will prove to be comparable to that of the Germans in 1848, or to that of the Huguenots who migrated to England, to Holland, and to America at the time of the revocation of the Edict of Nantes.

Naturally, with so many first-rate scientists added to the American community, I collaborated with several of them in research projects. Aurel Wintner had come over to America, if I am not mistaken, before the great rush of immigrants, on the basis of a recommendation from his teacher, my cousin Leon Lichtenstein. One summer Wintner and his family took a cottage in New Hampshire some twenty miles from us. In that part of New Hampshire, to be twenty miles away is to be a next-door neighbor.

Professor Wintner is a very alert, enthusiastic scientist, quick in his motions and his thoughts, and highly original in his ideas. Mrs. Wintner is the daughter of the well-known German mathematician Hölder. These marriages of mathematicians to the daughters of their professors are so typical of the academic world, both in Europe and here, that there has come to be a saying that the genetics of mathematical ability is peculiar—it is not inherited from father to son, but from father-in-law to son-in-law.

Ultimately Wintner came to be a more or less permanent summer visitor to our part of New Hampshire. We began to undertake work together concerning a variety of topics in his own field. Some of these dealt with the extension of certain of my ideas concerning generalized harmonic analysis to the orbit problem and the perturbation problem in celestial mechanics. These represent a modern approach to old eighteenth-century problems of Laplace and Lagrange.

Another piece of joint work dealt with a modern probabilistic approach to Maxwell's kinetic theory of gases, treating a gas as a set of moving particles acting under mutual forces. I had already done some earlier research in this field tying up with the work of two physical chemists, then at Columbia and now at Chicago.

A third line of work which Wintner and I followed out together was the tightening and simplification of the proofs of the ergodic theorems of Koopman, von Neumann, and Birkhoff. These theorems, which I have mentioned already, had furnished the missing step in Willard Gibbs's work and allowed a rigorous carrying through of his idea of time averages with averages over all possible worlds. In this latter work we benefited much by discussions with the young Dutch mathematician, E. R. Van Kampen, who was our companion on various hikes through the White Mountains. Poor Van Kampen, who seemed to have a most promising career ahead of him, died a year or two later of a brain tumor.

Over all this period I had hoped that a return to lecturing in China would not be many years away. This hope was, of course, completely destroyed by the events of the next few years. In 1937, my colleague, K. S. Wildes, of the electrical engineering department was my successor in China. He returned just about the time of the battle between the Chinese and the Japanese at the Marco Polo Bridge.

Besides its world consequences, this battle had personal consequences which I felt very deeply. At the time of the Marco Polo Bridge incident, Lee and Mrs. Lee were visiting friends in Shanghai. The ensuing war between China and Japan caught them away from home, preventing them from returning to Peiping. For part of this period Lee was able to find teaching employment in Shanghai, but for much of the time the Lees were left to live on their own resources and on Lee's knowledgeability in matters of Chinese art.

To suffer this interruption of his scientific development at what should have been its best and most critical period was a serious loss indeed; and the problem of how to handle the situation, when any handling should become possible, distressed me greatly. I did everything that I could to bring Lee over to the United States, but at this time I had no positive success.

Wildes had been as much interested and attracted by his study in China as I had been a year earlier. The two of us were very busy in the ensuing years trying to influence American opinion to give increased aid to China. We went to President Compton for help in this effort, and he took a leading share in the China relief situation. Other high authorities of the Institute also participated in this.

While Communism itself never appealed to me, I have not been able to feel that right views may not be held on many topics by members of a group of which I do not approve. When it was the fashion for Communism to condemn Nazism and stand up against race prejudice, the fact that these admirable opinions were held by Communists was made an excuse for rejecting them that only a fool could maintain. That after the defeat of Nazism the Communists have become the chief fear of the West and that they have behaved with much of the tyranny of the aggressors that they have replaced does not change in the least the fact that some of the things they stood for in the period between the wars belong to the attitudes of every decent man.

The change in Communist attitude and tactics is quite a sufficient reason for rejecting them as mentors, and certainly for distrusting their intervention; but it does not in the least alter the fact that in the period of confusion when no single party had clean hands, many young men tended to look toward Russia. For some of these, the participation in radical movements was an important stage in their moral growing up. It taught them not to take out their grievances against the world in mere sulking but to try to do something of public benefit. This habit of active participation in moral issues has long outlived the period when they believed in Communism.

Under these circumstances it is easy to see why I did not shy away from a request to help the Chinese cause, even when this cause was backed by armies not attached to the interests of the bumbling, inefficient Kuomintang.

For some time we felt confident that the help which the United States was giving to China was going through the right channels. However, disturbing rumors began to reach us. I gradually began to hear statements from informed Chinese and from traveled Americans to the effect that the Kuomintang was a broken reed, that it was not using American aid effectively, and that it was diverting large shipments of arms and of medical supplies to be resold by the more corrupt members of the party.

About this time, I was approached by a group of people who wanted me to sponsor aid for the Communist soldiers in China, who seemed to be doing a more competent job in fighting the Japanese than did the soldiers of the Kuomintang. I accepted the invitation because I felt it to be to the advantage of the United States.

The people who were really running the movement were a sincere but undistinguished and inefficient lot, and they seemed to me to be wasting more time in grubby social gatherings and in talk than in the effective collection of money. I later found out that the group contained some who acted as if they belonged to the more peripheral ramifications of the Communist Party, together with a rather larger group of well-intentioned and stable friends of China.

No alert person could have gone through the years from the depression to the beginning of the Second World War without some experience of the American repercussions of Communism. The young men coming up in academic life during the depression period were forced to recognize that they were stepchildren of the present world order. Security had become a dream of the past, and the various hatreds which made up the complex of Fascism, Nazism, and Ku-Klux-Klanism were an ever present threat, particularly to members of out-groups. They were seeking some movement or some attitude to which they could attach themselves, and they were bound to hear among other voices those of their convinced Communist colleagues.

From the very beginning I had been repelled by the totalitarianism of the Communists, even as I have always been repelled by the whole apparatus of orthodoxy and conversion in whatever religion it might occur. However, my very need for independence made me loath to interfere dogmatically with the decisions of the young people about me. In an age of many bigotries, I certainly could not feel the bigotry of the Communists as our greatest immediate threat, nor could I fail to recognize that the appeal which Communism had to some of my young friends was an appeal to their humanitarian instincts.

In the course of time, almost all of them have seen the power-politics side of the movement and the way in which good intentions have been turned into tyranny by ambitious men. They are not now Communists, nor have they been for many years. However, the effect of the fear of a witch hunt against Communism was to make it difficult for them to find an honorable and self-respecting way out and, if anything, tended to delay their exit from the Communist milieu.

My attitude toward the Chinese was reinforced by similar American support of the Spanish Loyalists. Here the moving spirit was Professor Cannon, of the Harvard physiology department. He was without any doubt the great man of American science at that period, and he had lectured in Spain a few years before.

Spain is a country which has not abounded in great scientists in modern times, but the one field in which it has done very important work has been the physiology of the nervous system in which Cannon himself was, of course, greatly concerned. It is no wonder that Cannon felt himself very much attracted to this revival of intellectual life in Spain and that he made it his duty to rally American support behind the Spanish Loyalists. In this he was in no position to reject aid from any quarter, and it is not surprising that an appreciable part of his aid came from Communist circles. This led to certain whisperings against Cannon, but he was far too sincere and forthright a man to be scared away by whisperings. I had joined in with Cannon in his support of the Loyalists, and it seemed to me that this policy was a valid pattern to follow in connection with the Chinese matter.

The times were also complex and worrisome for me in family matters. Father had retired from Harvard a little before the time of his accident. He was a disappointed man, and the bluntness with which President Lowell had accepted his retirement without the grace of a few kind words added to his disappointment. After his partial recovery from his accident, he continued to do research work at the Harvard Library, and even to walk there from Belmont, but his activity grew less year by year.

Soon after my return from China, he began to go downhill rather rapidly, and there was evidence that he had had a stroke. He needed hospital care, but this time there was much less sign of recovery than there had been on the previous occasion. He lapsed into an agitated state of depression, in which his mind was often confused. Yet he was fully aware that he was confused and that he was losing his grip on life. His depression often took the form of what seemed to me to be really a commentary on the baleful world political events of the time.

He would speak indifferently in Russian, German, Spanish, French, and English. When he spoke in languages I knew, I could observe in him no trace of grammatical confusion and no tendency to mix the words of one language with the grammar of another. Even when he could no longer recognize me as his son, the correctness and vigor of his polyglot speech was not in the least affected. Father's knowledge of language was not merely something painted on the surface of his brain but went right through its very texture.

I visited Father often, and took him out from time to time for trips in my car. However, he was on the way down, and it was scarcely even desirable that this half-life, which was all that was left to him, should be drawn out indefinitely. Finally, in the first year of the war, he died calmly and peacefully in his sleep.

For years Margaret's mother had made her home with us, except during one or two visits to her kinsfolk in Germany. During this period, as I have said, German became to a considerable extent the language of the household. What emphasized the role of German in our life was a little incident which occurred at one of our visits to a Boston club, Friends of China.

There was a Radcliffe student there of mixed Chinese and German origin, whose father had been a coal-mine operator in Peiping and had married the daughter of his landlady during his student days in Germany. Lottie Hu, the daughter of this marriage, was studying anthropology in the graduate school at Radcliffe. The vicissitudes of the war had cut off her income. She summoned up her courage to ask Margaret whether she might not live with us and earn her room and board and a little pocket money by helping with the family. Accordingly we took Lottie into our household, where she became the friend and companion of my young daughters.

Lottie was completely trilingual, speaking Mandarin Chinese, English, and German with equal fluency. Since German had already become the second language of the home, this continued under the new arrangement, and both my daughters made a certain amount of progress in the elements of the German language.

My daughters were in junior high school. We had the usual amount of friction between parents and children; in particular, the two of them had a certain resentment of my intellectual position. Peggy said, on more occasions than one, “I'm tired of being Norbert Wiener's daughter; I want to be Peggy Wiener.” I made no attempt to force the children into my frame, but the mere fact of being what I am inevitably subjected them to a sort of pressure with which my will had nothing to do.

I was proud of them, but I did not bring them up to be infant prodigies. I was particularly proud of Barbara on one occasion when she had read in her textbook some comment on the Latin Americans and she said to me, “Daddy, the author of this book seems to be very patronizing to the Latin Americans. Don't they hate it?” I replied to her, “You're damn tootin', child, and how!”

About this time there was a radio program established in Boston on the model of “Ask the Children.” Barbara took part in this. I am not at all certain of my wisdom in having let her participate, but she did well, and she learned the elements of the art of handling herself before an audience. I have kept a degree of interest in the later destinies of more than one member of this children’s panel, and they seemed to have done uniformly well and to have suffered no real disadvantage from the adventure.

Thus, like all families, we had our problems to consider and our decisions to make. I am neither certain of the correctness of the policies I have adopted nor ashamed of any mistakes I might have made. One has only one life to live, and there is not time enough in which to master the art of being a parent.

The bringing up of young children is not easy, but it lightened our household work to a considerable extent to have it shared among three women. My wife’s mother was always doing little bits of work around our houses in Belmont and in the country, and she had accumulated a remarkable set of tools and gadgets for these tasks, which she so enjoyed.

She was a country-bred German woman, with a romantic outlook on life which had led her to seek the open spaces of the American West. Here in New England our farmhouse was as much a delight to her as to any of us.

It was in the summer of 1939, just before the outbreak of the Second World War, that she died quietly in her sleep in her upstairs room in this New Hampshire house. We buried her in a little country graveyard open to the sweep of the winds blowing across from the Ossipee mountains. We chose a tombstone of a pattern traditional in these New Hampshire burying grounds, but on it we had carved an inscription suiting both her German origin and the massive vigor of her character. It was the beginning of Luther's hymn:

"Ein' feste Burg is unser Gott"—"A Mighty Fortress Is Our God." I am grateful that she was spared the full terror and humiliation of the Second World War, which was about to begin.

Even before the arrival of the war itself, the somber train of catastrophes had begun. The fall of 1938 was marked both by Munich and by the first of the series of West Indian hurricanes which have come to plague Boston of recent years. From that time on, all of us expected the world war to break on us. It held off until the summer of 1939.

That summer, after the death of my wife's mother, Margaret and I had taken a little trip together into Canada. This trip furnished the precedent for a similar motor trip which we make almost every summer. Later on I took another trip into Canada alone, this time to participate in the meeting of the American Mathematical Society in Madison, Wisconsin. I drove from my farm by a route north of the Creak Lakes, arriving in Sault Ste. Marie, Michigan, on the evening of the second day.

Here I learned that the war had already come. It was an experience curiously reminiscent of that time twenty-four years before, when the First World War had come to me on another trip, as a passenger on a German boat in the middle of the North Atlantic. All the fun and jollity of the summer mathematical meeting was exploded. We had hoped that this meeting would serve for the planning of an International Congress to take place in the United States during the summer of 1940, but this plan had to be put on ice for ten years.

I drove back East with an English colleague, and we had the opportunity to take stock of our emotions and expectations while picking grapes for a friend in New York State.

31 The War Years

1940–1945

I came back to M.I.T. in the fall of 1939 and took stock of the situation. It was not hopeful. The passive alliance between Russia and Germany had dashed cold water on our wishful thinking that the Nazis were going to be held back in the east, and although the two countries finally fought each other, we did not at that time expect such luck. Moreover, the good will that had been building up for Russia, largely because we could scarcely see from what other direction a blow might come which could limit the Fascist aggression, was much dampened by Russia's aggressive policy in Finland.

In academic and technical circles, most of us were aware that a world war would ultimately engulf the United States as well as all other important countries. Therefore we began to turn our thoughts toward finding out in what sector of work we might make ourselves of use.

The possibilities of active military service had never been very real to me on account of my nearsightedness, and the passing of the years had not increased my physical fitness. I have never fancied myself as an administrator, nor, as a matter of fact, has anybody else ever fancied me in that capacity. It seemed obvious that I should have to turn to some sort of scientific research work

I had had an apprenticeship in ballistic computation in World War I. Ballistic computation consists in the making of tables for artillery and small-arms fire which give the range of the weapon and various other related constants in terms of the angle of elevation of the gun, the powder charge, the weight of the missile, and so on. This work had trained me in much more than purely ballistic matters, for it had left me with a pretty general sophistication in the ways of the computing room. I had also spent much time in recent years working with electrical engineers. Therefore I foresaw that my destined berth in the war would be some sort of job in which I should apply computational techniques to electrical engineering problems.

Furthermore, my work with Lee had given me a look-in on problems of engineering design.

All this was clear, but what was not clear was the direction from which the call would come. As the nerve-wracking wait of the "Sitzkrieg" began to give way to a more active and threatening military schedule, most of us came to see that the main problem before America would be to keep England in the war as an effective combatant until such time as we might enter it ourselves. This meant that the submarine and the bomber campaigns were the two chief menaces which we should help to conquer.

Luckily, England herself had given us the best possible lead for helping her in these fields, in the brilliant invention of radar. M.I.T. was pushing this sort of research from the very beginning, even before the start of the European war and long before we got into it. But for the time being this seemed to be a matter for specialists, and I was not a radar specialist.

The stream of refugees from Germany speeded up for the moment and then ceased altogether. These last driblets of immigration did not seem to me to consist altogether of persons of the same moral value as some of those who had come before. More than one of these last drippings of the wine press showed an eagerness to indoctrinate us in the irresistible momentum of the Nazi advance. Their zeal could scarcely have been exceeded if they had been paid propagandists. At last it became quite clear to us that in addition to the great cultural crop of fine, persecuted men and women who have enriched our intellectual life, there were those whose main objection to Nazism was that they were excluded from it. However, the new summer vacation came along in its good time, and we tried to make our life as cheerful as possible, notwithstanding the catastrophe about us. One cannot live in a perpetual atmosphere of gloom.

The Inghams, from the English Cambridge, were caught in America by the war and became our summer neighbors. They shared with us the pleasures of mountain walks and bathing in Bear Camp pond.

We had a curiously interesting visit that summer from the Hungarian mathematician Erdös, the Japanese mathematician Kakutani, and the English mathematician Stone. They had just got into trouble on Long Island by inadvertently approaching too close to a radio-beacon station. They were put in jail overnight as suspicious aliens but were released later when the authorities were able to reach their sponsor, Professor Veblen, of Princeton. It was just after this contretemps that they drove up to New Hampshire, and we had a most pleasant little session on our porch. At present, Kakutani is teaching in the United States, but Stone and Erdös have gone back to Europe.

At the end of the summer, Ingham returned to England as he had planned, but his wife and children and their maid stayed again in our valley the next year. Again we went for long walks together, the children now participating in them more easily. I have seen the family several times since their return to England, where I believe one boy is going to the university and the other is an officer in the Air Force. They still retain a real love for New Hampshire and our valley.

Wintner remained our summer neighbor. He and I had planned to work together during the year 1940–1941, and he came to Cambridge to do so. It was unfortunate that my attention was concentrated on war work at the time. I feel that I was to a certain extent unjust to Wintner in not living up to our informal contract, for he found himself able to ignore the pressures of the warlike atmosphere. I could not, and though I was willing to work with him with a part of my attention, I was unable to devote to it my full interest. Thus we went our two ways, which gradually drifted farther and farther apart.

By spring, the catastrophe of Norway had occurred and the catastrophe of France was threatening. The emotional solace of our country house, in which we had become used to take refuge from the buffets of the outer world, was of no avail to us when we were confronted by the imminent loss of European civilization.

In August 1940, the summer meeting of the American Mathematical Society was at Dartmouth. It was as pleasant as a meeting could be when nothing but the war could really command our attention.

The algebra of complex quantities is vital for telephone engineering, and the Bell Telephone Company instrument, a numerical computer, was made to fill a definite need in such work. Its importance derives from the fact that our Arabic notation for numbers gives to the number 10 an artificial position justified by custom alone and constituting no part of the real foundations of arithmetic. Instead of writing a number as so many units, so many tens, so many hundreds, and so on, we can just as easily write an integer as a sum of ones, twos, fours, eights, and so on. In this case, instead of the ten digits of our conventional scale of numeration, we need only the two digits, zero and one.

The Russian peasants use what amounts to a scale of this sort, known as the binary scale, for their addition, multiplication, subtraction, and division. It has the great advantage over the scale of ten that the multiplication table reduces to the statement that one times one is one.

For obvious reasons, it is easier to mechanize arithmetic on the binary scale than on the scale of ten, and the Bell System instrument accordingly employed the binary notation. The sole serious disadvantage in doing all arithmetic on the binary scale is simply that we have adopted the decimal system and the bulk of existing numerical results are given in this tradition. When we have a large amount of new computation to do, it is often worthwhile to ignore this fact and to translate all our initial data to the binary scale and all our final results back to the decimal scale.

One place where the binary system of numeration is used is in the employment of gauges for the measurement of the thickness of a mechanical part. Suppose that we have one accurate gauge of a thickness of one inch; one accurate gauge of a thickness of two inches; one accurate gauge of a thickness of four inches; and one of a thickness of eight inches. Then we can combine these to give accurate measurements by inches from one inch to fifteen inches. The code is the following: we combine our gauges on top of one another in these combinations:

1 inch	1 inch gauge
2 in.	2 in. gauge
3 in.	2 in. gauge and 1 in. gauge
4 in.	4 in. gauge
5 in.	4 in. gauge, and 1 in. gauge
6 in.	4 in. gauge and 2 in. gauge
7 in.	4 in. gauge, 2 in. gauge, and 1 in. gauge
8 in.	8 in. gauge
9 in.	8 in. gauge and 1 in. gauge
10 in.	8 in. gauge and 2 in. gauge
11 in.	8 in. gauge, 2 in. gauge, and 1 in. gauge
12 in.	8 in. gauge and 4 in. gauge
13 in.	8 in. gauge, 4 in. gauge, and 1 in. gauge
14 in.	8 in. gauge, 4 in. gauge, and 2 in. gauge
15 in.	8 in. gauge, 4 in. gauge, 2 in. gauge, and 1 in. gauge

This is equivalent to writing the numbers 1 to 15 in the following way: 1, 10, 11, 100, 101, 110, 111, 1000, 1001, 1010, 1011, 1100, 1101, and 1111.

I do not remember whether it was before or after the Dartmouth meeting that Vannevar Bush had sent around to the various members of the M.I.T. faculty a questionnaire asking for suggestions concerning the mobilization and use of scientists in case of war. This was a matter on which I had very definite opinions, and I was strongly in favor of a scientific collaboration which would cross the frontiers between one science and another, and which should at the same time be voluntary, thus preserving a large measure of the scientists' initiative and individual responsibility. I distrusted all plans that might depend on a high degree of subordination of individuals to a completely authoritative setup from above, which would assign each man the narrow frame within which he was to work. I suggested therefore the organization of small mobile teams of scientists from different fields, which would make joint attacks on their problems. When they had accomplished something, I planned that they should pass their work over to a development group and go on in a body to the next problem on the basis of the scientific experience and the experience in collaboration which they had already acquired.

But nothing resulted. Those who work almost exclusively with gadgets tend to develop a love for them, since they lack the unpredictable factors which affect the operation of the human being.

Gadgeteering very easily becomes a sort of religion. Luckily, the vicissitudes of the last twenty years of gadgeteering have somewhat shaken the faith of many men, including Bush, in the unlimited scope of the machine. However, there remain many people who have not been as directly confronted as Bush with the disadvantages as well as the advantages of machines, and these people follow the tendency of the day to favor the big laboratory and the big administrator.

On the way back from the meeting, I began to discuss with Levinson, who was now a full-fledged colleague, the general problem of computing machines, and to wonder whether this might not be the field in which I was destined to do my war work. For some time I had been considering for Bush the use of machines to solve systems of partial differential equations and I felt that a sort of television scanning would be the proper basis for the mechanization of partial differential equation problems. My new experience with the binary machine convinced me that electronic binary machines would be precisely the devices required for the high speed of computation needed in partial-differential-equation problems.

I saw that for a machine to work properly on partial differential equations, it would have to go through an almost incredible bulk of work in an almost incredibly short time. This suggested to me that the future of high-speed computing machines for these particular purposes could not lie in Bush's models, which represented physical quantities by electrical or mechanical quantities, but rather in some enormous extension of the ordinary desk computer, working as I have said, on a scale of two rather than on a scale of ten.

Now that I had come to take a serious interest in problems of the speed of computation, I was forced to consider the relative merits of two grand strategies in computational methods. One of these, which Bush followed, was called analogy computation, in which the numerical digits of computation are represented as measurable physical quantities. The other, the digital mode of computation—which is that of the desk computing machine—represents a number by the sequence of its digits.

The important point in the distinction between the analogy computers and digital computers is that the latter do essentially what we ourselves do with arithmetical operations on paper. When we represent a number as 56, we mean that it is a combination of five tens and six units. When we multiply this by 38, which is three tens and eight units, we go through the operations indicated in the following table:

$$
\begin{array}{r}
56 \\
\underline{38} \\
48 \\
40 \\
18 \\
\underline{15} \\
2128
\end{array}
$$

We never have to go beyond the multiplication table and the simple rules of addition, nor do we represent our 56 and our 38 as quantities such as 56 degrees or 38 inches.

There are digital multiplying machines in which ten plays no role, which operate on the binary scale. This is how they work: Let me consider the operation in which $7 \times 5 = 35\cdot$

$7 = 4 + 2 + 1$

$5 = 4 + 1$

In the scale of 2, these statements are equivalent to writing 7 in the form 111, and 5 in the form 101—meaning that 7 is one 4, one 2, and one 1; while 5 is one 4, no 2's, and one 1. When we multiply these we get the following schedule of operations:

$$
\begin{array}{r}
111 \\
\underline{101} \\
111 \\
\underline{111} \\
11211
\end{array}
$$

If we remember that, in our scale of notations, 2 = 10, the number 11211 may be written 12011 or 20011 or 100011. The last representation is the truly binary one, which uses no digits other than 0 and 1. Here this means 32 + 0(16) + 0(8) + 0(4) + 2 + 1 = 35. This method is called the method of multiplying on the scale of two. I repeat, it is quite as digital as ordinary multiplication on the scale of ten.

In contrast, a particular analogy-computation instrument proceeds as follows: In an electrodynamometer, two coils are attracted to one another in proportion to the product of the current carried by these two coils, and this attraction can be measured by an appropriate sort of scale instrument. If, then, one coil is carrying seven units of current and the other five units, the scale will read something proportional to thirty-five. This sort of instrument for multiplying is known as an analogy instrument, because we are replacing our original situation, in which certain quantities are to be multiplied, by a new situation, in which we set up two currents in analogy to the original quantities and read off the product by a physical situation analogous to the original one.

Digital computing machines thus differ from analogical computing machines in that they can theoretically be read to the complete degree of precision of the numbers introduced, while analogical machines are restricted to the degree of accuracy with which the original situation is truly analogous to the corresponding situation, which we use to replace it in our computation. The Bush machines for solving differential equations are strictly analogy machines.

As to the relative merits of these machines, the great flexibility of electrical and other measuring apparatus makes the construction of a fairly good analogy machine easier than that of a fairly good digital machine. However, when it comes to high speed or high accuracy, the advantage is all with the digital machine. There are very few physical measurements which can be made with an accuracy greater than one part in ten thousand, and this corresponds to a determination of four decimal digits and not quite fourteen binary digits.

Moreover, to take a measurement with this degree of accuracy can scarcely be a truly instantaneous process. Analogy machines are intrinsically slower than is needed to fulfill the demands of the very fastest and bulkiest computation, so that I felt that they had already reached their apogee.

When it came to the matter of digital machines, I was forced to consider the real essence of the action of such a machine. The ordinary desk computer determines the position of certain wheels on the basis of the position of certain other wheels. Each such position is a choice between ten alternatives. It is not difficult to represent these ten alternatives by ten projections on a metal wheel, but the use of metal wheels involves very disagreeable and restrictive problems of inertia and friction.

It seemed to be preferable in every way to replace the mechanical choice characteristic of existing digital machines by an electronic choice of digits. The two advantages to be expected here were the vastly reduced inertia of a stream of electrons as compared with a sequence of mechanical parts, and the greater technical ease of canceling quasi-frictional losses—that is, resistance losses—by amplification. As a result of both of these, I was quite certain that the coming high-speed computing machines would be electronic digital machines. I may say that the idea had begun to come up in various places in the literature and that, in accepting this approach to computing machines, I was simply representing the spirit of the times.

As I have said, a decimal digital machine uses a choice among ten possibilities as its fundamental decision while a binary machine uses a choice between two alternatives. I suppose that the general use of the scale of ten came from the ten digits of our two hands. Certain races such as the Mayas have apparently counted on fingers and toes together, and use the scale of twenty. It is an interesting reflection that if the human race had been constructed after the pattern of the Walt Disney cartoons, with four digits on each hand, we should probably have adopted the scale of eight, which is merely a slight variation of the scale of two, since $2 \times 2 \times 2$ is 8.

However, there is a fortuitous advantage that, while it does not recommend the scale of ten, at least makes it easier to use it than, let us say, the scale of thirteen. Computing machines of the decimal type depend on the use of wheels with ten equally spaced teeth. To construct them involves laying out a decagon—a polygon with ten angles. This is a simple problem in plane geometry, while the construction of a regular figure with thirteen sides is not.

In the case of an electronic circuit, however, the parts which are equivalent to wheels are not dependent on ordinary plane geometry, nor is ten a particularly easy number to represent. The natural choices to be made in an electric circuit will be between pairs of alternatives.

There are well-known circuits already existing with two alternative positions of equilibrium, and these are known as flip-flop circuits. About the only easy way of constructing a circuit with ten choices seemed to be by a combination of flip-flop circuits. The logic of flip-flop circuits would lead to a combination of choices among a number of alternatives—a combination which was a power of two. Thus, it seems that the only natural way to construct a set of ten alternatives is to construct sixteen and throw away six.

In the design of a machine, you pay in effort and in cost not only for everything that the machine does but also for everything that the machine might be made to do; and to use sixteen alternatives for the purpose of selecting ten represents a wastefulness of 37 1/2%. I came to the conclusion that the high-speed computing machine for the solution of partial differential equations would be a digital electronic machine on the scale of two.

For work on a scale of two, we need to use machines which have only two possible alternatives, such as the presence or absence of a punched hole in a card. This device was already familiar in the Hollerith machines made by the International Business Machines Corporation. This particular way of writing a number in the scale of two is, however, unsuitable for a thoroughly high-speed machine. Punching a hole in a piece of cardboard is a slow process when one considers speed on the scale of millionths of a second per operation. Some such scale is needed before we can consider a computation as really high-speed, and, moreover, the problem of disposing of the used cards and of keeping a sufficiently large stock of new cards soon becomes almost astronomical.

The speed of punching the paper might be greatly increased by using an electric spark rather than a mechanical punch, but this would leave the problem of the bulk of material just as bad as it ever was. Thus, I was naturally driven to the idea of steel tape on which a magnetic mark would be made by an electromagnet. One can read such a mark at high speed and erase it at high speed, leaving the tape blank and ready for another use.

One of the main problems with such a tape is to make the marks so small that as many as possible can be kept clearly distinguishable on a given area. This requires extremely small pole pieces for the marking and reading magnets. It seemed to me that the smallness of these pole pieces might be vitiated by the spread of the magnetic field within the tape, unless the tape itself or at least its effective magnetic layer were extremely thin.

I therefore had the idea, partly on my own and partly through conversation with those of my colleagues who were better acquainted with technical developments in this subject than I was, that the best thing to do would be to treat separately the two requirements of the tape: to carry magnetization and to hold together. We might do this by imposing a thin magnetic layer on a non-magnetic material which would have all the strength. I had thought of a thin iron layer superimposed on a tape of brass or non-magnetic metal, but I had also thought—I believe through the suggestion of a colleague—of the device which now dominates the field: of a paper tape carrying a thin layer of magnetic oxide of iron.

I have recently talked with a friend at the IBM company concerning present practices in high-speed computing machines, and in particular in those which work according to what is now known as the Monte Carlo method, solving partial differential equations by an extremely often repeated process of averaging. Apparently the devices I suggested in 1940 are substantially those which are now employed.

The odds in a gambling house are extremely regular and predictable, and the Monte Carlo method consists in setting up a mathematical problem as an ideal game, in playing it out a large number of times, and in determining the theoretical winnings. The computational device which I suggested in 1940 had the same non-static character as the Monte Carlo method of the present day and also depended on the playing out of an ideal game.

I made a report of my suggestion to Vannevar Bush, but I did not get a very favorable reception. Bush recognized that there were possibilities in my idea, but he considered them too far in the future to have any relevance to World War II. He encouraged me to think of these ideas after the war, and meanwhile to devote my attention to things of more immediate practical use.

Later on I found that he had no very high opinion of the apparatus I had suggested, especially because I was not an engineer and had never put any two parts of it together. His estimate of any work which did not reach the level of actual construction was extremely low. The only satisfaction I can now get is that I was right something like ten years before the techniques to prove my ideas were developed.

Having discarded this as my task in the threatening war, I began to look around for other places where I might be more useful. At one time I had ideas concerning a mathematical and mechanical method for encoding and decoding messages. My ideas would certainly have worked, but in this field it is not enough for an idea to work. In fact, for it to have any merit at all, it must work considerably better than existing devices, or any devices easily invented.

The question of what it means for one decoding-coding device to work better than another is not simple. It can be taken for granted that any ciphered text which is sufficiently long can be decoded by a possible enemy if you give him enough time, and it also must be considered that the problem of decoding a cipher message is not necessarily trivial even when the cipher is known. A good cipher must combine a certain ease of decoding by machine, or by a recipient in the know, with a large measure of difficulty for an enemy to decode it without the help of a knowledge of the cipher.

As is usually the case when there are two different requirements for a system or an apparatus, this does not lead one to a single apparatus but to a number that is large according to the weight one puts on each of the two requirements. Thus, there are easy ciphers which will be useful in the field for messages which need only an hour's secrecy, and there will be difficult ciphers for messages whose secrecy must be maintained for months. There will be a whole spectrum of ciphers in between. For this reason, the design of ciphers is not a matter into which one can go cold, without knowledge of the existing tradition and of the practical demands for each particular use. Again I had to look around for another possible field of usefulness. I found this in the design of fire-control apparatus for anti-aircraft guns.

When I was a boy, fire control was conceived primarily from the point of view of coastal batteries and of battleships—that is, of gun platforms whose motion relative to the target was so slow that there was a chance for a considerable amount of computation by very crude manual devices before the target should have passed out of effective range.

Even in the First World War, the airplane had changed all this. The problem of shooting down an airplane is not, of course, like that of lobbing a mortar shell into a fortress but like that of shooting ducks on the wing. The duck will not stay still while you are shooting; and if you aim your gun where you see the duck, the bird will be considerably ahead of that point by the time the shot arrives. You must shoot ahead of your target, and you must estimate that amount you shoot ahead quickly and accurately. If your estimate is not correct you will probably not have another chance at that bird.

The result is that from the very beginning it was necessary to build into the control system of the anti-aircraft gun some mechanical equivalent of a range table which would automatically allow the gun the necessary lead over the plane to make the shell and the plane come to the same place at the same time. To some extent this a purely geometrical problem, but in its finer developments it involves an improvement of our estimate of the future position of the plane itself. This must be estimated from the past positions, or at any rate from the observed past positions. The problem of predicting the future position of the plane is what the mathematicians call a problem of extrapolation.

My previous electrical engineering work had made me familiar with the theory of *operators*. An operator in this sense represents a device which will change a certain electric input into a corresponding electric output.

Mathematically speaking, the operator may be represented by a transformation formula, but not all such transformation formulas lead to operators which are physically realizable. The main condition for physical reality which we must impose on an operator is that the output should involve only the past and present of the input. It will be seen that the problem of shooting ahead of an airplane demands that the realizable operator approximate the future position of the plane which could, in fact, be ascertained only by a non-realizable operator. Only a prophet with the knowledge of the mind of the aviator could predict the future position of an airplane with absolute certainty, but there are often enough, in fact, means which will allow one to accomplish the minor task of a quite correct prediction.

The mathematical processes which suggested themselves to me in the first instance for prediction were, in fact, impossible of execution, for they assumed an already existing knowledge of the future. However, I was able to show there was a certain sense in which these processes might be approximated by processes free from this objection.

I do not wish to lose myself here in technicalities which will be understood only by scientists or engineers. However, I did in fact consider certain possibilities of approximating to non-realizable operators by realizable operators. I suggested these notions to Professor Caldwell, who was normally in charge of M.I.T. work on Bush computing machines, and who was now engaged in applying these machines to war problems. After the custom of those times, Caldwell immediately put a classification on my ideas, so that thereafter I could no longer speak freely of them to anyone with whom I wished to talk.

For a trial setup for my problem, Caldwell and I were tempted to make use of Bush's differential analyzer because of the ease of assembling its parts for the simulation of a large range of different problems. In this the differential analyzer resembled a Meccano set; and in fact, when the English tried to follow in Bush's footsteps and construct a differential analyzer, they used ordinary Meccano parts with very creditable success.

We made several experimental runs with different settings of our apparatus, and we found that those which we had considered in advance to be better actually were. Our instrument was an assembly of a number of adding devices, multiplying devices, and integrating discs.

At this stage, prediction theory was made a government project, and a young engineer, Julian Bigelow, who had worked some time with International Business Machines, was assigned to the problem. This was the beginning of a long collaboration between us. Bigelow is a quiet, thorough New Englander, whose only scientific vice is an excess of scientific virtue.

He is a perfectionist, and no work that he has ever done is complete enough in his eyes to satisfy him.

He used to be an enthusiastic aviator, but this sport became impossible in war time and is, at any rate, too expensive for the average man. Most accidents of private flying are not serious, in the sense that the aviator can walk away from them unhurt, but there are no minor repairs to an airplane. They must be done by certified mechanics and have the O.K. of the Civil Aeronautical authorities. Since they generally occur at remote points, they constitute a real burden to the pocket. For many years, Bigelow nursed a series of old and decrepit cars. For the ordinary automobilist a car is an instrument to get somewhere, but for the enthusiastic gadget man it represents a challenge to his ability to overcome difficulties. Such an engineer will never be content with a car that functions normally. He is either trying to construct a super car or he is exercising his ingenuity in making run a car which, by all the canons of the motorist, should have been consigned to the junk heap years ago. If you ride with such a motorist, you will be safe from accidents of any consequence, but you will never, never travel without adventure. I remember one occasion when Von Neumann was interested in consulting with Bigelow as a possible engineer for a computing machine project. We telephoned from Princeton to New York, and Bigelow agreed to come down in his car. We waited till the appointed hour and no Bigelow was there. He hadn't come an hour later. Just as we were about to give up hope, we heard the puffing of a very decrepit vehicle. It was on the last possible explosion of a cylinder that he finally turned up with a car that would have died months ago in the hands of anything but so competent an engineer.

Bigelow and I began to try to ascertain the limitations of our prediction method, for it was almost certain that we should find serious limitations. This time, instead of trying out our predictor on a smooth curve, we tried it on graphs made of two straight lines joining each other at an angle.

It must be understood that the predictor consisted of one member which was made to follow a given curve and of another member which, on the basis of these past data, was expected to indicate the curve a little further in the future. This second member we will term the follower. When we put our apparatus to work on a curve which was not smooth, but in which one straight segment of a line was succeeded by another segment at an angle with the first, the predictor still worked, but in a very peculiar fashion.

What was interesting and exciting, and in fact not unexpected, was that the pieces of apparatus designed for best following a smooth curve were oversensitive and were driven into violent oscillation by a corner. We tried to test this repeatedly, and we always got the same result. Then the idea suggested itself to me: perhaps this difficulty is in the order of things, and there is no way in which I can overcome it. Perhaps it belongs to the nature of prediction that an accurate apparatus for smooth curves is an excessively sensitive apparatus for rough curves. Perhaps we have here the example of the same sort of malice of nature which appears in Heisenberg's principle, which forbids us to say precisely and simultaneously both where a particle is and how fast it is going.

The more we studied the problem, the more we became convinced that we were right and that the difficulty was fundamental. If then we could not do what we had wanted but had not really hoped to do (that is, to develop a perfect universal predictor), we should have to cut our clothes to fit our cloth and develop the best predictor that mathematics allowed us to. The only question was: What did we mean by the best predictor? If errors of inaccuracy and errors of hypersensitivity always seemed to be in opposite directions, on what basis could we make a compromise between those two errors?

The answer was that we could make such a compromise only on a statistical basis. For the actual distribution of curves which we wanted to predict, or let us say for the actual distribution of airplanes that we wanted to shoot down, we might seek a prediction making some quantity a minimum; and the most natural quantity to choose at the start, if we should be guided by easy computation, if not military significance, was the mean square error of prediction.

This means that we took the square of the error of prediction at each time, or in other words, the square of the difference between the predicted value and the true value. We then took the average of this over the whole time of the running of the apparatus. This average of the square error was what we were trying to minimize.

Thus we could set up the prediction problem as a minimization problem and give it a definite mathematical form once we made certain assumptions concerning the statistics of curves to be predicted. The branch of mathematics dealing with the minimization of quantities associated with curves is known as the calculus of variations, and it has a very well-known and well-recognized technique. In many cases it leads to setting up a certain differential equation for the function or curve which is to fulfill the minimization condition, but there are cases (and this case of ours was one of them) where it leads to the related sort of equation known as an integral equation.

This was lucky for me, for integral equations were well within my field of interest; but the even luckier thing was that the particular integral equation to which the problem leads is a slight extension of the one which had been considered by Eberhard Hopf and myself. The result was that not only was I able to formulate the prediction problem but also to solve it; and what was even luckier was the fact that the solution came out in a simple form. It was not hard to devise apparatus to realize in the metal what we had figured out on paper. All that we had to do was make a quite simple assembly of electric inductances, voltage resistances, and capacities, acting on a small electric motor of the sort which you can buy from any instrument company.

We made an apparatus which translated the height of a point above a given base line into an electrical voltage. We passed this voltage, which varied with the time, through an electrical combination of resistance wires, condensers, and magnetic coils. At another point in the system, we took off the voltage and measured it continuously by a voltmeter. The actual type of voltmeter which we used gave us a continuous graph of the output voltage. It was this output which was to serve as a prediction of the voltage a certain length of time in the future.

The next problem which I had to take up concerned prediction in the case in which the data from which we were to predict were not precisely given. This also led to a minimization problem, in which we had to specify not only the statistics of the data which we were supplied but also the statistics of the errors at the same time. This minimization problem led to another Hopf-Wiener equation. This was solvable by the same methods, and we obtained a very satisfactory theory.

In scientific work it is not enough to be able to solve one's problems. One must also turn these problems around and find out what problems one has solved. It is frequently the case that, in solving a problem, one has automatically given the answer to another, which one has not even considered in the same connection.

This proved to be true in the new prediction theory. The concept of predicting the future of a message with a disturbing noise on the basis of the simultaneous statistics of the noise and message turned out to contain in itself the whole idea of a new method for separating noises and messages in what would be in some sense the best possible way.

This happened at a very opportune time, for the new technique of radar had come to meet serious difficulties. In radar too it was important to pick out a confused and faint message from the background of noise. Noise to the electrical engineer means not only noise that you hear but any unwanted electrical disturbance. For example, the flutter and flicker which you see in a badly regulated television set is noise. The messages which come in through a piece of radar apparatus and serve to confuse the image for which we are looking instead of to define it are known as noise.

The separation of noise from messages is the function of a wave filter. Wave filters go back many years in the history of telephone engineering and are pieces of apparatus which manage to free a message from part of the accompanying noise. Originally they were designed to pass all messages over a certain range of frequencies (or pitches) with as little change in their intensity as possible and to weaken other immediately adjacent frequencies by as much as possible.

When, with the precedent of telephone filters, such filters were built for television, and it was found that, after a certain point, the sharper the design of the filter, the worse it would function. Why was this the case? The answer is that the telephone filter is tailored to the specific characteristics of the human ear. The human ear is a very accurate instrument for perceiving pitch and a fairly accurate one for perceiving loudness, but it is a very poor instrument for perceiving what is known as phase; or, in other words, the precise time that the oscillation of the air passes through zero. An alternating current is represented, as I have said, not by one quantity which gives it intensity, but by two, which give it intensity and phase. The picture of an alternating current somewhat resembles the teeth of a comb. As I move the comb forward and backward along its edge, I change a certain quantity known as phase. In sounds, this phase change is not completely imperceptible, but it is not particularly important, and the earlier filters for telephone work and for other sound work did not pay much attention to differences in phase.

Radar, like television, appeals to the eye; and in the sort of message that radar or television transmits, the eye is quite as sensitive to phase errors as to amplitude errors. Thus, the phase distortion which the old telephone-type filter generated in radar and television was too high a price to pay for the excellent transmission of amplitude over a large range of frequencies. To minimize the total error in television and radar, it was necessary to cut down the phase distortion, at the cost of permitting a little more amplitude distortion than would by itself be best. In seeking for this balance between the two distortions, the method which I had suggested—although not ideal—would at least work, and was far better than any which had previously been used.

I do not mean to say that others had not become aware of the failure of the earlier forms of filter design for radar nor that these other people had not understood what was in essence the reason for this failure, but simply that my method gave for the first time a simple, compact, and reasonable way of attacking the problem on the level of fundamentals.

Bigelow and I started up a little laboratory to explore the possibilities of predictors. We had a staff of two men working with us. One was an excellent machinist and electrician, who put our ideas into the metal almost as fast as we could conceive them. The other was a computer who had been an accountant. Reader, if you ever have to start a computing laboratory, be warned by me and do not take as a computer an accountant, no matter how honest and efficient. Your computer must work to so and so many places of accuracy. This means so and so many significant figures, whether the significance of the digits begins six places before or six places after the decimal point. Your accountant works to cents, and he will work to cents until hell freezes over. Whatever numbers our accountant computed he kept at all stages to exactly two places after the decimal point, whether they were numbers in the millions, where even the first place to the left of the decimal point was of no possible significance, or numbers which begin only five places after the decimal point.

This was his conscience, that he should be accurate to the last cent; and he simply could not understand that physical quantities are not measured in cents but on a sliding scale of values in which the cents of one problem might be the dollars of another. In particular, when he had to obtain a small number as the difference between two nearly equal large numbers, he could never realize that these large numbers would have to be measured to a much greater degree of accuracy than that which was to be demanded for their difference.

I took my responsibility in this project very seriously. I tried to work against time, and that is a thing for which I am completely unsuited. More than once I computed all through the night to meet some imaginary deadline which wasn't there. I was not fully aware of the dangers of Benzedrine, and I am afraid that I used it to the serious detriment of my health.

Be that as it may, I found one very disagreeable fact, that the burden of secrecy in my project weighed heavily on me and that Benzedrine plays hob with one's ability to keep a secret properly. It superimposed on my not very secretive nature a garrulity which was completely unfitting at the time. I had to give it up and look for a more rational way of strengthening myself to bear the burdens of war work.

My work was under the supervision of Dr. Warren Weaver, of the Rockefeller Institute. We made several trips, Bigelow and I, to consult with him, and to compare our ideas with those of other people working on prediction theory and on the smoothing of anti-aircraft data. We traveled south two or three times to Fort Monroe, in Virginia, and to an army camp on the North Carolina coast. There we found workers from the Bell Telephone Laboratory who were more than eager to exchange ideas with us, and we pooled our resources with them and with other workers in the same field. At these meetings, after our travel and our hard work, I am afraid that I often went to sleep.

When we got back home, we made an experimental setup for generating the sort of irregular functions which arise in the aircraft-prediction problem, and then we designed a prediction apparatus on the basis of the statistical observations which we made on this setup. We actually were able to construct a predictor which would show the shape of a pattern of voltage in time, let us say, half a second before it occurred. This allowed us to check on our theory and find the criterion for a piece of apparatus that would give us good prediction.

The problem of generating an irregular curve with a statistically controllable degree of irregularity was quite interesting. We had reflected on the ceiling a spot of light, moving in a more or less periodic course. We tried to follow this spot with another, reflected in a mirror which was controlled by a certain apparatus. In this apparatus, the actual motion of the spot was not proportional to the turning of the crank which regulated it but to a rather complicated mixture of derivatives and integrals of this motion. Moreover, the crank was attached to a system of weights and springs, which was very far from giving the kinesthetic sensations which one would naturally associate with an apparatus of the sort. In other words, the spot had to be moved by a control which was complicated to begin with and, furthermore, felt completely wrong. Naturally, each person would respond to the apparatus in a somewhat different way; and we based our set of the predictor not merely on the general behavior of the apparatus but on the specific ability of an individual person to control the apparatus at a certain specific epoch in his training.

We were gratified by our clear and consistent results. On one hand, we had made a mechanical setup which threw a great deal of light on the way in which we act when we are confronted with an artificial problem and on the nature of humanly caused irregular action. On the other hand, we had found a way to duplicate in some degree the properties of the type of irregular motion of an airplane in flight. Thus we had some hope of the theory which could be used for the design of a practical apparatus for bringing down airplanes.

The importance of our ideas in connection with the control of antiaircraft fire was double. There are two human elements which must be considered in this control. On the one hand, when the airplane pilot is flying and taking evasive action, his pattern of flight has a great deal to do with not only the limitations of his plane but those of his nervous system, so that his action is not too different from that of the hypothetical human action we had designed. On the other hand, the anti-aircraft gunner uses a technique in which he cannot follow his target perfectly but in which he introduces certain random errors because of the limitations of his sense organs and muscles. These two sorts of human elements are combined as part of the semimechanical processes by which the anti-aircraft gunner brings down his target.

At the beginning of the war the only known method of tracking an airplane with an anti-aircraft gun was for the gunner to hold it in his sights by a humanly regulated process. Later on in the war, as radar became perfected, this process was mechanized. It became possible to couple directly to the gun the radar apparatus by which the plane is localized, and thus to eliminate the human element in gun pointing.

However, it does not seem even remotely possible to eliminate the human element as far as it shows itself in enemy behavior. Therefore, in order to obtain as complete a mathematical treatment as possible of the over-all control problem, it is necessary to assimilate the different parts of the system to a single basis, either human or mechanical. Since our understanding of the mechanical elements of gun pointing appeared to us to be far ahead of our psychological understanding, we chose to try to find a mechanical analogue of the gun pointer and the airplane pilot.

In both these cases, the operators seemed to regulate their conduct by observing the errors committed in a certain pattern of behavior and by opposing these errors by actions deliberately tending to reduce them. This method of control appeared to us not unlike a method already known in electric circuits and now being applied in servomechanisms, or systems by which we switch in an outside source of power for control purposes, such as occurs in the power steering of a truck. We call this negative feedback.

We use negative feedback in controlling the power input to the gun turret of a ship. When the direction in which the gun is pointing and the direction in which our computing apparatus says that the gun ought to point are different, this difference is used to regulate a power input to the turret, which will be of such a nature as to bring the turret more nearly into the desired position.

It is a maxim of the physiologist that the pathology of an organ throws a very great light on its normal behavior. We asked ourselves the question:

Does a negative-feedback apparatus have a specific recognizable pathology? Here we were on firm ground.

To see the general purpose of negative feedback apparatus, let us take the case of a gun turret controlled by a handle. If this handle works the gun turret directly, then the same pressure on the handle will produce very different results if the gun turret is cold and the grease is sticky from what it will produce if the gun turret is warm and the grease flows easily. It will produce different results if the gun is depressed, thereby increasing the moment of inertia of the gun turret, from those which we shall find if the gun is elevated and the turret has a small moment of inertia about a vertical axis. The primary purpose of feedback control on the gun turret is to make the response of the turret more nearly proportional to the push on the lever and thus less dependent on the variable friction, the inertia, and other external circumstances.

Not only is a feedback system less dependent on changes of load than a system without feedback, but this dependence becomes increasingly less as more and more of the motion is fed back—or, in other words, if the feedback is put through larger and larger amplification. However, this improvement in behavior does not continue indefinitely, for after a certain stage, with a large measure of amplification in the feedback, the apparatus will go into spontaneous oscillation and behave in such a wild way that we have decreased rather than increased the load independence of the apparatus. We expected that if human control also were to depend on feedback, there would be certain pathological conditions of very great feedback, under which the human system, instead of acting effectively as a control system, would go into wilder and wilder oscillations until it should break down or at least until its fundamental method of behavior should be greatly changed.

This suggestion, which emanated equally from Bigelow and from me, I brought to the attention of my neurophysiologist friend, Dr. Rosenblueth. He had not yet left for Mexico and was still Dr. Cannon's colleague at the Harvard Medical School. The specific question we put was: Are there any known nervous disorders in which the patient shows no tremor at rest, but in which the attempt to perform such an act as picking up a glass of water makes him swing wider and wider until the performance is frustrated, and (for example) the water is spilled?

Dr. Rosenblueth's answer was that such pathological conditions are well known, and are termed intention tremors; and that very often the seat of the disorder lies in the cerebellum, which controls our organized muscular activity and the level on which it takes place. Thus, our suspicions that feedback plays a large role in human control were confirmed by the well-established fact that the pathology of feedback bears a close resemblance to a recognized form of the pathology of orderly and organized human behavior.

Within the last two years I have had an experience which may be regarded as a commentary on the ideas I am putting forward here. Suddenly my little granddaughter, who was staying with us, developed a purpose tremor of exactly the nature of the one discussed here. We took her at once to the hospital and found that she was suffering from some form of encephalitis involving the cerebellum. This was a condition with very grim possibilities, but she had the splendid luck to make a perfect recovery without any aftereffects. If I were a superstitious man, this experience and many others reported by medical. men might make me suppose that a disease has a vicious personality and wished nothing more than to revenge itself on the scientist who has pursued it.

To go back to the work of our team of three: we wrote up these ideas in an article, but what was even more important was that Bigelow and I felt that we could safely go ahead with the treatment of the human links in the control chain as if they were pieces of feedback apparatus. Accordingly, we felt justified in proceeding from our crude experimental setup in the direction of the design of a complete apparatus for anti-aircraft control and prediction.

Since it was clear that the anti-aircraft control apparatus was in essence a feedback loop and contained in its construction many subsidiary feedback loops, we had to find out something of the characteristics of these loops. These characteristics were not available, so that the over-all apparatus which we designed was essentially crude and of unverifiable behavior. Under the circumstances, it was not considered advisable to proceed much further with it, partly as such tentative behavior as we could compute mathematically did not suggest an excellent performance.

Our ideas were eagerly taken up by other workers in the field and did lead to a very definite improvement in practice, in particular of the part that consisted in filtering out experimental errors of observation. We were not finally commissioned to perfect our own design, but I was in fact asked to write a book on time series, extrapolation, and interpolation. This book was reproduced in a photo-lithographed form which, because of the yellow paper in which it was bound, came to be known as the “yellow peril,” a name previously confined to the yellow books of Springer’s mathematical series. My textbook was very freely used, not only during the war by the designers of systems of control for the aiming and firing of anti-aircraft guns, but also by servo-engineers and electrical-communication men, both then and later. It was reprinted after the war in an amplified and improved edition with an appendix by Professor Norman Levinson, who made the use of my methods considerably clearer.

The work I did on the statistical treatment of anti-aircraft-fire control has led eventually to a general statistical point of view in communication engineering. The years that have gone by between then and now have secured the general acceptance of this point of view in communication engineering, but they have also led to much more. I might almost say that the whole of engineering is rapidly assuming a statistical aspect, and that this is passing over to less orthodox fields such as meteorology, sociology, and economics.

Let me go back to my earlier remarks concerning Willard Gibbs and the revolution he and his contemporaries created in physics. The orthodox Newtonian view of physical dynamics provides for certain equations involving rates, known as differential equations. With the aid of these equations and with a knowledge of the initial values, or values at time zero, of the variables whose rates are being determined in relation to their numerical values, we can inch our way along in developing the history of these phenomena. At each time we know our values. From these we determine their own rates of change, and this gives us an approximation to the same values and rates an instant later.

If we choose an instant of time that is sufficiently short, we may progress along the history of our phenomena to any point in time which we wish to attain. This is the method of the astronomers in calculating the orbits of the planets and the method of the ballistics expert for the study of the flight of projectiles in determining their paths.

In astronomy, as I have said before, the computation of these orbits is a very precise mathematics, and our initial data are very accurately known. This is not the case in most ballistic or engineering problems. In firing a shell, for example, the angle of elevation is known only to a very limited degree of accuracy, as are the weight of the projectile, the charge of powder, and the various atmospheric conditions. The result is that we start with none of this precisely given but with each set of data given only within a certain range. The traditional way of solving the ballistic equation is to assume the initial data as given precisely. Then we find the range, the angle of impact, the velocity of impact, and other significant quantities, and we immediately start to revise these with the aid of methods of interpolation or correction, reckoned by procedure which is entirely distinct from the first.

In this process we waste a good deal of effort, first in making our data unrealistically accurate, and second in correcting our imperfectly realistic results. There is, however, another method which is now coming into use, and which finds its spiritual father in Willard Gibbs.

Gibbs pointed out that when a dynamical system develops according to its own laws as, for example, when a top spins freely, something occurs very much like the flow of a fluid. To characterize a top, we need a point in a certain space, but this space is not the same as the familiar three-dimensional one of solid geometry. The position of a top requires six co-ordinates—or measurable quantities—to give its position and six to give its momenta; and these together form a twelve-dimensional array, which we may call by analogy a twelve-dimensional space. In this space there is a certain measure of volume, so that a set of tops which fill a given volume at one time will fill an equal volume at any other. This type of invariance of volume is to be found in all dynamical systems in which there is no power input or output.

This flow may be conceived as a flow of probability, and so it was conceived by Gibbs. The probability that a particle will be at one time in a given region of this peculiar space is the same as that at a later time it will be in the corresponding region into which the first has flowed.

Thus the typical equation of flow is no longer a general system of what is known as ordinary differential equations but a set of integral equations. These integral equations relate past distributions to future distributions, in such a way that if we superimpose different past distributions we superimpose the corresponding future distributions. A system like this, in which sums in the output answer to sums in the input, is known as linear, and the integral equations of the flow method of treating dynamics may be taken as linear.

The method is quite practicable computationally; and if the problem to be solved is of any high degree of complexity, this method may well be easier than the purely Newtonian method. Certain simplifications of methods of this type are now being used extensively by members of the mechanical-engineering department at M.I.T.

In addition to purely computational advantages in the more complicated cases, this method is also essentially superior to the Newtonian method of computation from the logical point of view. The reason is this: what we put into our problem not only consists of precise data which we later have to ease off in accordance with the inaccuracy of the equations and the initial conditions but contains intrinsically the very inaccuracy which hinders our work. We are thus not overcomputing and relieving the effect of this overcomputing by an *ad hoc* study of its errors but putting all our cards on the table at the beginning. What we finally get is what we want, neither more nor less. This cuts down a lot of unnecessary effort, but it also increases the real precision of what we are doing.

No scientific measurement can be expected to be completely accurate, nor can the results of any computation with inaccurate data be taken as precise. The traditional Newtonian physics takes inaccurate observations, gives them an accuracy which does not exist, computes the results to which they should lead, and then eases off the precision of these results on the basis of the inaccuracy of the original data. The modern attitude in physics departs from that of Newton in that it works with inaccurate data at the exact level of precision with which they will be observed and tries to compute the imperfectly accurate results without going through any stage at which the data are assumed to be perfectly known.

If we follow in these unprecise problems the sort of computation which the astronomer uses in determining the orbits of the planets, we may happen to choose initial conditions which lead to final results not typical of the wider range of initial conditions with which we have operated, and this instability of our orbit may drive us to a false reckoning of our ultimate error.

As I have said earlier in the description of my work on prediction, the more sensitive our instruments become, the more unstable they will be. These cause an error of a different sort from that of imprecision, but an error equally serious. What I have said of mechanical instruments is true of methods of computation. The balance between errors of imprecision and errors of instability is something which we can compute only on a statistical basis. Why not, then, assume the statistical basis at the very beginning and obtain both mean result and error by a unified method of computation?

If this recognition of the statistical nature of all science is already proving to be valuable in the most Newtonian type of mechanical-engineering computation, how much more must it then be the natural method of computation in those fields in which our errors of observation are naturally very large!

Let us consider meteorology as one example. We know a good deal about the dynamics of the atmosphere; and if our observations of the initial conditions were extremely good, we might expect that we could compute the future in a purely Newtonian way, even though this way is very likely to involve a great deal of overcomputation. What we actually know about the atmosphere, though, is a sampling taken in no more than three or four observations per day per hundred thousand cubic miles of the atmosphere.

Recently, under the influence of John von Neumann, there has been an attempt to solve the problem of predicting the weather by treating it as something like an astronomical-orbit problem of great complexity. The idea is to put all the initial data on a super computing machine and, by a use of the laws of motion and the equations of hydrodynamics, to grind out the weather for a considerable time in the future.

The catch is that all the observations of the weather bureau give only limited information at a very few points, with colossal gaps between them. These one can fill up only by some sort of statistical reasoning. Thus, an adequate meterological method must partake both of dynamics and statistics. There are clear signs that the statistical element in meterology cannot be minimized except at the peril of the entire investigation.

I do not mean to deny the importance of dynamics, but I do mean to assert the virtues of that Gibbsian approach in which this dynamics is treated as a statistical flow.

Meterology is typical of most of those numerical sciences which have come to the fore late in the history of science. In economics, the so-called econometric science of economic dynamics suffers under the radical difficulty that the numerical quantities which are put into the dynamics are not well defined and must be treated as gross statistical estimates. Who knows precisely how to define a demand, and how to measure it in terms which will satisfy most other economists? Can any two economists check on the amount of unemployment in the United States at a given time?

Econometrics will never get very far until two steps are taken: One of these steps is that the observation of the quantities—demands, inventories, and the like—with which econometrics operates must be subject to the same criteria of precision and rigor as the dynamics by which they are combined. The other is that we should recognize from the beginning the statistical and imperfectly precise nature of the quantities with which we operate and that we should go over to a Gibbsian treatment of them.

What I have just said about meteorology and econometrics applies equally to sociological dynamics, to biometrics and in particular to the very complicated study of the nervous system which is itself a sort of cerebral meteorology. It belongs to the very grammar of the use of mathematical methods in semiprecise sciences. It is the heart of the engineering of the future.

This new technique was foreshadowed in my war work on anti-aircraft fire-control predictors and was carried further in my development of communication theory. As yet it has penetrated to only a few initiates in the appropriate fields of scientific work, but it is philosophically right, and it bids fair to change the entire face of all the precise and semiprecise sciences.

When I first wrote about prediction theory, I was not aware that some of the main mathematical ideas had already been introduced in the literature. It was not long before I found out that just before the Second World War an important little paper on the same subject had been published by the Russian mathematician Kolmogoroff in the *Comptes Rendus* of the French Academy of Sciences. In this, Kolmogoroff confined himself to discrete prediction, while I worked on prediction in a continuous time; Kolmogoroff did not discuss filters, or indeed anything concerning electrical engineering technique; and he had not given any way of realizing his predictors in the metal, or of applying them to anti-aircraft-fire control.

Nevertheless, all my really deep ideas were in Kolmogoroff's work before they were in my own, although it took me some time to become aware of this. A series of papers by Kolmogoroff and such pupils of his as Krein continued to appear in the *Doklady (Reports of the Russian Academy of Sciences),* and although these papers still stuck for the most part to the concept of prediction previously developed by Kolmogoroff, somewhat narrower than my own, I am by no means convinced that Kolmogoroff was not independently aware of the possibility of some of the applications I had made. If that was so, he must have had to keep them out of general publication because of their importance for the military-scientific work of the Soviets. A recent paper by Krein, in which he makes an explicit allusion to my own work in the field of applications, convinces me of this.

I have never met Kolmogoroff, and indeed I have never been in Russia, nor have I been in correspondence with him or with any of his school. Thus what I say about him is largely surmise. At an early stage of my work for the United States military authorities, before I had seen Kolmogoroff's paper, the question came up whether anybody abroad was likely to be in possession of ideas similar to mine. I said that they would unquestionably receive no particularly ready reception in Germany; that my own friends Cramer, in Sweden, and Lévy, in France, might well have been thinking along similar lines; but that if anyone in the world were working on these ideas it would most likely be Kolmogoroff in Russia. This I said because of my knowledge that for twenty or thirty years hardly had either of us ever published a paper on any subject but the other was ready to publish a closely related paper on the same theme.

Within the last two or three years I have seen a Russian book on prediction theory, communication theory, and similar topics which makes extensive references to both Kolmogoroff's work and to my own. It gives

Kolmogoroff the priority, and although this priority is only partial, I have just said that there is good reason for considering him not only as an independent discoverer of large parts of the subject but as the first man to write on it. The book takes my own work very seriously and treats me much more fairly than I would expect in a Soviet book, international relations being what they are.

The "yellow peril" book is still playing an important role in American research work, both for military purposes and for more general uses. It was with the permission of the government that it has been reprinted, and it must have been a copy of this book which filtered into Russia and served as a basis for the Russian comments of which I have just written.

From this point on, my work, or rather the work of my group, has spread out to cover a very wide field of communication theory and practice. In the first place, the "yellow peril" is most definitely a statistical treatment of problems of communication. When the book was written, almost nobody had thought of communication in these terms. I think I am to be pardoned for a certain pride in saying that the statistical approach to communication theory is now accepted almost everywhere.

I approached information theory from the point of departure of the electric circuit carrying a continuous current, or at least something which could be interpreted as a continuous current. At the same time, Claude Shannon, of the Bell Telephone Laboratories, was developing a parallel and largely equivalent theory from the point of view of electrical switching systems. This represented a direct development of his previous work on the use of the algebra of logic in switching problems.

As I have said before, Shannon loves the discrete and eschews the continuum. He considered discrete messages as something like a sequence of yeses and noes distributed in time, and he regarded single decisions between yes and no as the element of information. In the continuous theory of filtering, I had been led to a very similar definition of the unit of information, from what was at the beginning a considerably different point of view.

In introducing the Shannon-Wiener definition of quantity of information (for it belongs to the two of us equally), we made a radical departure from the existing state of the subject. For many years it was believed that the carrying power of a communication line per unit time was to be measured by the band width it could carry.

A band width of 200 cycles was supposed to be able to carry twice as much information per second as a band width of 100 cycles. This supposition ignored the fact that, in the absence of noise, any band width would be enough to carry any amount of information in one second. One single voltage measured to an accuracy of one part in ten trillion could convey all the information in the Encyclopedia Britannica, if the circuit noise did not hold us to an accuracy of measurement of perhaps one part in ten thousand.

In the early days of the telephone art, very few lines were burdened with messages to the ultimate limit of their message-carrying capacity. As the art developed and the new modes of communication like radio and television demanded a more complete exploitation of the message space available, it became clear that the noisiness of the line or air channel is another important factor which we must take into consideration. The ether is full of disturbances which the radio man terms static, and no conductor, be it metallic or gaseous, can carry electricity in smaller lumps than the single electron. The irregularity of the stream of electrons is known as the shot effect, and it is an important consideration in all modern communication design.

It was only shortly before World War II that the load on communication channels became heavy enough for this intrinsic noise to become a serious practical barrier to the use of the lines for even more communication. Thus, the statistical point of view in communication theory, which I had anticipated so many years before with my generalized harmonic analysis, and which Shannon and I had jointly made fundamental at the beginning of World War II, became inevitable and basic shortly after the war had begun.

The work we were doing on feedback in connection with the fire-control machine and the nervous system introduced another revolution, which, like the first, has received universal acceptance in the course of the last few years. When I first came to Tech, electrical engineering was divided into two fundamental parts, which were known in Germany as the technique of weak currents and the technique of strong currents, and in the United States as communication engineering and power engineering.

The distinction between these two fields is valid, but the nature of this distinction and the place to make it was not understood for a considerable period. The generating station for a television sender or a trans-Atlantic radio sender may use relatively large quantities of power, but it is directed primarily toward communication; while the fractional-horsepower motor used in a dentist's drill may employ relatively small quantities of power. Nevertheless, the first piece of apparatus is primarily oriented with respect to the message and the second one with respect to the energy consumed.

At a period at which this distinction was not fully appreciated, servomechanisms for the control of gun turrets and other pieces of heavy apparatus were naturally assumed to belong to power technique rather than to communication technique. The whole tradition of power technique was to consider electric currents and voltages as varying in time, while the whole tradition of communication technique, particularly under the influence of Heaviside, had led to consideration of a message as a sum of a large or infinite number of different frequencies. It was not easy to see that the frequency treatment, rather than the time treatment, was just as appropriate for the servomechanism as for the telephone, the telegraph, and television.

I think that I can claim credit for pointing this fact out and for transferring the whole theory of the servomechanism bodily to communication engineering. My whole point of view in these matters made me regard the computing machine as another form of communication apparatus, concerned more with messages than with power. Its nature, as I saw it, was that of a series of switching devices, so enchained together that the information coming out of a number of stages of these was introduced into a subsequent stage as ingoing and regulating information.

It was clear that while these switching devices might be gear wheels and the like, they could equally well be mechanical relays or the electrical relays which depend on vacuum tubes and other electronic phenomena. I was much more disposed, as I have said, to use switching devices which made a choice between two alternatives than those which made a choice between ten alternatives, and I tried to bring this concept of computing machines to the attention of the engineering public.

It was at Harvard, under the supervision of Howard Aiken, that I found the first of the newer switching computers dependent on relays. Aiken was developing them under a government grant. I was much struck with Aiken's work, which I greatly admired, and which Aiken himself considered as the modern carrying-out of the crude computers developed by Babbage in England a hundred years ago. Babbage had formed an excellent conception of their mathematical possibilities but had almost no understanding of the mechanical problems to which they gave rise.

I was surprised to find that Aiken was completely committed to the relatively slow mechanical relay as the mechanical computer's first tool and that he did not put any enormous value on the speed which could be derived by the use of electronic relays. This limitation of point of view has now been discarded by Aiken himself, who has become one of the most active and original inventors and designers of electronic computers. But at that time he labored under a curious moralistic quirk in accordance with which he considered work with mechanical relays as essentially sound and right and work with electronic relays as unnecessary and ethically sloppy.

Here I wish to emphasize again a weakness of attitude joined with a great strength in those men who show practical ingenuity in the devising of gadgets. It is the desire to fix the technique of a subject forever at the precise point to which their ingenuity has carried them and then to offer a profound intellectual and moral resistance—a block, in fact, to later work which departs from their principles. We mathematicians who operate with nothing more expensive than paper and possibly printer's ink are quite reconciled to the fact that, if we are working in a very active field, our discoveries will commence to be obsolete at the moment they are written down or even at the moment they are conceived. We know that for a long time everything we do will be nothing more than the jumping-off point for those who have the advantage of already being aware of our ultimate results. This is the meaning of the famous apothegm of Newton, when he said, "If I have seen further than other men, it is because I have stood on the shoulders of giants."

Yet the commercial possibilities of the invention in the metal tend to blind the industrial worker to this fundamental fact and to make him hope that he can hold back the stream of progress at the precise stage where he had made his own contributions. The patent system and the commercial value of an inventor's idea as something salable tend to push him in this direction. This is not realistic. As a practical man, the inventor should have the very practical consciousness that for many years his greatest contribution will not be a single gadget but the furthering of the whole stream of thought and ideas concerning an enormous class of gadgets past, present, and future. He should come to terms with this streaming of thought and realize that, just as he has gone beyond those who were born before him, he himself and his work will have to serve rather as a stepping stone to the future than as the end to which science and technique must finally arrive.

However, my interest in the development of computing machines carried me far beyond those machines past, present, or to come, which are made of brass and copper, glass and steel. The brain and the nervous system also share in the main characteristics of computing machines. Parallel to the yes and no of a relay is the fact that a nervous fiber can exist in what are fundamentally only two states: the state of carrying a message and the state of not carrying a message. This is the so-called all-or-none law of the nervous system; and, although it may not be as precisely true as its crude, cold formulation would suggest, it is sufficiently true to represent a fundamental fact of nervous conduction.

A nerve fiber, it is true, may be stimulated by messages of varying intensity, but the ultimate fate of each of these messages is either to die out and fail to reach the end of the fiber or to continue as what the chemists would call a self-catalyzing process and start an impulse which will go from one end of the fiber to the other. When it has reached the end of the fiber, its subsequent history is so nearly independent of the original strength of stimulation that this strength may be entirely neglected. Thus, there is a certain analogy between a nerve fiber and a flip-flop electric circuit, an electric circuit with two, and only two, states of equilibrium. This analogy is so close that, long before the message reaches the end of the fiber, it carries its information in the form of a number of impulses rather than in the form of the strength of the impulses.

Not only are nerve fibers switching devices, but they are devices which lead into other switching devices. The nerve fibers communicate with one another by junction points or junction systems known as synapses, and in these the question whether a new message is established in an outgoing fiber depends on the precise set of incoming messages received from various fibers. In the simplest cases, the synaptic system has a threshold, which means that if more than a certain number of incoming fibers receive messages within a certain critical interval of time, the outgoing fiber fires, and otherwise not.

We are so used to feedback phenomena in our daily life that we often forget the feedback nature of the simplest processes. When we stand erect, it is not in the manner in which a statue stands erect, because even the most stable statue needs to be fastened to some sort of pedestal or it would fall over. Human beings stand erect, however, because they are continually resisting the tendency to fall down, either forward or backward, and manage to offset either tendency by a contraction of muscles pulling them in the opposite direction. The equilibrium of the human body, like most equilibria which we find in life processes, is not static but results from a continuous interplay of processes which resist in an active way any tendency for them to lead to a breakdown. Our standing and our walking are thus a continual jujitsu against gravity, as life is a perpetual wrestling match with death.

In view of this, I was compelled to regard the nervous system in much the same light as a computing machine, and I communicated this idea to my friend Rosenblueth and to other neurophysiologists. I managed to get a group of neurophysiologists, communication engineers, and computing-machine men together at Princeton for an informal session, and I found on the part of each group a great willingness to learn what the other groups were doing and to make use of their terminology. The result was that very shortly we found that people working in all these fields were beginning to talk the same language, with a vocabulary containing expressions from the communication engineer, the servomechanism man, the computing-machine man, and the neurophysiologist.

For example, all of them were interested in the storage of information to be used later, and all of them found that the word *memory* (as used by the neurophysiologist and the psychologist) was a convenient term to cover the whole scope of these different fields. All of them found that the term *feedback,* which had come from the electronics engineer and was extending itself to the servomechanism man, was an appropriate way of describing phenomena in the living organism as well as in the machine. All of them found that it was convenient to measure information in terms of numbers of yeses or noes, and sooner or later they decided to term this unit of information the *bit.* This meeting I may consider the birthplace of the new science of cybernetics, or the theory of communication and control in the machine and in the living organism.

I had hopes that this new science was going to pass through a rapid development over a broad front. The subject has developed greatly, and I have participated in its later phases. However, the times were not favorable for the normal growth of new ideas, and I have had to watch very carefully through a period where what I intended as a serious contribution to science was interpreted by a considerable public as science fiction and as sensationalism.

Science fiction is in vogue, and it is the fashion even among certain serious scientists to see merit in its writings. I myself as a child was a devotee of Jules Verne and H. G. Wells, to whom the present literature of science fiction owes its origin, but it is an infinitely slicker and more pernicious article. On the one hand, it leads to fantasies of power and of brutality quite as devastating as anything in the thud-and-blunder type of gangster story or the most uncomic comics. On the other hand, it is helping to create a generation of youngsters who believe that they are thinking in scientific terms because they are using the language of science fiction. It is a real difficulty in our schools of science and engineering to have to try to educate young men who believe that they have a calling toward science merely because they are accustomed to playing with the ideas of destructive forces, other planets, and rocket travel.

This vicious daydreaming is largely a product of World War II, which has done so much to demoralize the whole generation of science. The period of the war was one in which the status of science and that of mathematics were changing rapidly. In the first place, leisure was vanishing in every sector of life. Before the war, I used to find the M.I.T. boys playing a game or two of bridge after lunch in one of the lounges of Walker Memorial. I often participated in these games.

I did not regard the time as wasted either by myself or by the students, for between games we used to have an occasion for wide-ranging discussions which might be pure bull session or might involve a real play of ideas. From the beginning of the war, everyone was in deadly earnest, and all chance of intellectual play was restricted. To the present day, it is hard to find young men who dare to take enough time off from work to consider what their work is about. The hours spent in the fantasy of space books are no replacement for a good bull session.

Before the war, and particularly during the depression, positions in science were not easy to get. The requirements for these positions had become exceedingly high. During the war, this situation had changed in two respects. First, there were not enough men to carry out all the scientific projects which the war involved. Secondly, in order to carry out these projects at all, it became necessary to organize the work so as to use those with a minimum amount of training, ability, and devotion.

The result was that young men who should have been thinking of preparing themselves in a long-time way for their careers lived in a lighthearted way from hand to mouth, confident that the existing boom in scientists had come to stay. Such men were in no state to accept the discipline or hard work, and they evaluated whatever intellectual promise they might have as if it had been already realized in performance. With the older men crying out for assistance and manpower, these boys would shop around for those masters who would demand least and grant them the most in indulgence and flattery.

This was a part of a general breakdown of the decencies in science which continues to the present day. In most previous times, the personnel of science had been seeded by the austerity of the work and the scantiness of the pickings. There is a passage in Tennyson's "Northern Farmer: New Style" which says: "Doänt thou marry for munny, but goä wheer munny is!"

Thus, an ambitious man with slightly anti-social tendencies or, to put it more politely, indifferent to spending other people's money, would formerly have avoided a scientific career as if it were the plague itself. From the time of the war on, these adventurers, who would have started out as stock promoters or lights of the insurance business, have been invading science.

The old assumption which we used to make must be discarded. We all knew that the scientist had his vices. There were those among us who were pedants; there were those who drank; there were those who were overambitious for their reputations; but in the normal course of events we did not expect to meet in our world men who lied or men who intrigued.

When I began to emerge from my sheltered life into the scientific confusion of wartime, I found that among those I was trusting were some who could not be held to any trust. I was badly disillusioned more than once, and it hurt.

The meeting to consolidate communication theory took place well after Pearl Harbor. It may surprise the reader that in all this talk about war work, I have not mentioned Pearl Harbor and the actual entry of America into the war. The fact is that all of us had long been convinced that the war was coming to America in some form or other, and the actual opening of hostilities did not change my work on means of defense.

In the fall of 1941, the tension of the successive defeats experienced by the Allies in Scandinavia, Holland, and France, the Battle of Britain, and the ambiguous and to-and-fro situation in North Africa, had grown as great as one thought one could bear, yet it became complicated by the fairly general feeling that something was about to blow off in Japan. While none of us was exactly prepared for Pearl Harbor, I do not think that we were convinced that a military dictatorship like Japan would play the game of war and diplomacy according to the standard rules, particularly when these rules were manifestly advantageous to us. Thus, Pearl Harbor came, to me at least, as much more of a shame and a humiliation than a surprise.

Pearl Harbor and our subsequent entry into World War II on both sides of the globe had a number of direct effects upon me. It is true it could not involve me in war research any more than I was already involved, because I was fully occupied in this direction and had been for more than a year. However, the war wiped out plans which Manuel Sandoval Vallarta and I had made to go to South America in the interest of international good will, on funds primarily emanating from the State Department (or, in his case, from the Mexican Government).

What I felt to be much more important to me was what was going to happen to my dear friends the Lees. We had just managed to secure for them passage on a liner from Hong Kong. Then Pearl Harbor came and prevented the boat from sailing, or at least from sailing with our friends on board. Thus, Lee, who had already gone for five years through the Chinese-Japanese War without any adequate contact with his profession, was sentenced to wait five years more, until after V-J Day, before we could bring him over to the United States. During all that time, an offer which we had secured him from the electrical-engineering department of M.I.T. stood open, or at least ajar; and when he finally arrived he was able to step into an instructorship. This has now been succeeded by an assistant professorship and an associate professorship.

The problem of what professional man is to do when he comes back as a sort of Rip Van Winkle who has slept for a decade and wakes to find a changed world is very difficult. The obvious thing might appear to be to spend a year or two in studying the various developments of the intermediate period. This must be done to some degree or other, but it is not a completely adequate way of treating the situation. The very bulk of new material tends to produce an intellectual indigestion in the student. He must come into competition with the younger generation who have learned the field the easy way while it has been developing and who are at home in it. Our Rip Van Winkle cannot expect to compete with them.

What made Lee's situation easier was that I had recently developed a considerable part of the statistical theory of communication engineering in the "yellow peril." I pointed out to Lee that the one way to avoid being disastrously behind the game was to move deliberately ahead of the game and thus secure an advantage of some years while the other people were catching up. Lee saw the point.

The situation was made considerably easier by the fact that for years we had been so much in the habit of working together that my mental processes and ways of writing were quite familiar to him. Thus, he took over the problem of working out in detail the communication and engineering consequences of ideas which I had only sketched in general terms and of making himself interpreter to the engineering public of the field which I was later to call cybernetics.

Lee has established himself in this program and has been busy for some years in carrying it through to a most successful conclusion. He is now writing a book on communication engineering from the new standpoint, and he is showing in it a great patience, thoroughness, and consideration of the reader. For me, close as I am to the origin of the subject, such a detached treatment would be impossible.

Lee has presented the new ideas to quite a number of government and industrial laboratories. He has brought up a whole generation of young electrical engineers to do research along statistical lines and to employ my point of view as a habitual approach to communication problems. He has also organized very successful summer meetings, so that engineers already actively engaged in the communication industry have been able to come to M.I.T. for a refresher course on the cybernetic viewpoint.

In these ways the difficulties of ten years' isolation have been bypassed successfully. The head start which Lee has had in the new methods has allowed him time to catch up with what has happened between 1936 and 1946, and, what is more, the work in which he has been engaged has given him specific problems to use as a touchstone for his understanding of and familiarity with the research of the intermediate period. In other words, we have seen in the pay-off of the policy which the two of us began to adopt the moment the Lees arrived in Boston's South Station after the war.

With Lee back at M.I.T., I was greatly encouraged to go on with further investigations of servomechanisms and the entire class of topics which I was later to give the name of cybernetics. As I have said, Lee is himself now finishing a book on the matter. However, not all that the two of us could do together on that, nor all that any hundred people could do, would suffice to cover more than a small part of the literature on servomechanisms and on the automatic factory, to which our early work has led. The automatic factory bids fair to become the norm rather than the exception, even within the lifetime of those who are now in college. It is giving rise to a new profession of experts who are able not only to design these factories but to set up on them problems of the most varied sort. The modern technique of automatic-factory design is well beyond the ambit of a theoretical man like me. As I shall show in a later chapter, I have conceived it to be my primary function not so much to develop the automatic factory further as to explain its nature and its consequences, and to alert both labor and management to the need of facing these intelligently.

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As the war went on, the work left the hands of the pure scientists like myself for those of the designers, and I was at loose ends. It was about January 1944 that I heard from Vallarta that there was to be a meeting of the Mexican Mathematical Society in Guadalajara the following spring and that they wished a good attendance from the United States, and my own presence in particular.

Almost from the moment I crossed the frontier, I was charmed by the pink-and-blue adobe houses, the bright keen air of the desert, by new plants and flowers, by the indications of a new way of living with more gusto in it than belongs to us inhibited North Americans. The high, cool climate of Mexico City, the vivid colors of the jacaranda blossoms and the bougainvillaeas, the Mediterranean architecture, all prepared me for something new and exciting. The many times I have returned to Mexico have not belied these first impressions. It will be a sad day for me when I come to feel that I have no further chance to renew my contacts with that country and to participate in its life.

The Rosenblueths met me when I arrived and saw that I was put up in interns' quarters at the Instituto Nacional de Cardiología. As soon as I had begun to acclimatize myself and to get over the terrible lassitude due to the elevation, Arturo and I started to work together on the sort of muscle tremor known as *clonus*: the familiar spasmodic vibration which many people experience when they sit cross-legged with one knee under the other. It seemed to be an admirable case for studying feedback tremor in the neuromuscular system.

I had begun my trip with very little Spanish, what I had being no more than a general knowledge of Latin and the Romance languages and two or three lessons from a Mexican student at M.I.T.

I brought a Spanish grammar along with me, and I tried to make my very inadequate Spanish serve as a medium of communication with my interne companions, most of whom spoke a very tolerable English. I used to dine with the interns in the official doctors' dining room, and I found that I gradually developed a taste for the strong pepper sauce, which seemed to be the invariable accompaniment to all dishes. They were very friendly to me and very courteous at the same time, for both the Spanish language and the Mexican tradition permit a rather close combination of courtesy and familiarity. Their term for me was *maestro,* which is applied both to a teacher and to a craftsman such as a carpenter or a mason. As applied to a teacher, it is both far more familiar and far more respectful than *señor.*

I played a lot of chess with the young doctors and occasionally with Arturo, although I think his estimate of me as a game player was better indicated by the Chinese Checkers which we played continually when I visited him. I used to walk uptown and make purchases. For this I had to wait until my blood count had risen enough to overcome the functional anemia which besets all new visitors to Mexico City.

I learned a great deal more about Arturo during this visit. He had not started as a scientist but as a musician, and had earned his living for some time by playing classical piano music in a Mexico City restaurant. Arturo is also a first-rate chess player and a superb bridge player, so good that in neither of these games does he allow me to play with him. He is a great enthusiast for the climate and arts of his native land. In this I cannot gainsay him, although I think that he often tends to underrate the New England countryside and, for that matter, the country in general, as opposed to the city.

After Mexico, the land of which he speaks with the greatest admiration is France, where he did most of his medical studying. He would find quite enough pleasant things to say about New England if only he were speaking to a non-New Englander, but there is something puckish in him which makes him take a pleasure in teasing.

He is a hard worker, and he makes the greatest demands on the sincerity and industry of those about him, demands which are only exceeded by those he makes upon himself. According to my way of looking at things, he involves himself too deeply in the expected outcome of a particular piece of research, so that if in fact it comes out differently, he will be disproportionately worried and spend excessive effort at trying to salvage that which has already proved itself unsalvageable.

Our work on clonus progressed in a manner which was satisfactory to me, but which did not come up to Arturo's rigid requirements for experimental research. The paper has never been published, although I think that many of its ideas have come to be accepted generally.

Besides our study of clonus, we also did some work on the heart as a conductor of rhythmic contractions. This work was later carried to a greater degree of perfection by another one of Arturo's collaborators.

As soon as I could get about freely, I looked up the Vallartas and the mathematicians at the university. Manuel and his wife, Maria Luisa, gave us a warm welcome to Mexico. They lived with a number of brothers, sisters-in-law, nieces, and nephews, in the large house of Maria Luisa's parents, the Margains, on Avenida Insurgentes. Father Margain, who died recently, was a doctor, and his sons were doctors, architects, and lawyers.

Like a number of Mexicans in their circle, my friends Manuel and Maria Luisa were connected with the saga of Maximilian. It had been a great-uncle of Maria Luisa, Lieutenant Margain, who at Querétaro had commanded the squad which shot Maximilian and his two generals Mejía and Montemar. On the other hand, one of her great aunts had been lady-in-waiting to Carlota. An ancestor of Manuel had been governor of a province and a political mainstay of Maximilian's antagonist and conqueror, the Indian president Benito Juárez. Thus, my visit to Mexico made me feel at once as if I had been immersed in the fascinating and violent history of the country.

Another Mexican scientist friend of mine was Nápoles Gándara, who is professor of mathematics at the University of Mexico, and who invited me to lecture there. He had been up to M.I.T. to study with Dirk Struik. His main distinction is not so much the merit of his original research as the way in which he has stood up for many years for mathematics and the training of mathematicians. He is largely and possibly entirely of Indian blood, and he possesses a full share of that modest determination and oneness of purpose which derives from his Indian ancestry.

The Indian element in Mexico and the Spanish differ in many ways. It is the Spaniard rather than the Indian who has the romantic *élan* which we associate with the South. On the other hand, the Indian is unsurpassable where steadiness, loyalty, and conscientiousness come into consideration. Thus, each race furnishes qualities which the country needs. It is a splendid thing that the Indian has come to his own and is participating in the development of a new middle class, which stands on the tripod of the Spaniard, the Indian, and the foreigner.

My Mexican friends came from all of these three elements of society, and it was an exciting thing to see how this new middle class of diverse origins constituted a cordial, friendly, and well-organized body of people.

My friend Erro, the astronomer, took me to see Torres Bodet, the Minister of Education, who was doing much for the education of illiterates. One expects that when, in a Latin country, the head of a national observatory introduces a foreign scholar to the Minister of Education, it will be an affair of frock coats, striped trousers, and great formality. As a matter of fact, Erro appeared in riding breeches and a blue-and-white sweatshirt. Mexico has a charming combination of Spanish and Latin American formality with the free and easy ways of the United States.

I saw a great deal of the medical crowd in Mexico City as well as of the mathematicians, physicists, and astronomers. Everywhere I found a new and active intellectual life in the making. The Mexicans are quite conscious of the distance they must go to come up to the level of the countries older in scientific reputation, but they are determined to make up for their late arrival in scientific history, and the level of work increases year by year. In the meanwhile, there is something peculiarly charming in the friendliness and warmth of heart and in the intellectual devotion of these friends of mine, and Mexico will never seem to me a truly foreign country.

Among my medical friends I wished to speak particularly of Dr. García Ramos, for he in his own personality is a living example of the new Mexico. He was born in Querétaro, of parents in quite modest circumstances, and is overwhelmingly Otomi Indian in blood. He went into the army as a boy. At every stage in which examinations or ability could promote a man's career, he stood out in the first rank. He was sent to the army medical school, from which he was graduated with real distinction. Arturo took him on as his assistant.

García Ramos is now a well-known physiologist in his own right, having received a Guggenheim Fellowship to study in the States. He was a major in the army when I first knew him, and he is now a full colonel. He has retired from active military service, and is at present the head of the Mexico City Nutrition Laboratory. There is no future for a general who does not make the army his first consideration, and García Ramos is infinitely more interested in medical research than in the army. The army has therefore nothing further to offer him.

Among Mexican mathematicians the late Professor G. D. Birkhoff of Harvard has had a great influence. Some years before, he had developed an alternative explanation of certain phenomena which occupy a key position in Einstein's gravitational relativity. Birkhoff's theory, which is not actually relativistic, is meant to account for the displacement of light by the attraction of the sun for certain anomalies in the orbit of Mercury and for the shift of light from the remote corners of the universe toward the red end of the spectrum. At the time at which Birkhoff was working, several of the Mexicans were up in Cambridge in contact with him. They are now passing Birkhoff's influence on to their own students. The new subject became a favorite with the young Mexican school of mathematics, and paper after paper was written on Birkhoff's work. After he died in 1942, the Mexicans continued to carry on the work as a tribute of piety.

I could go on from name to name of my friends without finding a chance to refer to much more than a small part of them, but there is one person whom I must mention in particular, our janitor, Olvera, a tall, gaunt peon, who possesses the instinctive culture which one so frequently finds in strata of Mexican society which tend to be illiterate. By this I mean in the first instance a pride in a correct and elegant speaking use of the Spanish language. Indeed, Olvera himself is not illiterate. He has taken full advantage of his situation as an employee of an institution of learning to extend his culture in many directions. In particular, together with several of the younger doctors and with the stenographers, he has joined an English class conducted by Mrs. Rosenblueth. He has been one of her most conspicuously successful pupils. Now he is able to speak in an English which has the same chosen quality that belongs to his Spanish, and on one occasion he is said to have remarked to two skylarking American boys in the laboratory, "Gentlemen, this conduct is not worthy of an international scientist." Indeed, Olvera's choice of words, both in Spanish and in English, is so notable that when Arturo and I have any matter of phraseology come up in a paper we are writing, we say, "How would Olvera have said it?"

Olvera is utterly devoted to the laboratory and to Arturo, and this devotion may even become embarrassing at times. As a vigorous man, Arturo naturally prefers to go like anyone else to the barbershop or the bootblack; but no, Olvera will not let him. The barber and the bootblack must come to his office at those times when Olvera has decided that the personal appearance of the boss needs a bit of renovating, and Arturo sits embarrassedly upon his chair while the door is closed on his humiliation.

The particular pride of the beautiful modern building of the Instituto is the pair of mural paintings by Diego Rivera on the history of the medicine of the heart. For all Rivera's personal flamboyancy, these paintings show a great depth of serious study and scholarship. The pieces of medical apparatus which appear are all correctly delineated. Whether they are of the accepted pattern or not, they all would work. To achieve this, Arturo and Rivera consulted with each other many times.

However, the artistic merit of these paintings goes far beyond such technical details. One of them, which concerns the earlier history of cardiology, has a generally red tone, emanating from the pile of fagots at which Servetus was burned. It must be remembered that, besides being a heretic in the eyes of John Calvin, he was one of the discoverers of the circulation of the blood. Naturally a man like Rivera takes a delight in showing that the burning of heretics was as much a misdeed of the Protestants as of the Catholics.

The other painting is largely blue in coloring, from the light emanating from the Roentgen tube and the other electrical apparatus of the modern cardiologist. In both there is a careful depiction not only of the individuals who have contributed to the science but of their distinct national types. Many single faces or groups are portrayed with great understanding and emotion, and in particular there is a splendid piece which shows the consumptive Laënnec using his stethoscope on a dying heartsick patient. You see the pose of the sick doctor echoing in every line that of the patient, who shows the characteristic Hippocratic face.

The time came for me to go to the mathematical meeting at Guadalajara. I will not say that the mathematics at this meeting was either terrifically novel or very exciting, although it represented a genuine attempt for a country which was rather a newcomer to the mathematical field to do work on a high level. The meeting itself and the city in which it took place were equally charming.

We had quite an American contingent at the meeting, both invited and self-invited. One of the invited guests was Professor Murnaghan, of Johns Hopkins, who found a not unusual difficulty in adjusting himself to the diet of the country. One morning the Rosenblueths came down early, and Rosenblueth said in English to nobody in particular, "I feel fine, period!" I answered, "Murnaghan feels rotten, colon."

We made many excursions about Guadalajara and its surroundings. I was particularly delighted by the sincere and manly painting of Orozco.

There was a great painting of his in the Governor's Palace, in which he represented the wars between Fascism and Communism in a powerful but brutal symbolic fashion. However, the most interesting group of his paintings was in the Hospicio. This was a public orphanage which seemed to me more human and far less institutional than most boarding schools. The children were not in uniform but played in ordinary dress with an abundance of toys in the shady and tree-filled courtyards. There were two school orchestras, one of older and one of younger inmates, and the conductor and music teacher, who had achieved remarkable results on a truly professional level, was an elderly Indian gentleman with the impassive face that one has learned to know in the portraits of Juárez.

It was the chapel of this institution that had been decorated by Orozco, and although the paintings were not the conventional paintings of the Christian tradition, they were most certainly religious paintings, and in the main representations of a new Apocalypse. They had some of the flat red and blue colors of El Greco. However, the use of color was not their strongest point, and the drawing was extremely modern. One of them showed the wheel crushing the city wall of the Aztecs. The moral of this was that the Western civilization of the wheel, which the Aztecs had never known, had crushed the indigenous cultures. Other panels carried out the theme of the conquest and showed the Spanish soldiers with their swords and the monks with their robes. Through the nave of the whole great church of centuries ago, these paintings carried a spirit of grim beauty and power, and one felt, for all its grimness that it was a worthy background against which the children could develop their sense of the nobility of art.

We members of the congress witnessed a dance exhibition by the school teachers of Guadalajara, both men and women. They had nothing schoolmarmish or schoolmasterish about them. The whole show went off with a verve and sincerity which excited our greatest admiration.

When I returned to the States I found that the interest in the sort of work that Arturo and I had been doing together, namely the application of modern mathematical techniques to the study of the nervous system as a problem in communication, had excited a spirited interest. A colleague of mine had persuaded the Macy Foundation, in New York, to organize a number of meetings devoted to this subject. The series ran for several years. Here a group of psychiatrists, sociologists, anthropologists, and the like came together with neurophysiologists, mathematicians, communication experts, and the designers of computing machines, to see if they couldn't find a common basis of thought.

The discussions were interesting and, in fact, we did learn to speak more or less in one another's language, but there were great obstacles in the way of a complete understanding. These semantic difficulties resided in the fact that on the whole there is no other language which can give a substitute for the precision of mathematics, and that a large part of the vocabulary of the social sciences is and must be devoted to the saying of things that we do not yet know how to express in mathematical terms.

Indeed, I found then, as I have found on so many other occasions, that one of the chief duties of the mathematician in acting as an adviser to scientists in less precise fields is to discourage them from expecting too much of mathematics. They must learn that there is no intellectual virtue (and that there is, in fact, a severe intellectual vice) in using a number of three digits when our available accuracy runs to one digit. Thus, while we were quite convinced that the same modes of thought traverse the problems of communication—whether they be social, physiological, or mechanical—it was the mathematicians rather than the physiologists and sociologists who had most to throw cold water on an overestimation of the detailed possibilities of mathematics in these other fields.

Arturo attended several of these earlier Macy meetings. We wanted to continue to work together in the intimate way in which we had already started and to secure a backing for this future work that would enable us to keep it up together for a number of years. We managed to interest both M.I.T. and the Instituto Nacional de Cardiología in the project and to secure funds in New York from the Rockefeller Foundation. Here Warren Weaver, who had now returned to his normal work from his war duties, was very enthusiastic and hopeful about the possibilities that had emanated from my research on prediction. He represented in this the natural sciences group of the Rockefeller Foundation. Dr. Robert Morison, who represented the biological sciences group, was also interested in the proposition. He was a close friend of Arturo's and had been a member of our dinner group at the Harvard Medical School. Between him and Weaver, we got the signal to go ahead.

M.I.T., the Instituto, and the Rockefeller Foundation came to a decision that I should spend half of every other year in Mexico and that, on the other hand, Arturo should spend part of the intervening years at M.I.T., for a period of five years. With slight modifications we have adhered to this program, and there now remains only one half year of the original program for Arturo to spend in Boston.

Besides the work Arturo and I did on heart conduction and on clonus, there is a group of biological researches, on some of which he and I have worked together, and some of which have received my independent attention. Most of these have not been carried through to a definitive result, but they still present features of interest for further work.

One piece of work I did with Rosenblueth concerns an attempt to set up and to solve the differential equations of impulse flow along a nerve, and in this manner to compute the passing distribution of electricity which occurs as an impulse goes by. This is the so-called theory of the nerve spike. This sudden rise and sudden drop of potential in the passing of a nerve impulse seemed to me to divide itself into at least three separate consecutive phenomena.

Another research which we undertook together had to do with the statistical theory of the conduction of impulses through a synapse, or a place where incoming nerve fibers join with fibers proceeding further in the nervous system. This was done during one of Dr. Rosenblueth's stays in Cambridge.

Two other researches which have not yet matured but which seem to me to lead in a promising direction have been undertaken by me in collaboration with workers in the electronics laboratory of M.I.T., in particular with Dr. Jerome Wiesner. One of these, in which the leading idea is Wiesner's, concerns an attempt to analyze sounds instrumentally in such a way that the pattern of sounds might be conveyed to the skin as a series of local pressures or vibrations. We made promising headway in the matter but did not come to a definite choice as to just how to pursue the best path in making such apparatus available to the deaf as an alternative mode of registering sound by touch.

This represents one phase of my general interest in the philosophy of prosthesis. I have believed that much could be done with artificial limbs by realizing that the deprivation of the amputee is quite as much sensory as motor, and that the amputee's loss of part of the information which is available to the normal person leaves him to assume a condition which parallels not merely paralysis but ataxia. Ataxia, which is a loss of steering impulses, does not prevent a person from moving, but prevents him from moving in a purposive way, by depriving him of an awareness of his own motions.

Closely related to this concept is the idea of a more adequate iron lung for paralytics. The existing iron lung has saved many lives, but it tends to make the patient dependent on a rigid process of breathing over which he has no control, and it tends to cause him to unlearn the normal process of breathing. There seems to me a real possibility to take off electric signals from such breathing muscles as are not completely dead and to amplify them so that the patient might have the satisfaction of controlling his own iron lung, as well as the exercise of making use of what is left of his breathing muscles. This work awaits the organization of a group of physiologists, physicians, and engineers to carry out the necessary researches.

Of all the things I have done in physiology, that which seems to me most significant is the application to the study of brain waves of the statistical theory of what are known as time series.

My war work on filtering and prediction of time series had represented an extension of my earlier work on generalized harmonic analysis and on the Brownian motion as tools for the study of irregular phenomena distributed in time. For years I had the intention of using these tools in every region in which they seemed apt. From the very beginning of the study of brain waves, ever since I had contact with some of the original electroencephalographers in Arturo's seminar at the Harvard Medical School, I had felt that this field was one in which I could accomplish something;

and I have never ceased to importune my neurophysiologist comrades with requests that they give a sympathetic hearing to these methods and that they try them out if possible on some experimental data.

In the early days of brain-wave work, it was supposed that the stray electric currents in the brain, as observed through the scalp, would throw a new light on the physiology of the brain and the associated mental phenomena. Much has in fact been done in the treatment of epilepsy through the study of brain waves, but the great expectations of the thirties have not yet been realized. The reason is that the brain waves as we see them originally are a mixture of very varied phenomena such as we would find if, for example, we observed the stray electric currents around a computing machine or a control machine. They speak a language of their own, but this language is not something that one can observe precisely with the naked eye, by merely looking at the ink records of the electroencephalograph. There is much information contained in these ink records, but it is like the information concerning the Egyptian language which we had in the days before the Rosetta Stone, which gave us the clue to the Egyptian script.

Of recent years, I have been a member of a group involving people from the various laboratories at M.I.T. and from the Massachusetts General Hospital which has been endeavoring to find the Rosetta Stone for the script of the brain waves by means of harmonic analysis. We have had a conspicuous example of success in this field in the past in the work of the great American experimental physicist, Michelson.

Michelson invented an instrument called the interferometer, which was the most delicate machine ever invented for the study of the spectrum of light, and which enabled him to carry out such a seemingly impossible task as the determination of the angles subtended at the earth by some of the fixed stars. The principle of this instrument can in fact be realized in an instrument for the study of brain waves and other such oscillation. We have called this instrument the autocorrelator. Many people at M.I.T., and Lee in particular, have reduced the design of autocorrelators to a surprising degree of perfection.

When the crude original records of brain waves are transformed by the autocorrelator, we obtain a picture of remarkable clarity and significance, quite unlike the illegible confusion of the crude records which have gone into the machine. We are at the very beginning of our work in this field, but we have great hopes of what it will offer for the future; and we should not be surprised if the ambitious expectations of the early electroencephalographers of thirty years ago, of a really legible form of electroencephalograph record, will now begin to be realized.

The analogy between the interferometer and the autocorrelator is deep and significant, and the earlier work of Michelson has given us a whole language for the reading of the results presented by such machines.

I have in fact done the greater part of my share in this brainwave work in the last three years, since the end of my visit to Mexico, but I consider it essentially as the consummation of the lines of research which Arturo and I had embarked on together.

The autocorrelation study of brain waves is not the only field where my mathematical interests and Rosenblueth's physiological interests have met. The original analogy we found between machine and human feedbacks has been supplemented again and again by striking new analogies which we keep on finding between the nervous system and control or computation machines.

From the very beginning, I was struck by the similarities between the nervous system and the digital computer. I did not mean to claim that these analogies are complete, or that we can exhaust the properties of the nervous system by calling it a digital computer. I merely wish to suggest that certain aspects of its behavior are close to those of the digital computer.

The nervous system is certainly a complicated net of elements which transfer impulses. Fundamentally, if an impulse is strong enough to go from the end of one nerve fiber to the other, it reaches the further end as a whole without much influence of the strength of the impulse at the nearer end, provided that it goes through at all. Thus, the nerve fiber transmits yeses or noes. When an impulse reaches the end of a nerve fiber, it combines with various other impulses that have reached the same level to determine whether the next nerve fiber discharges. In other words, the nerve fiber is a logical machine in which a later decision is made on the basis of the outcome of a number of earlier decisions. This is essentially the mode of operation of an element in a computing machine. Besides this fundamental resemblance, we have auxiliary resemblances pertaining to such phenomena as memory, learning, and the like.

There is one other medical matter that has attracted my attention of recent years. Walter Cannon, going back to Claude Bernard, emphasized that the health and even the very existence of the body depends on what are called homeostatic processes, namely, processes which tend to keep temperature, blood pressure, and the many other factors of the interior environment of a living being so stable that life is possible. That is, the apparent equilibrium of life is an active equilibrium, in which each deviation from the norm brings on a reaction in the opposite direction, which is of the nature of what we call negative feedback.

Thus, when the body goes wrong, there must be cases where the failure is an intrinsic breakdown of the feedback process, and where the mathematical description of the manner of failure indicates the nature of the feedback process and the nature of its breakdown. A colleague of mine, Paul Hahn, and I have applied this sort of discussion to the history of leukemia, and we see considerable evidence that in this excessive growth of the white blood corpuscles there is a homeostatic process balancing the creation and the destruction of the blood cells which is not completely abrogated, but which goes on at an incorrect level. I feel that this concept of a disease of homeostatis may well prove useful in many fields of medicine.

There has been in the past a great tendency in medicine to think in terms of localization. This has been particularly the case in matters concerning the brain, where a separate function has been discovered or postulated for almost every area of the cortex or surface of the hemispheres. However, the tendency of a strong emphasis on localization has been to subordinate general questions of organization to localizable atomic phenomena.

It seems to me that our studies of control apparatus are giving us a better insight into the way in which these local phenomena are built up into large processes extending all over the brain—or, in fact, the whole human body. In healthy activity these overall processes must be understood, since under pathological conditions they may break down in a manner which cannot be assigned to the failure of the individual parts. There are diseases, like leukemia, where certain processes such as the formation of white corpuscles are apparently running wild. However, even in this diseased activity there are strong signs that what is at fault is not so much an absence of all internal control over the process of corpuscle formation and corpuscle destruction but a control working at a false level.

Most of the pieces of research which Arturo and I undertook during my various visits to Mexico and his to the United States, have already appeared in the technical journals. I shall have more to say of the details of my later visits. My wife and I hope to visit Mexico more times in the future, whether for my research and writing or just to enjoy the life of a country which has been charmingly hospitable to us.

33 Moral Problems of a Scientist. The Atomic Bomb 1942–

One day during the Second World War I was called down to Washington to see Vannevar Bush. He told me that Harold Urey, of Columbia, wanted to see me in connection with a diffusion problem that had to do with the separation of uranium isotopes. We were already aware that uranium isotopes might play an important part in the transmutation of elements and even in the possible construction of an atomic bomb, for the earlier stages of this work had come before the war and had not been made in the United States.

I went to New York and had a talk with Urey, but I could not find that I had any particular qualification for solving the special problem on which he requested help. I was also very busy with my own work on predictors. I felt that there I had found my niche for the duration of the war. It was a place where my own ideas were particularly useful, and where I did not feel that anyone else could do quite so good a job without my help.

I therefore showed no particular enthusiasm for Urey's problem, although I did not say in so many words that I would not work on it. Perhaps I was not cleared for the problem, or perhaps my lack of enthusiasm itself was considered as a sufficient reason for not using me, but that was the last I heard of the matter. This work was a part of the Manhattan Project and the development of the atomic bomb.

Later on, various young people associated with me were put on the Manhattan Project. They talked to me and to everyone else with a rather disconcerting freedom. At any rate, I gathered that it was their job to solve long chains of differential equations and thereby handle the problem of repeated diffusions. The problem of separating uranium isotopes was reduced to a long chain of diffusions of liquids containing uranium, each stage of which did a minute amount of separation of the two isotopes, ultimately leading in the sum to a fairly complete separation. Such repeated diffusions were necessary to separate two substances as similar in their physical and chemical properties as the uranium isotopes. I then had a suspicion (which I still have, though I know nothing of the detail of the work) that the greater part of this computation was an expensive waste of money. It was explained to me that the effects on which one was working were so vanishingly small that without the greatest possible precision in computation they might have been missed altogether.

This however did not look reasonable to me, because it is exactly under these circumstances of the cumulative use of processes which accomplish very little each time that the standard approximation to a system of differential equations by a single partial differential equation works best. In other words, I had and have the greatest doubts, to the effect that in this very slow and often repeated diffusion process those phenomena which may not be justifiably treated as continuous are of very slight real importance.

Be that as it may, while I did not have any detailed knowledge of what was being done on Manhattan Project, the time came when neither I nor any other active scientist in America could fail to be aware that such a project was under way. Even then we did not have any clear idea of how it was to be used. We were afraid that the main use to be made of radioactive isotopes was as poisons. We feared that here we might well find ourselves in the position of having developed a weapon which international morality and policy would not permit us to use, even as they had held the Germans back from the use of poison gas against cities. Even were the work to result in an explosive, we were not at all clear as to the possibilities of the bomb nor as to the moral problems which its use would involve. I was very certain of at least one thing: that I was most happy to have had no share in the responsibility for its development and its later use.

So far the moral problem of warfare had not concerned me directly. However, in the fall of 1944 a complex of events took place which had a very considerable effect on my later career and thought. I had already begun to reflect on the relation between the high-speed computing machine and the automatic factory, and I had come to the conclusion that as the essence of the computing machine lay in its speed and in its programming, or determination of the sequence of operations to be performed by means of a magnetic tape or punched cards, the automatic factory was not far off. I wondered whether I had not got into a moral situation in which my first duty might be to speak to others concerning material which could be socially harmful.

The automatic factory could not fail to raise new social problems concerning employment, and I was not sure that I had the answers. A vast redistribution of labor at different levels would be created. When the human being is being used mechanically, simply as an inferior sort of switching or decision device, the automatic factory threatens to replace him completely by mechanical agencies. On the other hand, it creates a new demand for the highly skillful professional man who can organize the order of operations which will best serve a particular function.

It will also create a demand for trouble-shooters and maintenance crews of a particularly well-trained sort. If these changes in the demand for labor come upon us in a haphazard and ill-organized way, we may well be in for the greatest period of unemployment we have yet seen. It seemed to me then quite possible that we could avoid a catastrophe of this sort, but if so, it would only be by much thinking, and not by waiting supinely until the catastrophe is upon us. I shall say something later in this chapter of my present opinion in the matter.

Accordingly, when a colleague wished for some information concerning my newer work I answered that I was not by any means certain that this work should be communicated to him, or indeed to the public at large. I felt all the more strongly about this inasmuch as he had requested the information for military purposes, and I did not know whether I should be a party to the use of my new ideas for controlled missiles and the like.

I showed the letter to a colleague of mine who happened to have a flair for journalism. He immediately suggested that I send my note to the *Atlantic Monthly* as a basis for what might be a more elaborate article. I followed his suggestion and sent it. If I had thought out fully how I was thus subjecting myself to a deep moral commitment while he was subjecting himself to nothing at all, I might well have hesitated, although I probably would put this hesitation behind me as an act of cowardice. The moral consequences of my act were soon to follow.

About this time I had agreed to participate in two meetings: the earlier one on applied mathematics, called together by Princeton at the end of its second hundred years of existence; and a later one organized by Aiken at Harvard on the subject of automatic high-speed computing machines. To this second meeting, which took place under the joint auspices of Harvard University and the Navy Department, I had agreed to give a paper. Meanwhile I reported at the Princeton meeting on my work on prediction theory. While my talk covered material which I knew could ultimately be used for military purposes, I had counted on the abstractness of my presentation and the natural inertia of many of my colleagues to prevent the work from being put to immediate and uncontrollable military use.

However, my hand was forced. A colleague who had previously taught at M.I.T. and had gone back to his home University of California, because of his earlier associations there and because of the climate, had been pushing my name as head of or consultant for a military or semimilitary project on mechanical computation, to be located in California. He had not consulted me in the matter, but he had assumed that an invitation to California would be accepted by me without question.

I have said that the project was semimilitary. It was in fact to be under the Bureau of Standards, but it was quite clear that all the facilities engineered by the project if it should be successful would be pre-empted for years by the military services. My acquaintance had tried to commit me not only to work whose objective was distasteful to me but to work which would involve conditions of secrecy, of a police examination of my opinions, and of the confinements of administrative responsibility. These I could not accept. They would have bound me to a course of conduct which would have broken me down in a very few months. When the invitation was passed on to me, I considered my *Atlantic Monthly* letter, and I had no alternative but to say no. I probably would have said no anyway, as I did not fancy myself as an administrator.

Then I recalled the military meeting at Harvard at which I had already committed myself to speak. There were some two weeks before it would come off, so I thought that I had ample time to change my policy. I went to Aiken and tried to explain the situation. In particular, I pointed out to him that the California offer had made it necessary for me to take a definite stand on my war work and that I could not accept one sort of a military association and reject another. I therefore asked to be released from my promise to give a talk.

I gathered from Aiken that there would be time to take my name off the list of speakers for the meeting. However, when the meeting came, I found that Aiken had done this by merely running a line through my name on the printed programs which had been issued to members of the meeting and to the press.

This procedure was extremely embarrassing to me. It was even more embarrassing to him. The newspapermen came to me and asked whether this striking out of my name had anything to do with the letter that had appeared in the *Atlantic Monthly*. I said that it had, and I tried to explain to them the circumstances that had forced my hand. I took full responsibility in the matter and said that I acted with Aiken's consent and that I was not taking this step out of pique or personal animosity.

Naturally, they went to Aiken in the matter. Without reflection, he assumed that I had been involved in some deep plot to discredit him and to turn the meeting into a public scandal. In fact, as I have said, I had consulted him at the very beginning and had understood that there was to be no publicity about the affair. The matter would have had no publicity if it had not been for the emphasis which he had placed on my participation in the meeting by the way he scratched out my name.

All these emotional experiences were nothing to those through which I went at the time of the bombing of Hiroshima. At first I was of course startled, but not surprised, as I had been aware of the possibility of the use of the new Manhattan Project weapons against an enemy. Frankly, however, I had been clinging to the hope that at the last minute something in the atomic bomb would fail to work, for I had already reflected considerably on the significance of the bomb and on the meaning to society of being compelled to live from that time on under the shadow of the threat of limitless destruction.

Of course I was gratified when the Japanese war ended without the heavy casualties on our part that a frontal attack on the mainland would have involved. Yet even this gratifying news left me in a state of profound disquiet. I knew very well the tendency (which is not confined to America, though it is extremely strong here) to regard a war in the light of a glorified football game, at which at some period the final score is in, and which we have to count as either a definite victory or a definite defeat. I knew that this attitude of dividing history into separate blocks, each contained within itself, is by no means weakest in the Army and Navy.

But to me this episodic view of history seemed completely superficial. The most important thing about the atomic bomb was, in my opinion, not the termination of a specific war without undue casualties on our part, but the fact that we were now confronted with a new world and new possibilities with which we should have to live ever after. To me the most important fact about the wars of the past was that, serious as they had been, and completely destructive for those involved in them, they had been more or less local affairs. One country and one civilization might go under, but the malignant process of destruction had so far been localized, and new races and peoples might take up the torch which the others had put down.

I did not in the least underrate the will to destructiveness, which was as much a part of war with a flint ax and of war with a bow and arrow as it is of war with a musket and of war with a machine gun. What came most strongly to my attention was that in previous wars the power of destruction was not commensurate with the will for destruction. Thus, while I realized that as far as the people killed or wounded are concerned, there is very little difference between a cannonade or an aerial bombardment with explosive bombs of the type already familiar and the use of the atomic bomb, there seemed to me to be most important practical differences in the consequences to humanity at large.

Up to now no great war, and this includes World War II, had been possible except by the concerted and prolonged will of the people fighting, and consequently no such war could be undertaken without a profoundly real share in it by millions of people. Now the new modes of mass destruction, expensive as they must be in the bulk, have become so inexpensive per person killed that they no longer take up an overwhelming share of a national budget.

For the first time in history, it has become possible for a limited group of a few thousand people to threaten the absolute destruction of millions, and this without any highly specific immediate risk to themselves.

War had made the transition between an overwhelming assertion of national effort and the push-button declaration of the will of a small minority of people. Fundamentally this is true, even if one includes in the military effort all the absolutely vast but relatively small sums which have been put into the whole body of nuclear research. It is even more devastatingly true if one considers the relatively minimal effort required on the part of a few generals and a few aviators to place on a target an atomic bomb already made.

Thus, war has been transported, at least as a possibility, from the field of national effort to the field of private conspiracy. In view of the fact that the great struggle to come threatens to be one between the United States and the Soviet Government, and in view of the additional fact that the whole atmosphere and administration of the Soviet Government shares with that of the Nazis an extremely strong conspiratorial nature, we have taken a step which is intrinsically most dangerous for us.

I did not regard with much seriousness the assertions which some of the great administrators of science were making, to the effect that the know-how needed for the construction of the atomic bomb was a purely American thing and could not be duplicated by a possible enemy for many years at least, during which we could be counted upon to develop a new and even more devastating know-how. In the first place, I was acquainted with more than one of these popes and cardinals of applied science, and I knew very well how they underrated aliens of all sorts, in particular those not of the European race. With my wide acquaintance among scholars of many races and many countries, I had not been able to discern that scientific ability and moral discipline were the peculiar property of those of blanched skin and English speech.

But this was not all. The moment that we had declared both our possession of the bomb and its efficiency by using it against an enemy, we had served notice on every country that its continued existence and independence of policy were conditioned on its prompt possession of a similar weapon. This meant two things: that any country which was our rival or potential rival was bound to push nuclear research for the sake of its own continued independent existence, with the greatly stimulating knowledge that this research was not intrinsically in vain; and that any such country would inevitably set up an espionage system to get hold of our secrets.

This is not to say that we Americans would not be bound in self-defense to oppose such leaks and such espionage with our full effort for the sake of our very national existence; but it does mean that such considerations of legality and such demands on the moral responsibility of loyal American citizens could not be expected to have the least force beyond our frontiers. If the roles of Russia and the United States had been reversed, we should have been compelled to do exactly what they did in attempting to discover and develop such a vital secret of the other side; and we should regard as a national hero any person attached to our interests who performed an act of espionage exactly like that of Fuchs or the Rosenbergs.

I then began to evolve in my mind the general problem of secrets; not so much as a moral issue, but as a practical issue and a policy which we might hope to maintain effectively in the long run. Here I could not help considering how soldiers themselves regard secrets in the field. It is well recognized that every cipher can be broken if there is sufficient inducement to do it and if it is worthwhile to work long enough; and an army in the field has one-hour ciphers, twenty-four-hour ciphers, one-week ciphers, perhaps one-year ciphers, but never ciphers which are expected to last an eternity.

Under the ordinary circumstances of life, we have not been accustomed to think in terms of espionage, cheating, and the like. In particular, such ideas are foreign to the nature of the true scientist, who, as Einstein has pointed out, has as his antagonist a world which is hard to understand and interpret, but which does not maliciously and malignantly resist this interpretation. "The Lord is subtle, but he isn't plain mean."

With ordinary secrets of limited value, we do not have to live under a perpetual fear that somebody is trying to break them. If, however, we establish a secret of the supreme value and danger of the atomic bomb, it is not realistic to suppose that it will never be broken, nor that the general good will among scientists will exclude the existence of one or two who, either because of their opinions or their slight resistance to moral pressure, may give our secrets over to those who will endanger us.

If we are to play with the edged tools of modern warfare, we are running not merely the danger of being cut by accident and carelessness but the practical certainty that other people will follow where we have already gone and that we shall be exposed to the same perils to which we have exposed others. Secrecy is thus at once very necessary and, in the long run, quite impossible. It is unrealistic to give over our main protection to such a fragile defense.

There were other reasons, moreover, which, on much more specific grounds, made me feel skeptical of the wisdom of the course we had been pursuing. It is true that the atomic bomb had been perfected only after Germany had been eliminated from the war, and that Japan was the only possible proving ground for the bomb as an actual deadly weapon. Nevertheless, there were many both in Japan and elsewhere in the Orient who would think that we had been willing to use a weapon of this terribleness against Japan when we might not have been willing to use it against a white enemy. I myself could not help wondering whether there might not be a certain degree of truth in this charge. In a world in which European colonialism in the Orient was rapidly coming to an end, and in which every oriental country had much reason to be aware of the moral difference which certain elements in the West were in the habit of making between white people and colored people, this weapon was pure dynamite (an obsolete metaphor now that the atomic bomb is here) as far as our future diplomatic policy was concerned. What made the situation ten times worse was that this was the sort of dynamite which Russia, our greatest potential antagonist if not our greatest actual enemy, was in a position to use, and would have no hesitation whatever in using.

It is the plainest history that our atomic bomb effort was international in the last degree and was made possible by a group of people who could not have been got together had it not been for the fact that the threat of Nazi Germany was so strongly felt over the world, and particularly by that very scholarly group who contributed the most to nuclear theory. I refer to such men as Einstein, Szilard, Fermi, and Niels Bohr. To expect in the future that a similar group could be got together from all the corners of the world to defend our national policy involved the continued expectation that we should always have the same moral prestige. It was therefore doubly unfortunate that we should have used the bomb on an occasion on which it might have been thought that we would not have used it against white men.

There was another matter which aroused grave suspicion in the minds of many of us. While the nuclear program did not itself involve any overwhelming part of the national military effort, it was still in and for itself an extremely expensive business. The people in charge of it had in their hands the expenditure of billions of dollars, and sooner or later, after the war, a day of reckoning was bound to come, when Congress would ask for a strict accounting and for a justification of these enormous expenditures. Under these circumstances, the position of the high administrators of nuclear research would be much stronger if they could make a legitimate or plausible claim that this research had served a major purpose in terminating the war. On the other hand, if they had come back empty-handed—with the bomb still on the docket for future wars, or even with the purely symbolic use of the bomb to declare to the Japanese our willingness to use it in actual fact if the war were to go on—their position would have been much weaker, and they would have been in serious danger of being broken by a new administration coming into power on the rebound after the war and desirous of showing up the graft and ineptitude of its predecessor.

Thus, the pressure to use the bomb, with its full killing power, was not merely great from a patriotic point of view but was quite as great from the point of view of the personal fortunes of people involved in its development. This pressure might have been unavoidable, but the possibility of this pressure, and of our being forced by personal interests into a policy that might not be to our best interest, should have been considered more seriously from the very beginning.

Of the splendid technical work that was done in the construction of the bomb there can be no question. Frankly, I can see no evidence of a similar high quality of work in the policymaking which should have accompanied this. The period between the experimental explosion at Los Alamos and the use of the bomb in deadly earnest was so short as to preclude the possibility of clear thinking. The qualms of the scientists who knew the most about what the bomb could do, and who had the clearest basis to estimate the possibilities of future bombs, were utterly ignored, and the suggestion to invite Japanese authorities to an experimental exhibition of the bomb somewhere in the South Pacific was flatly rejected.

Behind all this I sensed the desires of the gadgeteer to see the wheels go round. Moreover, the whole idea of push-button warfare has an enormous temptation for those who are confident of their power of invention and have a deep distrust of human beings. I have seen such people and have a very good idea of what makes them tick. It is unfortunate in more than one way that the war and the subsequent uneasy peace have brought them to the front.

All these and yet other ideas passed through my mind on the very day of Hiroshima. One of the strong points and at the same time one of the burdens of the creative scholar is that he must stand alone. I wished—oh how I wished!—that I could be in a position to take what was happening passively, with a sincere acceptance of the wisdom of the policy makers and with an abdication of all personal judgment. The fact is, however, that I had no reason to believe that the judgment of these men on the larger issues of the situation was superior to my own, whatever their technical information might be. I knew that more than one of the high officials of science had not one tenth my contact with the scientists of other countries and of other standpoints and was in nowhere nearly as good a position to assess the world reaction to the bomb. I knew, moreover, that I had been in the habit of considering the history of science and of invention from a more or less philosophic point of view, and I did not believe that those who made the decisions could do this any better than I might. The sincere scientist must back his bets and guesses, even when he is a Cassandra and no one believes him. I had behind me many years of lonely work in science where I had finally proved to be in the right. This inability to trust the Powers That Be was a source of no particular satisfaction to me, but there it was, and it had to be faced.

One of my greatest worries was the reaction of the bomb on science and on the public's attitude to the scientist. We had voluntarily accepted a measure of secrecy and had given up much of our liberty of action for the sake of the war, even though—for that very purpose, as many of us thought—more secrecy than the optimum was imposed, and this at times had hampered our internal communications more than the information-gathering service of the enemy. We had hoped that this unfamiliar self-discipline would be a temporary thing, and we had expected that after this war—as, after all, before—we should return to the free spirit of communication, intranational and international, which is the very life of science. Now we found that, whether we wished it or not, we were to be the custodians of secrets on which the whole national life might depend. At no time in the foreseeable future could we again do our research as free men. Those who had gained rank and power over us during the war were most loath to relinquish any part of the prestige they had obtained. Since many of us possessed secrets which could be captured by the enemy and could be used to our national disadvantage, we were obviously doomed to live in an atmosphere of suspicion forever after, and the police scrutiny on our political opinions which began in the war showed no signs of future remission.

The public liked the atomic bomb as little as we did, and there were many who were quick to see the signs of future danger and to develop a profound consciousness of guilt. Such a consciousness looks for a scapegoat. Who could constitute a better scapegoat than the scientists themselves? They had unquestionably developed the potentialities which had led to the bomb. The man in the street, who knew little of scientists and found them a strange and self-contained race, was quick to accuse them of a desire for the power of destruction manifested by the bomb. What made this both more plausible and more dangerous was the fact that, while the working scientists felt very little personal power and had very little desire for it, there was a group of administrative gadget workers who were quite sensible of the fact that they now had a new ace in the hole in the struggle for power.

At any rate, it was perfectly clear to me at the very beginning that we scientists were from now on to be faced by an ambivalent attitude. For the public, who regarded us as medicine men and magicians, was likely to consider us an acceptable sacrifice to the gods as other, more primitive publics do. In that very day of the atomic bomb the whole pattern of the witch hunt of the last eight years became clear, and what we are living through is nothing but the transfer into action of what was then written in the heavens.

While I had no share in the atomic bomb itself, I was nevertheless led into a very deep searching of soul. I have already explained how my work on prediction and on computing machines had led me to the basis of cybernetics, as I was later to call it, and to an understanding of the possibilities of the automatic factory. From the strictly scientific point of view, this was not as revolutionary as the atomic bomb, but its social possibilities for good and for evil were enormous. I tried to see where my duties led me, and if by any chance I ought to exercise a right of personal secrecy parallel to the right of governmental secrecy assumed in high quarters, suppressing my ideas and the work I had done.

After toying with the notion for some time, I came to the conclusion that this was impossible, for the ideas which I possessed belonged to the times rather than to myself. If I had been able to suppress every word of what I had done, they were bound to reappear in the work of other people, very possibly in a form in which the philosophic significance and the social dangers would be stressed less. I could not get off the back of this bronco, so there was nothing for me to do but to ride it.

I thus decided that I would have to turn from a position of the greatest secrecy to a position of the greatest publicity, and bring to the attention of all the possibilities and dangers of the new developments. I first thought of the trade unions as the people who would naturally be most interested in the matter. My friends directed me toward two union leaders, one of them an intellectual counselor who had himself very little direct authority among the union people with whom he was associated, and the other a high official of the typographers' union. In both cases I found a confirmation of what my English friends had told me some years before: The union official comes too directly from the workbench, and is too immediately concerned with the difficult and highly technical problems of shop stewardship, to be able to entertain any very forwardlooking considerations of the future of his own craft.

I found plenty of good will among my union friends but an absolute block on their part to communicate my ideas to their union workers. This was in the middle forties; since then the situation has changed radically. I have been in repeated communication with Mr. Walter Reuther, of the United Automobile Workers, and I have found in him both an understanding of my problems and a willingness to give my ideas publicity through his union journals. In fact, I have found in Mr. Reuther and the men about him exactly that more universal union statesmanship which I had missed in my first sporadic attempts to make union contacts.

There is another quarter in which the sort of ideas I have had concerning the automatic factory have made gratifying headway. This is in the circles of management itself. In the winter of 1949 I gave a talk to the Society for the Advancement of Management concerning the automatic factory as a technical possibility and the social problems it would introduce, and in both matters I was backed up by high management authorities, as, for example, an executive of Remington Rand, Inc. In December of 1952 I was asked to give a talk on a similar subject as part of a symposium on the automatic factory held by the American Society of Mechanical Engineers.

The progress in the general attitude from the first talk to the second was remarkable. Not only was the attending public much larger and my technical remarks confirmed by automatic-machine men for several industries, but the social consciousness of the group as a whole was far beyond what I had found three years before.

While there were a good many who were more sanguine than I had been as to the possibility of achieving a large measure of industrial automatization without catastrophe, there was a general awareness of the interest of the public at large in a meeting which was going to affect so profoundly their future method of life. In particular, problems of the grade-up of repetitive factory workers into trouble-shooting men (and indeed into a sort of junior engineer) occupied a great deal of attention.

Another much-debated problem was that of the new leisure we might expect in the future and the use that could and must be made of it. Indeed, I heard hard-boiled engineering administrators express views which sounded remarkably like the writings of William Morris. Above all, I had everyone backing me in cautioning that the new displacement of human beings from the repetitive labor of the factory must not be taken as a devaluation of the human being and a glorification of the gadget.

The years that have passed since this talk have seen the automatic factory develop from a remote possibility into a beginning actuality, and we can start to assess on a factual basis its probable impact on society. The first industrial revolution of the early nineteenth century replaced the individual by the machine as a source of power. No factory worker of the present day is earning any large part of his wages by the horsepower of his output. Even if he is doing the hardest sort of physical labor, as for example, if he is a steel puddler, his pay is not primarily given him as a prime mover in a power process. What he is actually paid for is his experience and knowledge of how to exert his strength most effectively in a highly purposeful manufacturing process.

However, the strong men of industry such as the steel puddlers are in a decided minority. The factory worker finds a small electric motor or a pneumatic tool at his elbow, and these will give him the sheer physical strength of ten men. His business is to accomplish a certain purpose by going through certain motions in a given succession. If, for example, he is pasting labels on tin cans, he must see that he has the correct stack of labels before him, that he has moistened them correctly, that he has put them in the correct position on the can, and that he turns at the proper time from one can to the next. This sort of laborer goes through a purely repetitive process, making the minimum demands on any but the lowest level of judgment and observation.

Of course, there are other forms of factory labor. There are the foremen and there are the members of the troubleshooting gangs, who at the very lowest level must be skilled craftsmen and, on the higher levels, are assimilated in their function to junior engineers. Leaving out these higher ranks of labor, the routine factory worker is often doing so conventional a task that every motion of his and the cue for every motion, may be assigned in advance. This is the point of such efficiency systems as the motion study of Taylor and the Gilbreths.

I have already indicated that it is this level of work which will be replaced by the operations of the automatic factory. Essentially, to my way of seeing things, most of the human labor which the automatic factory displaces is an inhuman sort of human labor, which has been considered a natural task for human beings only since the historical accident of the industrial revolution. Nevertheless, any sudden and uncompensated displacement of this labor must have catastrophic consequences in the direction of unemployment.

Where will this labor have to go? The most obvious answer is that even the automatic factory will always require a considerable group of trouble shooters, skilled craftsmen, and specialists in programming or in the adaptation of the machines to specific problems. During a gradual process of automatization, the natural place for unskilled factory labor to go is into these higher cadres, by some sort of up-grading. The question of the possibility of this up-grading thus becomes vital.

There is a considerable amount of evidence that the sources of labor which furnished the unskilled factory labor of the past generation are drying up, because, since soon after the end of the First World War, we have had no extensive body of immigrants seeking to establish and settle themselves in the country and willing to accept any degree of economic undervaluing. It is the children of this last extensive generation of immigrants who fought in the Second World War, and the rising generation of the present day consists of their children's children. These younger generations are unwilling to accept the permanent position of economic inferiority belonging to the unskilled workers in the old type of factory. Many of them are going into the professions, and even those who are not are beginning to demand that their work be interesting and not a blind alley.

This is not the first time in our industrial history in which technical advances have been conditioned by the decreasing availability of labor of a certain type. Automatic telephone switching came in simply because the old system of hand switching bade fair to demand the entire population of girl high school graduates.

Another matter which may make the stepping up of labor easier than it might have seemed a few years ago is the training of a very considerable part of the young men in our military services as technicians of a relatively high grade. This has been particularly the case in the Air Force. The sort of young man who can be trained to direct and to care for a radar instrument is certainly the sort who can easily learn to be a member of a factory trouble-shooting gang.

Thus it is quite possible, although it is not certain, that the labor environment for the automatic factory has come just at the right time. At any rate, the atmosphere into which the automatic factory is coming is one where it fits into a definite niche of human activity, and one which has been alerted to both the advantages of automatization and the risks.

While this has been the work of many hands, I feel proud that some part of the healthy and understanding atmosphere into which automatization is coming, and of the collaboration of labor and management in being prepared to work out jointly a mode of industrial life which embraces the automatic factory, may be due to my early efforts to alert both of these elements.

34 Nancy, Cybernetics, Paris, and After

1946–1952

In the summer of 1946 there was to take place a private mathematical conference on harmonic analysis in France, at the University of Nancy. I was invited to participate. As a matter of fact, much of the meeting was to deal with my ideas. I traveled to England on a Dutch boat, and before taking part in the meeting I made my usual visit to England and my English friends. I visited University College, London, where J. B. S. Haldane was teaching. He had been divorced from his first wife, and he was now married to a brilliant young geneticist who had been his assistant during the war in physiological experiments they had made concerning the effect on various gases under high pressure.

Both of them had repeatedly put on diving suits and descended into steel tanks of water, and had been subjected to concentrated atmospheres of different gases until the gases had become so poisonous that they had gone into convulsions. I believe Haldane had gone into convulsions four times and his wife seven times. This was in keeping with the tradition which Haldane had followed of subjecting himself as his own guinea pig to every extreme physiological condition which he could find; and of his intrepidity earlier in the war, which made him specialize in the opening and disarming of enemy mines cast ashore.

In general, Haldane is the type of person who deliberately puts himself into positions of danger, discomfort, or unpopularity when there is a job to be done which he considers important. There is something in him, on a more rational level, similar to the reckless drive I had also found in my colleague Paley.

While staying with the Haldanes, I had a good time visiting my colleagues at the National Physical Laboratories at Teddington, the various colleges of the University of London, the University of Manchester, and at Cambridge. I found that Manchester was well at the front of the new technique of high-speed automatic computing machines. At the National

Physical Laboratories, Turing was making the same sort of synthesis between mathematical logic and electronics which Shannon had made in the United States. In short, I found the British atmosphere entirely ripe for the assimilation of the new ideas which I was then developing concerning control, communication, and organization.

Actually, the idea of a comprehensive book on these subjects began to come to a head when I reached Paris. There an M.I.T. colleague introduced me to one of the most interesting men I have ever met, the publisher Freymann, of the firm of Hermann et Cie.

Freymann, who, alas, has just died, was a Mexican, and he first came to Paris as a cultural attaché in the Mexican diplomatic service. One of his grandfathers was a retired German sea captain, who had made his home in the region of Tepic, on the west coast of Mexico. The other grandfather was a Huichol Indian chieftain from the same region. My friend's two grandmothers were both of Spanish origin. Freymann, who kept a drab little bookshop opposite the Sorbonne, where every now and then one of the scientific or other intellectual notables would drop in, delighted in relating to me the way each of his two grandfathers tried to capture him from the influence of the other, the one telling him always to be European, and the other reminding him that he was an Indian.

We talked at length about Mexico, and finally the talk came around to my scientific work. Freymann broached the matter which had chiefly interested him. Would I write up my ideas concerning communication, the automatic factory, and the nervous system as a booklet for his series?

He explained to me that he was the son-in-law of the former publisher Hermann, and that he had been the only one of the family to wish to continue the firm when his father-in-law had died. He told me of the various devices by which he had secured the publication contracts for a number of societies, and how he had used this to build up a really intellectual publishing house, as nearly free from the motive of profit as any publishing house can be.

I had already heard of the singular group of French mathematicians who pooled their efforts with the pseudonym *"Bourbaki,"* the result of a student hoax, when the group had started to write under the name of a French general of former days. Freymann told me that he was in fact the founder of the group and that he wished to extend it by furthering a new fictitious university, called the University of Nancago, after the two existing schools at Chicago and Nancy.

I felt that it would be fun to get in with so interesting a group. I agreed to do a book for Freymann, and we sealed the contract over a cup of cocoa in a neighboring *patisserie*.

During all this time I was in contact with Mandelbrojt, who in fact was the organizer of the Nancy congress. We did some mathematical work together, and he accompanied me to Nancy in the little high-speed rail car that now takes the place of an express between the capital and that city. I was put up with the other foreign bigwigs at the beautiful hotel which forms a part of the harmonious square of buildings at the Place Stanislaus.

The glory of the square dates back to the eighteenth century, when the ex-King of Poland became the Duke of Lorraine. Nancy almost rivaled Paris or Versailles as a capital. They say that the etiquette of the court of Nancy was even stricter than that of Versailles. In fact, this ultimately led to the death of the duke. One day, it is said, while walking on the roof and a bit tipsy, he fell down one of the chimneys, and as no person of sufficient rank was available to touch the royal person, he had to stay there until he suffocated.

The hotel at which I stayed was the headquarters for the foreign visitors. There was Harald Bohr, from Denmark; Carleman, from Sweden; Ostrowski, from Basel, and dear old Papa Plancherel from the Zurich Federal Institute of Technology. Jessen was there from Denmark and Beurling from Sweden, both of whom belonged to a younger generation.

Of these, Harald Bohr and Carleman are now dead. Carleman's death was peculiarly tragic, as it so typically followed a Scandinavian pattern which is familiar to those who know the plays of Ibsen and Strindberg. He died of drink—not the social drinking which leads so often to ruin here—but the fiery, passionate dipsomania which is a common disease even in the very best circles of the Scandinavian countries. During the meeting he was often a bit drunk, and afterwards in Paris I saw him come to Mandelbrojt's apartment for an advance on the travel money due him, red-eyed, with a three-day beard.

Of all the Nancy people, the one whom I saw most often was Laurent Schwartz. His wife was Paul Lévy's daughter, whom I had met years before when I visited her father at Pougues-les-Eaux. Schwartz was active along lines very similar to my own. He had generalized still further the field which I had already treated in my *Acta* paper on generalized harmonic analysis. He reduced it to that highly abstract basis which is characteristic of all the work of the Bourbaki school to which he belonged.

As individuals and as a group, we guests were taken into the small-city social life of Nancy. It was a difficult and austere time in France, when wine was replaced by grape juice, and the delicious French bread loaded with perhaps fifty per cent of maize flour; but our hosts outdid one another in their hospitality. It was quite clear that if one hostess had three sorts of cakes at her party, the next would be constrained to have four, and the next five. At these parties we met M. le Recteur, M. le Maire, M. le Préfet. We were left with the conviction that M. le Recteur, M. le Maire, M. le Préfet, together with their respective ladies, had been meeting one another week in and week out for years. With all the courtesy, culture, and education that this life manifests, its stiffness would make a small New England town look like a sanctuary of social freedom.

At the time of which I write, the University of Nancy had been less damaged by the centralizing pull of Paris than many others. Schwartz has since gone to the capital, after the conventional pattern of French academic careers. At the time of my visit, however, Nancy was an excellent center for young visiting mathematicians who wished to see French university life at its best and who wished to get the full attention of young men still at their greatest vigor and on the way up. Now it again shows signs of relapsing into some of the French provincial apathy.

The meeting was a highly successful one, and we found it possible to integrate our work very well. I returned to Paris. After a few days' work with Bouligand, in which we started a joint paper, and further consultations with Freymann, I crossed over to England and did a little brief visiting before I took the boat from Southampton. I think it was on this second trip that I visited Oxford again and that I went even further west to Bristol, where I saw Grey Walter, who showed me the exceedingly interesting work he was doing on electroencephalography.

Richard Caton had made electroencephalographic studies on animals in England in 1875, but a German, Hans Berger, was the first to make observations concerning certain electrical potentials which displayed themselves on the human scalp. These potentials have their origin in the electrochemical activity of the brain and show a certain relation to various neural and mental disorders which is not too clearly decipherable. Originally, there were great expectations of these phenomena as a direct mode of access to the physiology of the brain, and they have in fact shown certain readable characteristics in cases of epilepsy and of threatening epilepsy.

Apart from this, there are certain regularities of brain waves which can be observed under the proper conditions. The most conspicuous and consistent of these is the so-called alpha rhythm, which is an oscillation of a period of about a tenth of a second.

The art of reading such irregular oscillations is not easy, and there is much that they contain which is not accessible to the naked eye. As I have said in an earlier chapter concerning my experiences on work in physiology jointly with Arturo Rosenblueth, I have recently developed mathematical tools which enable the observer to make more nearly definite statements concerning brain waves. This work is now under way as a joint undertaking in which scientists from M.I.T. and the Massachusetts General Hospital collaborate.

Dr. Grey Walter, although an American by origin, has long lived in Europe, and may be considered one of the leaders of the European study of brain waves or electroencephalography. He is a man who is full of enthusiasm and energy, and he has devised apparatus to give a comprehensive picture of brain waves in different parts of the brain. This picture is interesting and will without any doubt be useful in the study of the normal physiology of the brain and the diagnosis of its disorders. It is, however, more synoptic and less precise in mathematical detail than the tool which we are using in our researches. In fact, Walter's view of science is closer to that of a graphic artist than to that of a mathematician.

At about the same time as I myself, Walter had begun to see the analogies between the feedback machine and the human nervous system and to construct mechanisms which would exhibit some of the features of animal behavior. I have been working on the "moth" which would automatically steer itself into a light. Walter's automata were called "turtles," and had a more complicated repertory of behavior. This included a mechanism by which they might avoid one another in their motions and another mechanism by which when they were "hungry"—literally, when their storage batteries had been exhausted—they might be directed into something analogous to a rabbit hutch, where they could feed themselves on electricity until their storage battery was recharged.

I returned home from Southampton on a Dutch ship, the same one I had taken on the way over. On both voyages a large part of the passenger list were Dutch peasants who had immigrated to the United States, many of whom had lived in the Michigan region around Grand Rapids. They were mostly farmers by origin, and of that intensely religious Calvinistic upbringing which one sees so much in Holland. They had been home after the war for a first contact with their kinsmen who had suffered greatly in the struggle, and they played an important part in the restoration of the country. I am afraid that I forfeited much of their esteem by swearing once or twice in Dutch, and the shiver that passed over them was of a quality that I would not have seen even in the New England countryside.

Nevertheless, when these fine, simple, dignified, respectable people got into the smoking room and had a glass or two of Holland gin under their belts, they would sing the old Dutch songs and even start to dance in the old Dutch manner which is shown in the paintings of Jan Steen, Adriaen Brouwer, and the elder Brueghel. The clothes had changed, but the faces of these staid farm women and vigorous, tight-faced farmers had not changed at all, and even some of the songs, and I suppose some of the dances, dated back to the seventeenth century.

When I got back to the States I found that I had to take up again my work in Mexico. My daughter Barbara was a bit undecided what to do that summer, and I took her down with me. I started more neurophysiological work with Arturo Rosenblueth, and I continued to live among the same group and in very much the same manner as on my previous visits. Barbara and I, and later Margaret, who joined us, found quarters in an apartment house in the real estate development built up on the grounds of the old race track, and we shared in the occupation of a roof garden from which we could see the snows of Popocatepetl and Iztaccihuatl. There was a young American couple occupying another apartment in the same building. The husband was also working at the Instituto, and we used to discuss together the book on prediction theory and control apparatus which I had promised Freymann.

I went to work very hard on this, but the first thing that puzzled me was what title to choose for the book and what name for the subject. I first looked for a Greek word signifying "messenger," but the only one I knew was *angelos*. This has in English the specific meaning "angel," a messenger of God. The word was thus pre-empted and would not give me the right context. Then I looked for an appropriate word from the field of control. The only word I could think of was the Greek word for steersman, *kubernētēs*. I decided that, as the word I was looking for was to be used in English, I ought to take advantage of the English pronunciation of the Greek, and hit on the name *cybernetics*. Later on, I found that a corresponding word had been used since the early nineteenth century in France by the physicist Ampère, in a sociological sense, but at that time I did not know it.

What recommended the term cybernetics to me was that it was the best word I could find to express the art and science of control over the whole range of fields in which this notion is applicable. Many years before, Vannevar Bush had suggested to me that new scientific tools should be found to deal with the new theories covering control and organization. Ultimately I began to look for these tools in the field of communication. My early work on probability theory, as exemplified in my studies of the Brownian motion, had convinced me that a significant idea of organization cannot be obtained in a world in which everything is necessary and nothing is contingent. Such a rigid world is organized only in the sense in which a rigidly welded bridge is organized. Everything depends on everything else and nothing on one part of the bridge structure rather than on another. The result is that in such a bridge there is no way to localize the strains, and unless a welded bridge is made of materials which can give and readjust their internal strains, the strains are almost certain to be so concentrated that at some place or other the bridge will break or tear and will collapse.

Thus, a bridge or a building is able to endure only because it is not completely rigid. Similarly, an organization can exist only if the parts of it can give to a greater or less degree in response to the systems of internal stresses. Organization we must consider as something in which there is an interdependence between the several organized parts but in which this interdependence has degrees. Certain internal interdependencies must be more important than others, which is the same thing as saying that the internal interdependence is not complete, and that the determination of certain quantities of the system leaves others with the chance to vary. This variation from case to case is a statistical one, and nothing less than a statistical theory has enough freedom in it for the notion of organization to be significant.

I was driven back on the work of Willard Gibbs, and on the conception of the world not as an isolated phenomenon but as one of many possible phenomena with an allover probability distribution. I was forced to consider causality as something of which there can be either more or less rather than as something which is either there or absent.

The whole background of my ideas on cybernetics lies in the record of my earlier work. Because I was interested in the theory of communication, I was forced to consider the theory of information and, above all, that partial information which our knowledge of one part of a system gives us of the rest of it. Because I had studied harmonic analysis and had been aware that the problem of continuous spectra drives us back on the consideration of functions and curves too irregular to belong to the classical repertory of analysis, I formed a new respect for the irregular and a new concept of the essential irregularity of the universe. Because I had worked in the closest possible way with physicists and engineers, I knew that our data can never be precise. Because I had some contact with the complicated mechanism of the nervous system, I knew that the world about us is accessible only through a nervous system, and that our information concerning it is confined to what limited information the nervous system can transmit.

It is no coincidence that my first childish essay into philosophy, written when I was in high school and not yet eleven years old, was called "The Theory of Ignorance." Even at that time I was struck with the impossibility of originating a perfectly tight theory with the aid of so loose a mechanism as the human mind. And when I studied with Bertrand Russell, I could not bring myself to believe in the existence of a closed set of postulates for all logic, leaving no room for any arbitrariness in the system defined by them. Here, without the justification of their superb technique, I foresaw something of the critique of Russell which was later to be carried out by Gödel and his followers, who have given real grounds for the denial of the existence of any single closed logic following in a closed and rigid way from a body of stated rules.

To me, logic and learning and all mental activity have always been incomprehensible as a complete and closed picture and have been understandable only as a process by which man puts himself *en rapport* with his environment. It is the battle for learning which is significant, and not the victory. Every victory that is absolute is followed at once by the Twilight of the gods, in which the very concept of victory is dissolved in the moment of its attainment.

We are swimming upstream against a great torrent of disorganization, which tends to reduce everything to the heatdeath of equilibrium and sameness described in the second law of thermodynamics. What Maxwell, Boltzmann, and Gibbs meant by this heat death in physics has a counterpart in the ethics of Kierkegaard, who pointed out that we live in a chaotic moral universe. In this, our main obligation is to establish arbitrary enclaves of order and system. These enclaves will not remain there indefinitely by any momentum of their own after we have once established them. Like the Red Queen, we cannot stay where we are without running as fast as we can.

We are not fighting for a definitive victory in the indefinite future. It is the greatest possible victory to be, to continue to be, and to have been. No defeat can deprive us of the success of having existed for some moment of time in a universe that seems indifferent to us.

This is no defeatism, it is rather a sense of tragedy in a world in which necessity is represented by an inevitable disappearance of differentiation. The declaration of our own nature and the attempt to build up an enclave of organization in the face of nature's overwhelming tendency to disorder is an insolence against the gods and the iron necessity that they impose. Here lies tragedy, but here lies glory too.

These were the ideas I wished to synthesize in my book on cybernetics. My first goals were rather concrete and limited. I wanted to give an account of the new information theory which was being developed by Shannon and myself, and of the new prediction theory which had its roots in the prewar work of Kolmogoroff and in my researches concerning anti-aircraft predictors. I wished to bring to the attention of a larger public than had been able to read my “yellow peril” the relations between these ideas and show it a new approach to communication engineering which would be primarily statistical. I also wished to alert this larger public to the long series of analogies between the human nervous system and the computation and control machine which had inspired the joint work of Rosenblueth and me. However, I could not undertake this multiform task without an intellectual inventory of my resources. It became clear to me almost at the very beginning that these new concepts of communication and control involved a new interpretation of man, of man’s knowledge of the universe, and of society.

Communication is by no means confined to mankind, for it is found in different degrees in the mammals, the birds, the ants, and the bees, to say the least; but, notwithstanding all the communication involved in the cries and nuptial dances of the birds, the dumb play by which a bee indicates to its hive mates the direction and distance of sources of honey, and all the rest of these modes of communication which we are just beginning to understand, man’s language is more developed and more flexible than that of the animals and presents problems of a quite different sort.

Beside the obvious multiplicity of tongues and the wide scope of any individual language as a mode of expression, the extensive areas of the brain which seem to be devoted to the different aspects of speech and hearing, and reading and writing, testify to the overwhelming human importance of highly developed methods of communication.

To communicate with the outer world means to receive messages from it and to send messages to it. On the one hand, it means to observe, to experiment, and to learn; and on the other to exert our influence on the outer world so that our actions become purposeful and effective. Experimentation in fact is one form of a two-way conversation with the outer world, in which we use outgoing commands to determine the conditions of incoming observations, and in which, at the same time, we use our incoming observations to increase the effectiveness of our outgoing commands.

Communication is the cement of society. Society does not consist merely in a multiplicity of individuals, meeting only in personal strife and for the sake of procreation, but in an intimate interplay of these individuals in a larger organism. Society has a memory of its own, far more durable and far more varied than the memory of any individual belonging to it. In those societies which are fortunate enough to possess a good script, a large part of this communal tradition is in writing, but there are societies which, without writing, have preserved a whole tradition in the form of a technique of ritual memorization of tribal chants and histories.

Sociology and anthropology are primarily sciences of communication, and therefore fall under the general head of cybernetics. That particular branch of sociology which is known as economics and is distinguished by possessing rather better numerical measures of its values than the rest of sociology is a branch of cybernetics, by virtue of the cybernetic character of sociology itself. All these fields share in the general ideology of cybernetics, even if many of them are as yet insufficiently precise in their numerical techniques to make it worth-while to take advantage of the full mathematical apparatus of the larger subject.

Besides its function in these already existing sciences, cybernetics is bound to affect the philosophy of science itself, particularly in the fields of scientific method and epistemology, or the theory of knowledge. In the first place, the statistical point of view so manifest in cybernetics and in my earlier researches forces us to a new attitude toward order or regularity. Perfect information has nothing in it that is measurable, and measurable information cannot by that token be perfect. If we can measure degrees of causality (and much of my work on information theory has indicated that that is a perfectly possible goal), then that can only be because the universe is not a perfectly tight structure but one in which small variations are possible in different regions. We can then observe how much a change in one aspect of the universe will bring out changes in others.

Thus, from the point of view of cybernetics, the world is an organism, neither so tightly jointed that it cannot be changed in some aspects without losing all of its identity in all aspects nor so loosely jointed that any one thing can happen as readily as any other thing. It is a world which lacks both the rigidity of the Newtonian model of physics and the detail-less flexibility of a state of maximum entropy or heat death, in which nothing really new can ever happen. It is a world of Process, not one of a final dead equilibrium to which Process leads nor one determined in advance of all happenings, by a pre-established harmony such as that of Leibniz.

In such a world, knowledge is in its essence the process of knowing. There is no use in seeking for a final knowledge in an asymptotic state of the universe at the end of time, for this asymptotic state (if it exists) is in all likelihood timeless, knowledgeless, and meaningless. Knowledge is an aspect of life which must be interpreted while we are living, if it is to be interpreted at all. Life is the continual interplay between the individual and his environment rather than a way of existing under the form of eternity.

All this represents the manner in which I believe I have been able to add something positive to the pessimism of Kierkegaard and of those writers who have taken Kierkegaard as their inspiration. Among these, the most important are the existentialists. I have not replaced the gloom of existence by a philosophy which is optimistic in any Polyanna sense, but I have at least convinced myself of the compatibility of my premises, which are not far from those of existentialism, with a positive attitude toward the universe and toward our life in it.

These are the main ideas which I mulled over in my mind as I was writing my book on cybernetics. I discussed them with Arturo and with the American physiologist who was my neighbor in the same apartment house. We all felt hopeful that these ideas might amount to something, although none of us, not even I, had any concept of the excitement which they were actually to cause when they appeared in print.

I had some qualms about taking so much time away from the work Arturo and I had embarked on. This had languished slightly for a cause for which neither Arturo nor I was completely responsible. Arturo is an afternoon and evening worker who does not get steamed up until 3 or 4 P.M., and he can keep going until well after midnight. I am a morning worker, at my best on waking up, and I begin to lag by two in the afternoon, being totally unable to do creative work after dark. The result is that there were many gaps in our collaboration, which I could only fill by undertakings initiated by myself and which I did fill by the writing of *Cybernetics*.

In my work on this book, I was spurred on by certain fortuitous circumstances which were very threatening at the time and forced me to devote my energy to this new undertaking, which has become the basis of all my later career. Expenses were piling up on me from various sources, and I had no accumulated wealth from which to pay them off. I decided to do what many another writer has done; if it was possible, I would write myself out of this financial hole. To run ahead a little, this is exactly what I succeeded in doing; and though my writing has never tended to make me a rich man, *Cybernetics* represented the beginning of whatever security I enjoy at the present.

By now the second term of the M.I.T. academic year was approaching, and I was getting ready to return to the States. Very shortly before my return, I completed the book and sent it to Freymann, in Paris. There was a burden off my soul, and I spent some of the few days which remained to me in Mexico in visiting Taxco and in having a good time with my Mexican friends.

For some years I had been developing cataracts, and by this time they had advanced in both eyes to the point where they seriously interfered with my reading. There was nothing to do but have my lenses out. Naturally, an eye operation is an emotionally alarming thing. However, I was lucky enough to have an oculist in whom I had great confidence and who knew how to handle me emotionally as well. The result was that I found the operation less of an ordeal than I had expected and I was quite ready for the operation on the other eye when the time came and for the series of minor operations on both eyes which were necessary to give me the largest measure of eyesight possible.

To a considerable extent, my nearsightedness and my cataract operations canceled each other. The final result is that I now have better distance sight without glasses than I have ever had and that my reading vision is of a tolerable quality. Nevertheless, the operations have left me with eyes which are rather sensitive both to excessive light and to long use. They have forced me to change my habits of work and in some respects to change them in a way very advantageous to myself.

I now do most of my mathematical work on a blackboard rather than on paper, and this relieves me from the unpleasant necessity of going from near vision to far vision and back again, which I can do only by the use of bifocals or trifocals. Even more, it has made me supplant my habits of writing either in longhand or on a typewriter by the effective use of very efficient secretaries.

The mere act of writing had always been an unwelcome task for a man of my physical clumsiness, and my antipathy toward it had tightened my style and introduced a general crabbedness into my literary work. Now I was free of this, and, since my operations, I have become far more of a literary man than I ever thought possible before.

I have always been conscious of literature as something at least as much for the ear as for the eye. One thing that contributed to this idea was the period of six months when, at the age of eight, I had been forbidden to use my eyes and all my instruction was by ear. Dictating forces on the author a consciousness of sound, and I like it very much. I have a memory considerably better than average, and the absence of notes is no handicap to me. When an idea occurs to me which must be pieced into a larger manuscript, I dictate it to my secretary, and we work together on smoothing the joints.

I do all of my dictating directly to a secretary, and I am repelled by the impersonality of the dictating machine. No secretary who is not an educated person and a person of taste could do the work which I require of her, and such a person can and should always give a continual criticism of what I am dictating by reaction and expression, if in no other manner. There is thus established what I should call, in my cybernetic vocabulary, a feedback process, which I use to full advantage.

Moreover, in my periods of dictating, there are long moments in which I am thinking of the next thing to say, and during which I can scarcely remember to stop the accursed cylinder of a dictating machine and then to start it up again.

I showed the manuscript of my cybernetics book to the M.I.T. authorities, in particular to the officials of the Technology Press. They were much interested. Indeed, they hoped that they might find a way to publish the book in America.

From one point of view this was not difficult, as the book was written in English, even though it was to appear in a French series. From another point of view, since I had already granted Freymann the rights in the book (for it was accepted as soon as it was received) there were both legal and moral considerations to be settled before we could use his sheets for the offset printing of an American edition.

All this was settled at last, and the Technology Press and John Wiley and Sons took the book over. By the way, the same collaboration of publishers was reprinting my “yellow peril” at the same time.

Freymann had not rated the commercial prospects of *Cybernetics* very highly—nor, as a matter of fact, had anybody on either side of the ocean. When it became a scientific bestseller we were all astonished, not least myself.

This book at once transformed me from a working scientist with a good but limited reputation in my field into something of a public figure. This has been gratifying, but it has had its disadvantages, for I have since been committed to work with the most varied scientific groups and I have had to form part of a movement which has rapidly grown beyond the possibilities of my personal control.

Cybernetics was a new exposition of matters about which I had never written authoritatively before and, at the same time, a miscellany of my ideas. It came out in a rather unsatisfactory form, as the proofreading was done at a period at which I could not use my eyes and the young assistants who were to have helped did not take their responsibility seriously.

After the publication of the book, which got good reviews and, as I have said, a most unexpected sale, I came to be in much demand for the writing of papers of a more or less popular nature and for public lectures. For a time I followed the course of acceding to the blandishments of invitations to write and to speak, which gave me a new and perhaps spurious sense of importance.

Since then I have learned that if I want to contribute anything more to science, and if I wish to keep in reasonably good health, I must conserve my energies. In general, invitations for lecturing do not return to me in rewards or in prestige what they take out of me in fatigue. I have also found out by bitter experience how much a lecturer must protect himself against exploitation.

For very similar reasons, I do not touch requests for engineering consultation. In a field like my own, the consultant is usually far more interested in getting my name than in getting any ideas which I may have. Moreover, the ordeal of being quizzed by relays of company engineers, of contacting and meeting a number of strangers who are primarily interested in draining me dry, and of remaining affable under all these exactions, is something which the torturers of the Inquisition omitted to add to their repertory.

During all this time my daughters were in their late teens and early twenties, and going through college. Barbara remained undecided for some years between a scientific career and journalism. She had first spent a year at Radcliffe, and then studied at M.I.T. for a time. She did her journalistic work at Boston University, but it was not until after her marriage to Gordon Raisbeck that she completed these studies at Drew University, near her home in Morristown, New Jersey. In the time between, she had done a considerable amount of work in scientific journalism for Science Service in Washington.

Peggy matriculated at Tufts College, which was the school where I had done my undergraduate work. She specialized in biochemistry, and after her graduation and a certain amount of graduate study at M.I.T., in London, and at Boston University. She worked for a while at the Worcester Foundation for Experimental Biology. Peggy married shortly thereafter and is now active with a pharmaceutical firm in northern New Jersey.

Both my sons-in-law are engineers in the Bell Telephone Laboratories, doing work of a definitely mathematical nature as well as other more applied work. Thus, I have in my own family exemplified that peculiar genetics of mathematics of which I have already spoken, that mathematical ability goes down from father-in-law to son-in-law.

As 1950 came on, I received an invitation to go to France as a Fulbright fellow and to lecture at the Collège de France. On the French side, this offer emanated from Mandelbrojt. I finally decided that I could not spend a whole year on this undertaking, and I did not sail for France until December.

My French friends found me a hotel in Savoy where I could rest up before my rather arduous duties began. These had become arduous because I was also to participate in a congress on high-speed computing machines and automatization which was to take place in Paris early in January, 1951.

After this congress was over, I went to England for a few weeks and stayed with the Haldanes. Soon Margaret came over together with Peggy.

Margaret and I left immediately for Paris. We were put up for a few weeks in a building belonging to the Paris Observatory and at once found ourselves taken up into the intellectual and social life of the community.

I enjoyed my teaching at the Collège de France and was treated exactly as one of their own men. Every day of my lectures, of which there were twenty, I would go to the little office, reflect over my talk for a few minutes, sign the daybook, and be conducted into the lecture room by the wooden-legged *appariteur,* or university servant. I lectured in French, calling on my audience for help when my vocabulary ran out.

The very first day, I found an old friend at my lectures. It was a French doctor who had been working at the Instituto Nacional de Cardiologia in Mexico, and who had taken care of me on one occasion when I had overexerted myself. He saw to my health through our whole stay in France and invited my wife and me to many very delightful occasions at his apartment. He has visited America several times since, and we have had the chance to return his hospitality on our own soil.

The mathematicians took us completely into their family life. I saw a lot of dear old Hadamard and his wife, both of whom seemed to us almost ageless, although they were in their eighties. Fréchet and Bouligand were also our hosts.

I gave several outside lectures, some of them on engineering topics before a group from the Ecole Supérieure de Télécommunications. I also gave a philosophical lecture in a hall almost under the shadow of the home of Existentialism; the apartment of Sartre. We went to a salon at the apartment of one of the philosophy professors, where I was lionized in the thorough French manner.

I spent much time gossiping with Freymann in his back office, playing chess in the Bar Select on Boulevard Montparnasse, and in the other minor amusements of the city. We were great frequenters of the movies, and we learned to know a little bit about the good restaurants and cafés of Paris.

I had written another book for general consumption the previous year. It was a more popular account of cybernetics, with the social elements underlined. This was *The Human Use of Human Beings,* which had been published by Houghton Mifflin and has appeared as a paperback among the Anchor Books of Doubleday. I now tried to vend it to a Paris publisher for translation into French. I finally found such a publisher in M. Dufèze, of the Editions des Deux Rives.

Peggy visited us at Easter, and the family went on a trip to Nancy, where I gave a talk. Laurent Schwartz and his friends were as cordial as they had been on the previous occasion. My wife's French is better than mine, and Peggy found herself quite able to take part in our conversation and in our social life.

Later on in the spring, when my lectures in Paris had come to an end, Margaret and I went to Madrid. The summer before this last French trip, there had been a meeting of the world Mathematical Congress at Cambridge, Massachusetts, in which I had participated. At that time, the Spanish were very cordial to me, and I had received an invitation to lecture in Madrid. I protested that they might not like my views when they found out what they were, but they pooh-poohed the matter.

I accepted their invitation. My host had meanwhile read some of my writings and had decided that my views were dangerously liberal for me to present in a totalitarian country. Although I speak Spanish as well as I do French, he requested me to give my lectures in French; as I am now convinced, so that they should be less generally understood. He enjoined me to speak of engineering and mathematics only, and not to speak of anything political, philosophical, or biological.

We were put up at an excellent hotel and received the most splendid hospitality, but throughout the whole period we felt that we were being isolated from the country and kept in ignorance of what was going on. In view of our knowledge of Spanish and our experience as travelers, this ignorance could not be maintained, for I used to go out for walks in the nearby park and talk with the people there, and Margaret and I made a train trip to the Escorial. When our host found out that we had thus eluded his surveillance he was very angry, and he was even angrier when another Spanish friend offered to take us on a motor trip to Seville.

Thus we were glad to get out of the country and return to the free life of France. We spent part of our vacation in the delightful Basque town of St.-Juan-de-Luz. There I turned myself in all earnest to a task which I had begun in America and continued in Paris and Madrid—the writing of the earlier volume of this autobiography, which appeared under the title of *Ex-Prodigy.* It was a tremendous emotional strain to relive the severe experiences of my childhood as an infant prodigy, but the writing of them was also the best sort of psychiatric therapy.

We found some difficulties in securing transportation home, and we returned to Paris to clear up this matter, together with other questions of importance. We settled in a delightful leftbank hotel near the church of St. Germain. Then we went back to Savoy to rest until we were ready to return to the States.

One of the doctors of the Savoyard town where we stayed, was the father of our Paris medical friend. Toward the end of our stay in Savoy, my overexertion in lecturing and in writing sent me to bed with a racking headache, and for a while I had to go to the Geneva Canton Hospital. Meanwhile, my Paris friend had written to his father suggesting a suitable treatment, and with the aid of this I was soon much relieved. Even so, our trip to the boat at Genoa was a great deal of torture, and I was handed over to the care of the ship's surgeon. He continued the treatment, and by the time I reached home I was a reasonably well man but dead tired.

My wife and I went almost immediately to Mexico, where the university was celebrating its four hundredth anniversary. It was a time when honorary degrees were given out, and I received one of them. Mexican festivities are lavish in the extreme, and the two weeks of ceremonies, while delightful, exhausted me. I continued my work with Arturo until we returned to the States, in January 1952.

Even before this, Indian mathematicians had begun to negotiate with me to have me visit India and lecture there. It was not until the Christmas holidays of 1953 that I felt that I had any right to go.

35 India

1953

In December 1953, I started on a seven weeks' lecture trip in India as a guest of the Indian government and a group of government-supported institutions. The occasion for the trip was the All-India Science Congress at Hyderabad. A trip of this sort had long been under discussion. It had hung fire for some years because it was difficult to make the several demands of the Indian Government and my own meet one another precisely, not only in time but also in the nature of the trip itself. I was disinclined to take the trip alone without the protection and support of my dear wife; but this was manifestly impossible to ask for if my stay were to be brief, nor was it really practicable at the time for me to suggest a longer stay.

However, it became clear fairly early in 1953 that matters were going to break just right for something like a six or seven weeks' trip to India. My health, which had been rather strained by the severe ordeal of my recent successes, had recovered sufficiently to make it clear that with a bit of judicious care I should be equal to the trip. Moreover, my daughter Peggy was obviously on the point of becoming engaged to a young engineer and college friend, John Blake. In fact, the engagement was announced early in the fall term of 1953. On the one hand, this made it essential that my wife remain in the United States to take care of the manifold preparations for a wedding. On the other, it caused a lively period of preparations, during which I should be for a time more or less of a superfluity in my own household. I closed the arrangements and left the United States on the nineteenth of December, to return a week before my daughter's wedding, set for the twentieth of February.

As in the case of my earlier experiences in China and Mexico, so in my Indian trip my motive was more than restlessness or idle curiosity. More and more Indian authors are publishing in our scientific journals, and we need the Orient more and more to supplement a West which is showing the intellectual and moral enfeeblement following two World Wars. I was glad to have the opportunity to see something of yet another new recruit to international scientific life and to get the feel of its atmosphere.

I had, of course, been prepared somewhat for my Indian voyage by my earlier trips abroad. My visits to China and Japan had given me a degree of insight into the Orient and into the special problems of countries combining great intellectual ability with great poverty and now just beginning to enter upon the stage of a truly international scientific life. On the other hand, Mexico, with which I have become thoroughly familiar in the course of a decade, combines some of these problems with those belonging to a tropical climate. I had known many Indian students and colleagues both in the United States and in England. These people, many of whom I was to see again in India, had given me a certain picture of specifically Indian matters, and in particular of the strong religious attitudes which underlie Indian life.

I left Boston for Paris by air on the afternoon of December nineteenth. I am not an enthusiast for air travel. The trip is too short to encourage new contacts or to permit one to undergo the spiritual preparation for new and intense experiences.

There was a strike on at the Paris airports, and we were not sure of our destination. In fact, it was not until we had made an unscheduled landing at Shannon that we learned that we were to leave the plane at Brussels. I finally learned that we would be forwarded to Paris by a chartered bus, and not by plane. We drove hour by hour through Belgium and crossed the frontier well along in the day. Arriving after dark at the Gare des Invalides, in Paris, I found that my further trip by Air India was still unsettled because of the strike.

The three days which I spent in Paris as a result were a delightful combination of visits to friends, impromptu lectures, and contacts with my publishers and my colleagues. I had the good luck to find that there was a small sum waiting for me at Hermann et Cie. in the way of royalties I did not even know existed. But poor Freymann, who broke this good news to me, died of a stroke soon after my visit.

I found that I would have to leave Paris on the night of the twenty-third for Geneva, from which my plane was to leave for India. I spent Christmas Eve in Geneva with a neurologist whom I had seen many times before and whom the family knew very well.

To step upon a plane of Air India was to enter India in Switzerland. The pilots were Indians—largely Parsis—as were the two hostesses. The crew contained the extra contingent of servants which one must always expect in the Orient, and the meals were specially prepared to suit the religious and dietary demands of the passengers. Since I had announced myself a vegetarian, I found that this was an ideal arrangement for me.

We landed in Bombay, where I was rushed through customs and immigration inspection with the help of the secretary of the Indian Atomic Energy Commission, who had been deputed to take care of me and the other foreign scientific visitors in Bombay who were arriving for the All-India Science Congress. We hit it off from the beginning, and he took me out to the beach at Juhu to have tea with him and his wife under the palm trees. The cordial reception which I found upon first putting foot on Indian soil continued throughout my whole stay in the country.

After settling down at the Taj Mahal Hotel, which is a fascinating combination of the East and the West, I went the next day to the cornerstone laying of the new Atomic Energy Institute on military territory near the harbor. There was an interesting group of notables present, including Nehru himself, who gave a short and excellent speech. Among the visitors present was the Cardinal of India, a tall Goanese gentleman representing the old Portuguese religious tradition in India. This Christian tradition, like the old Syrian Christian tradition of the South, is often ignored by foreigners or, at any rate, not sufficiently emphasized. Actually, the Portuguese are older in India than the Mogul emperors. Notwithstanding the fact that Goa is not at present in India itself (as of the date of my writing, at least), Goanese are found all over India, particularly on the Bombay side. They consider themselves a thoroughly Indian element in the population. I had the pleasure of meeting Goanese officers both in the Army and in the Navy, and it was manifestly clear that they considered themselves, and were regarded by the others, as true Indians.

Another thing that struck me was that the cornerstone ceremonies were in English. English remains one of the chief languages of India, even though there is a deep-seated movement of the politicians to replace it by either Hindi or the local languages, and to confine a thoroughly English education in India to the people who, in part at least, are of English origin.

These people, the Anglo-Indians, are an older and more important element in the population than many might be inclined to expect, and they are having rather a thin time under the pressure to accept themselves as Indians and to be accepted by other Indians. In fact, there is no doubt that this is the only real future for them.

Anglo-Indian ladies, together with Parsis and southern Christians, serve as air-line hostesses for the very peculiar reason that this job involves the wearing of European dress, and European dress for women is looked upon with very little favor by both Hindus and Moslems.

For all officer ranks in the Army and Air Force, and to a certain extent for all ranks in the Navy, English is the only acceptable language. This results largely from the technical nature of modern warfare and from the lack of adequate technical vocabulary and phraseology in any of the native languages, which indeed borrow heavily from English in these matters. English remains the language of Indian science in all its branches, and although there is an effort to switch over to Hindi for the future, it is still too early to say how successfully this move will be.

The English rule in India has lasted about as long as the Mogul rule before it, and its effects were not transitory. It is of course fashionable for patriotic Indians to play up their own history rather than that of the English, and even to write of the Indian Mutiny as a war for independence. Nevertheless, the deep hatred for England which characterized much of the first half of this century has largely subsided, and there is probably no foreign country that is so well thought of by India as England, now that respect for England has ceased to be a sign of subservience to an alien rule.

The Englishmen who have helped India through its transition to a full independence, and who remain on in India in positions for which no adequate Indian replacement has yet been found, are regarded not only with respect but also with affection. I am referring to such people as Lord and Lady Mountbatten, to the high army officers who have stayed on, on loan in technical jobs, and to a certain small group of heads of scientific institutions.

What I am saying here goes for all of India, but I was particularly struck by the role of the English language in the South. I have been told by Indian friends who are not professional Anglophiles that in the city of Madras, ninety-five per cent of all classes of the population understand English and speak it tolerably. This is probably due to the fact that Tamil is a very difficult language, and that it does not pass current outside its own region in India, and that Hindi is quite as foreign and quite as difficult for a Tamil as English can be.

One finds Tamil-speaking men in all parts of India who, because of their native ability and intellectual discipline, are more than likely to use English as the habitual vehicle of their communication with their north Indian friends.

After the laying of the cornerstone, I went to a party at which Nehru was the chief guest and had a chance to meet him and to see the enormous respect and even reverence with which he is regarded. He seemed rather tired and frail, but I hear that he has great resources of strength. These India and he will need; for in the Congress Party, of which he is the leader, there appears to be no man of even approximately his stature as a second-in-command.

The day after the cornerstone laying I went by plane to Ahmadabad. I was sent there to participate in the meeting of the Indian Academy of Sciences. Here the chief figure was Sir C. V. Raman, the physicist and Nobel laureate.

I was a house guest of Professor Vikram Sarabhai, the physicist, who turned out to be a close friend of my friends the Vallartas. In fact, the Vallartas had been house guests there shortly before me. Mme. Sarabhai is a well-known dancer in the classical Indian style, and she maintains her troupe of dancers and musicians in the house. I had seen her perform with them in Mexico at the occasion of the four hundredth anniversary of the university.

In view of our having friends in common, the Sarabhais took me literally into their family. In fact, I was invited to participate in the weekly dinner of the larger family, which took place at the house of Vikram Sarabhai's father, a leader in the textile industry in Ahmadabad.

At the family feast—I should say clan feast, because the Indian family is a more comprehensive group than the Western family—I was the only non-Hindu present and, as an indulgence to my foreignness, I was granted the use of a table at which to eat, while the rest ate off little platforms on the floor.

Among the members of the Sarabhai family present, perhaps the most interesting was Vikram's maternal grandfather, an old gentleman who had served many native states as a dewan, or prime minister. Slender, ascetic, and unbelievably aristocratic-looking, such old men as Vikram's grandfather, fill an important role in the development of the new India. The Vedic injunctions lay down very precisely the proper course of a man's life. According to my understanding of them, which I am not sure is literally authentic, a man should spend twenty years as a youth, twenty years as a soldier (or, perhaps more precisely, as an active participant in the struggles and controversies of the world), and twenty years as a head of a family—that is, of the larger family in the Indian sense. After this, it was recommended that he become a *sunnyasi,* or religious recluse, and that he devote the few years left to him to the contemplation of the divine and to the attainment of that very Indian sort of salvation which is known as nirvana.

In this way, he may interrupt the ordained sequence of reincarnation into new animal and human bodies.

The classical type of *sunnyasi* still exists in India, and the story of one of these is given in Kipling's tale "The Miracle of Purun Bhagat" in the *Second Jungle Book*.

Nevertheless, the purely contemplative life is not enough for an India which is mistress of its own fate, and which needs to have so much done to interrupt the continued succession of poverty, ignorance, and misery that it cannot spare its able and experienced men the luxury of securing their own salvation. Thus, the deep and religious impulse of these fine old men to the otherworldly has been sublimated into a selfless service of the needs of the community, in those matters in which experience and integrity are indispensable, and in which there is no personal motive of advantage.

The Indians are not slow to find in their own scriptures a justification for this more worldly and practical but equally religious equivalent for the life of the *sunnyasi*. In doing this they are on a perfectly sound basis. No country can make adequate use of motives and modes of action which are merely passed on to it from the outside, but must find somewhere in its own body of tradition and in its own soul the moral sanction for the developments which are necessary to meet new problems.

Like other modern people, some of these old gentlemen wear Western costume, but there are many who cling to the costume and ways of their own country. When they do so, they exhibit an astonishing grace and beauty. It is remarkable how aristocratic a simple wool shawl can look when it falls over the shoulders of a beautiful and gentle old sage, in the lines of the garments of a Buddha in one of the temples of Ajanta or Ellora.

At the Sarabhai family dinner, Sir C. V. Raman and Lady Raman were present. Raman is both an important individual and a type significant in the new development of India. He is a southern Brahmin with a subtlety and depth of intellectual power which belong to many southern Brahmins as their birthright. However, there is in his character an additional positiveness and definiteness which show a familiarity with authority and a readiness to take an active part in life. Raman is an applied physicist and a good experimenter rather than the sort of theoretical physicist one might expect in India.

I gave a couple of lectures at Ahmadabad, one on my attempts to do something in quantum theory and one on the theory of prediction. I then went on to Poona, where I visited the university, which is headed by one of these fine old Indian gentlemen of whom I have spoken. I also talked at the National Chemical Laboratory. I met students there who were working on diverse and important problems in physical chemistry. The head of this laboratory, G. I. Finch, is the great climber of Mt. Everest, who is extremely popular among his subordinates and among Indians generally.

India has made a right-about-face in its attitude to the English. It had been difficult to make friends across the barrier between the rulers and the ruled. The ruler may be a snob, and the subject may fear that he is playing the sycophant and identifying himself with authority for his own advantage. The separation of India from England has greatly facilitated true Anglo-Indian friendship.

The wise and moderate counsel of Gandhi and Nehru has contributed much to making England the most popular western country in India. Nor must we forget the intelligent policy followed by the Mountbattens, as the last viceroy and vicereine, in relinquishing British authority in India and in making ready for the new order. I will not say that there does not remain much criticism of the British policy in the past, but this is combined with the healthy realization that such criticism is a very poor refuge from facing the living problems of an old-new country.

After my return to Bombay, a Russian scientific delegation came to India to participate in the Indian Science Congress that was to take place in Hyderabad. Obviously, one of their purposes was to perform indirect propaganda by showing the Indians the cream of Soviet science. On the whole, the members of this delegation were well-chosen, for while it contained a few of those who were principally political apologists, it also contained a number of sincere and unpolitical scientists of high rank and fine personality.

Throughout my trip I was to be lodged at the same hotels at which the Russians were placed. It was necessary for me to define my policy from the start. I thought, and I believe rightly so, that any display of personal hostility was unjustified and would not redound to the good name of America among the Indians. I went to several members of the delegation and said, “Look here, we're to be together for some weeks, and I don't want to embarrass you or myself. Let's be friendly and talk freely over such scientific matters as have no technical or political implications.” I found a ready response to my attitude, and at no time in the trip did the real scientists of the delegation embarrass me by anything that could be propaganda, or any digging for information.

At the beginning, the Russian group consisted entirely of the people who had been picked as scientists. Of course, some of these scientists were in the fields of philosophy of economics, which naturally meant that they were expected to take an orthodox Marxian point of view. The remaining delegates seemed very much like scientists anywhere else and did not seem to be held to any sharp party line on their respective scientific problems.

At this early period, conversation with the Russian group was relatively easy. Later on, they accumulated a body of translators and the like from the Bombay consulate and from their embassy at Delhi. This body was, in fact, of comparable size with the original delegation, and consisted of people who did not make on me and on the Indians nearly so favorable an impression as the scientific delegates themselves. They appeared rather to be policemen whose main purpose was to shepherd their charges away from possible western European and American influences and to prevent them from saying or hearing anything that might have been injurious to their government.

Scientists who had been conversing with me in very usable and effective English were forced by these channelers to speak almost nothing but Russian. Then the entire atmosphere of the meeting became, I shall not say hostile, but distinctly less open and friendly. Every time one of us fell into conversation with one of the Russian group, there was an interpreter sitting on a chair nearby, either translating from the Russian, or at least taking in every word that was said in English. Occasionally they seemed to give the high sign to their charges that further conversation was out of bounds.

Thus, the Russians tended to withdraw themselves into a compact mass, dining together and having very little contact either with the foreign delegates or with those Indians who did not manifestly form an organized group of Soviet sympathizers. Here I relate what I saw, although I was told that toward the end of the meeting the Russians did in fact secure some more general and independent contacts.

Shortly after the Russians came, we moved on to Hyderabad, where all the foreign delegates were given quarters in the Hill Fort, a rather shabby and run-down palace which had formerly belonged to a son of the nizam. Here some of the vast rooms were turned into dormitory spaces, and I found myself sharing one of these with a couple of elderly and distinguished English scientists. After a certain amount of feeling one another out, we got along very well together.

The isolation of the Hill Fort was most suggestive of the setting for an Agatha Christie detective story. Here we had together under one roof, and dining at the same table, a delegation from Russia and a mixed lot of other scientists like myself. Indian scientific notables and cabinet ministers came and went every few hours, but except for this we were quite alone, and none of us spoke a word of any Indian language. We gradually became a little fed up with the schism which divided us into two groups between whom there was a minimum of communication, and we westerners decided that the non-Russians should sit in alternate chairs at the table and force the Russians to take places in between us.

The day came when we non-Russians set out on a chartered plane for Aurangabad, from which we took a bus to the marvelous cave temples and sculptures of Ajanta and Ellora. Whatever the English have done in India, they have conserved its antiquities and historical monuments. The Indians are continuing in the good tradition which they found when they came to power by continuing to preserve these monuments and by encouraging their citizens to learn something about their own great past.

We spent a couple of nights in Aurangabad and left one morning for Hyderabad in a plane which we had expected to bring the Russians, who also were to visit the caves. There had been a series of mishaps in their setting out, and not only did they cancel their flight, but they remained plane-shy for the rest of their stay in India. Their decisions to discontinue the Ajanta trip were made rather thoughtlessly, for the Indians had put themselves out to find the Russians accommodations at Aurangabad.

We teased the Russians more than once, and I must admit they were good sports about it. When there was a meeting of Friends of China at Hyderabad, they were actively prominent, but I found that none of the Russians knew a word of Chinese. I displayed my knowledge of Chinese a bit ostentatiously, and I ribbed the Russians on their lack of knowledge of such an important world language. Furthermore, I asked them why they did not break ranks and make closer contact with the people of the country.

I did this with the knowledge that a recommendation from a westerner like myself would have a decided negative value for them. In fact it was clear that their policy of sending delegates *en masse* was a weak one. Many of the westerners were rich in Indian contacts and could really meet the Indians at their homes.

At the end of our stay in Hyderabad, some of us were invited to a series of informal parties at which members of the cabinet of Hyderabad were present, and to some at which they were hosts. It was most heartening to see Moslems and Hindus working together without discrimination of creed and to find Hindu ministers at a breakfast table presided over by the wife of a Moslem minister.

I took the plane from Hyderabad to Madras, where I was met by my old friend, Vijayaraghavan, whom I had known some eighteen years ago in England and later in America, where he had visited my family. At the time he was a slender young man concealing his Brahminical topknot in a spotless white turban, which my two young daughters played with. He left a deep impression on them, and I believe in fact that they named a doll after him. He visited us again in 1952, and this time he did not wear a turban, for there was no longer any topknot to conceal. My grown daughters regarded him with the same affection which they had shown him as children.

In Madras he was my kind friend, host, and counselor; and though I stayed at the hotel, not at his house, we spent the entire waking time of my stay together and I dined several times at his house. When one realizes that I am by Hindu standards a *mlechchha* an outcast, and that a generation ago any Brahmin would have considered himself polluted by my very presence at meals, I felt that this was a very considerable favor and token of friendship. We used to go in the hours of dawn for a swim in the magnificent surf of the Indian Ocean, and he would bring along his daughter and his little grandson.

I gave a scientific lecture at his institute and met his charming group of friends. I received a splendid idea of the lively and cordial intellectual life of Madras. I lectured to a group of Vijayaraghavan's friends on the automatic factory and its possible effect on the future India. I think I was almost the only one there who wore European attire.

Finally, Vijayaraghavan, together with his mother and his daughter, went with me to a little cloth shop by a temple in the Mylapore suburb of Madras and helped me pick out for my daughter Peggy a splendid saffron silk sari with dark-red gold-woven trimmings, together with the material for a blouse to go with the sari.

On many occasions during my stay, we talked over many things both scientific and personal, and we speculated much on the lives that our grandchildren might live and whether they might not find a better world in which religious and racial prejudices should have abated and in which all the peoples could meet for all purposes in an atmosphere of universal humanity.

From Madras I made a brief excursion to the delightful city of Bangalore, where I saw more of Raman and I took an active part in lecturing and in the intellectual life. Thence I returned for a week as a guest of the Tata Institute at Bombay. Bombay was full of first-rate scientists, both Indian and foreign, and I found a rich opportunity both to teach and to learn and, in particular, to collaborate with and to criticize the work of several young mathematicians.

My special crony during this episode was Professor Kosambi, who had been a boy at the Cambridge High School during a period when his father, a refugee from British India, was working over the rich Sanskrit material of the Harvard Library. The son, perhaps owing to his American early training, has been a bit more of a fighter and a bit less of a serene Indian scholar than most of his fellow countrymen. However, I found that he was not the only Indian to counter my admiration for the serenity of the Indian soul with an equal admiration for the drive of the westerner.

Among the other Bombay scientists whom I met were Masani and Chandrasekharan. Masani is a Parsi, and I saw a good deal as well of his Parsi colleague Bhabha, of the Tata family. I found the Parsis an extremely interesting group, accepting the new India very thoroughly. Yet they were partly divided in soul between their patriotism and the quasi-European position, almost as westerners, which their small minority of a hundred thousand souls had accepted under the English.

My stay in Bombay was one of the most profitable parts of my Indian trip, and I shared my very newest work with my Indian colleagues. When I went to India I had already been at work on the problem of prediction of multiple time series such as, for example, the weather at two or more points. This led to a certain formal mathematical problem in factoring what are known as matrices. I thought that I had a complete solution of the problem already, but when I spoke to Masani, he showed me that the question should be conceived in a larger way than that in which I had conceived it and that there remained much to be done. While in Bombay, I turned my maximal effort toward the solution of the problem and was luckily able to close the books on it.

I think that the fact that I was engaged in active new creative work in India, rather than the mere presentation of work already done, brought me much closer to the Indian mathematicians that I otherwise could have hoped to come. At any rate, I tried to live up to my opinion that the best and indeed the only way of teaching advanced students in science is to participate with them in a common enterprise.

I gave a talk to the Bombay Rotary Club on national and racial relations, and I met an interesting assembly of former M.I.T. men, who seemed to be taking a very active share in the new national development of India. I also visited the College of St. Francis Xavier, where the Spanish Jesuit fathers seemed a much-beloved group, fraternizing freely with their Hindu, Parsi, and Moslem students. Indeed, I found the Church in India a much freer and more liberal institution than it had seemed to me in the Spain from which these fathers came.

From Bombay I went by airplane to Calcutta, where I had the privilege of working for a week at the Indian Institute of Statistics, headed by Professor Mahalanobis. The Mahalanobises received me as a house guest, and I was admitted to a charming degree of intimacy with the family. I was given a room which had been intended for the old age of their friend, Rabindranath Tagore.

There was a most interesting group of Indian and foreign scientists going and coming, and I profited by the wise and understanding criticism which Professor Bose, of the University of Calcutta, made of the new physical ideas of Armand Siegel and myself. I gave a number of lectures to the staff of the Institute, and I kept myself at their disposal for discussions of their own research.

I used to go to a nearby temple—the one that forms the scene of some episodes in the film *The River*—to think over my scientific work and I had the pleasure of being received cordially by one of the devotees there. He was a Post Office official who came every day to worship. He was bearded and dressed in a scrupulously neat Indian costume. He invited me to see the inner precincts of the temple, which, until recently, had been closed to non-Hindus.

The Mahalanobises and the charming company that gathered at their house talked over world science and world politics with me with a great freedom and directness. They sent me to see the sights of Calcutta, including the zoological gardens and the art museum.

I followed up my trip to Calcutta with a visit to Benares and later to Agra. Benares struck me as a sinister fairyland. Agra, on the other hand with its palaces and tombs, including the Taj Mahal, was a lesson on the possibility of combining kingly lavishness with discipline and proportion.

From Agra I went on to Delhi. There I saw the fine National Physical Institute run by Professor Krishnan, and I was much gratified to see the equal emphasis on scientific progress and on the setting-up of a corps of workmen to make this scientific progress possible. The work that was being done on the use of solar energy was already beginning to yield results, and promises even greater ones for the future. If it is merely confined to cooking by solar heat instead of over flames of burning cow dung, it will already have made an important contribution toward the improvement of the fertility of the Indian soil.

I lectured at Krishnan's institute as well as at the university. At the university, I took up the theme on which I had already spoken during my popular lecture at Mylapore, of the significance of the automatic factory for the future of India.

My stay in India led me to reflect on the future role of the country in an industrialized and scientific world. As I have said, Indian scientists are the intellectual equals of those in any country. On the other hand, the class of skilled technicians, the non-commissioned officers of science and technology, are much more difficult to recruit. In artistic matters Indian craftsmanship is excellent, but it tends to lack the precision and uniformity demanded by the workshops of the West. Much brave work is being done in recruiting a cadre of these noncommissioned officers, very largely within the military services themselves and in the great new national laboratories. The National Physical Institute depended for its supply of skilled workmen on the Sikhs, who show the same abilities in the workshop which have made them in the past one of the mainstays of the Indian army. However, the facilities of the country up to the present have rendered the new class of skilled workmen into which they are entering a rather limited one.

At the bottom of the population there is an unlimited supply of unskilled and not too efficient labor, which makes a country very susceptible to a devastating proletarianization of even worse character than that which took place in England under the early days of the industrial revolution.

In view of these circumstances, I doubt whether India should undertake its industrialization in accordance with the accepted western pattern of mass factory labor. This is one of the quickest roads toward an immediate industrialization, and it gives India a chance to capitalize on its unquestioned asset of mass population. But I doubt whether this process is worth the price in human misery. Wretched and undernourished as a villager is, the industrial city promises to be even more wretched and to deprive the urbanized villager of whatever very small status he may have under Indian conditions of poverty. The unchecked growth of a nineteenth-century factory system is already making the outskirts of the great city into an unlovely hybrid of Indian famine and Manchester drabness.

I am not willing to ignore the possibility that the future industrialization of India may bypass much of the drabness and misery of Manchester or Chicago through the early introduction of the automatic factory. Misery is a result of unemployment, but it is even more a result of the sheer lack of goods. The automatic factory makes its demands on human efforts not at the bottom but at the very high level of the scientist-engineer and at the relatively high level of the small group of highly skilled trouble shooters and maintenance workers. It is quite in the cards that India can supply both of these within a matter of decades, while it can not supply a large group of fairly skilled factory workers able to earn enough to maintain them in a half-decent life for a large part of a century.

Of course, I may be wrong. The hothouse atmosphere of rapid industrial growth under the regime of the automatic factory may conceivably foster evils greater than any which it can alleviate. I do not know. What I do know is that the introduction of the new economics of the automatic factory may take place in India faster than most of us are willing to admit and that it might well be an easier avenue toward a prosperous and effective industrialized country than any of its alternatives.

In other words, this is a possibility which we indeed may have to discard, but which India cannot afford to discard without a thorough consideration of what it means. I am told that Nehru is interested in thinking out the possibilities of this alternative path to industrialization.

36 Epilogue

I am writing the last pages of this book at the age of sixty, an advanced period in the life of a creative mathematician. However, I am still at work, and I would not like to think that my efforts are now over. Many of my ideas still contribute to the growth of engineering and physics, so that a book of this sort can be nothing more than an interim report.

Many scholars find it interesting to speculate whether their motives for entering scholarship and their later success are due more to heredity or to environment. In my own case, it is particularly difficult to separate these two factors, because, to a large extent, my heredity was my environment.

I owe to my father not only his share of those genes which I carry but the sort of training which he conceived to be proper for a boy with exactly those traits of character which I derived from him. Without my share in Father's nature, I would have been an unfit subject for his training; and without his training, the potentialities which I derived from him might well have gone undisciplined and unrealized.

One part of Father's own outlook and one part of the training which he gave me was a very thorough synthesis of the theoretical and the practical. Father was a philologist who regarded the history of languages not as the quasi-biologic growth of almost isolated organisms but rather as an interplay of historic forces. For him, philology was a tool of the cultural historian, exactly as the spade is of the archeologist. It is not surprising that the son of a father who could not be contented with the formal and the abstract in the study of languages should himself fail to be contented with that thin view of mathematics which characterizes those mathematicians who have not made a real contact with physics.

My father was one of the most independent of men, and I could never have been a loyal son of his without declaring my own independence, even of himself. Fundamentally, he did the research he liked to do, and that was the kind of work which I had seen about me from my earliest childhood.

His work was disciplined not in accordance with the injunction of others but in accordance with the inner demands of a stern and self-critical nature. As my father's son, I could do nothing else but proceed in this pattern.

I have done those things I have done not in response to orders from outside but because my wishes followed a pattern which appealed to me and because the individual pieces of work which I have done have seemed to build up in a definite organized direction. My discipline has been a self-discipline, in the image of the discipline imposed upon me as a child by my father.

The discipline of the scholar is a consecration to the pursuit of the truth. It involves a willingness to undergo such real sacrifices as are demanded by this consecration, whether they are sacrifices of money or sacrifices of prestige, or even in the extreme (but not unprecedented) case, of personal safety. However, the main part of this discipline is intrinsic and belongs to one's relation to science itself rather than to one's reaction to the external environment within which science is carried on.

In the first place, discipline does not preclude the making of errors. What it does preclude is the retention of an error which has clearly and distinctly betrayed its wrongness. If a theorem is inconsistent, or if a proof refuses to become complete under the greatest pressure which you can exert, cast it from you.

This is the negative side of intellectual discipline. There is, however, a positive correlate to this. If a theorem merely looks grotesque or unusual and if your maximum effort cannot discover any contradiction, do not cast it aside. If the only thing that seems to be wrong about a proof is its unconventionality, then dare to accept it, unconventionality and all. Have the courage of your beliefs—because if you don't you will find that the best things you might have thought about will be picked up from under your own nose by more venturesome spirits; but, above all, because this is the only manly thing to do.

I am lucky to have been born and to have grown up before the First World War, at a period at which the vigor and *élan* of international scholarship had not yet been swamped by forty years of catastrophes. I am particularly lucky that it has not been necessary for me to remain for any considerable period a cog in a modern scientific factory, doing what I was told, accepting the problems given me by my superiors, and holding my own brain only *in commendam* as a medieval vassal held his fiefs. If I had been born into this latter day feudal system of the intellect, it is my opinion that I would have amounted to little. From the bottom of my heart I pity the present generation of scientists, many of whom, whether they wish it or not, are doomed by the “spirit of the age” to be intellectual lackeys and clock punchers.

Have I gained or lost from my father’s unconventional training? I do not know, for I have had only one life to live. My conjecture is that under a more conventional and milder regime I might have come through with less emotional trauma, but that I would not have developed the strong individuality of my scientific vein, which was due to early contact with a very powerful and very individualistic man. It was this struggle to maintain my individuality in the presence of a tremendously vigorous father which certainly gave the very specific form to my work which it later assumed.

While I might have achieved something under another training, one thing is clear: that without any training and guidance at all, my career would have been hampered and my productivity would have been distorted. It is very easy for a constitutionally vigorous mind to fritter its power away in trivialities. I put the highest value on my early contact with the standards of the intellect, and even though quite a different contact might have set me up as a scholar in another way, the absence of contact would have left me an ineffectual crank. I know of a number of cases where the relative paucity of scientific contacts, while not absolutely fatal, was still damaging and limiting.

A scientist must know what is being done in order that the very individuality of his own work may come to full fruition. He must live in a world where science is a career, where he has companions with whom he can talk and in contact with whom he may bring out his own vein.

It may well be true that ninety-five per cent of the really original scientific work is being done by less than five per cent of the professional scientists, but the greater part of it would not be done at all if the other ninety-five per cent were not there to help create a high level of scientific opinion. Even the self-trained scholar must pay tribute to that atmosphere of disinterested scholarship created by the universities, which furnishes the frame within which he may operate.

There is no doubt that the present age, particularly in America, is one in which more men and women are devoting themselves to a formally scientific career than ever before in history. This does not mean that the intellectual environment of science has received a proportionate increment. Many of today’s American scientists are working in government laboratories, where secrecy is the order of the day, and they are protected by the deliberate subdivision of problems to the extent that no man can be fully aware of the bearing of his own work. These laboratories, as well as the great industrial laboratories are so aware of the importance of the scientist that he is forced to punch the time clock and to give an accounting of the last minute of his research. Vacations are cut down to a dead minimum, but consultations and reports and visits to other plants are encouraged without limit, so that the scientist, and the young scientist in particular, has not the leisure to ripen his own ideas.

Science is better paid than at any time in the past. The results of this pay have been to attract into science many of those for whom the pay is the first consideration, and who scorn to sacrifice immediate profit for the freedom of development of their own concepts. Moreover, this inner development, important and indispensable as it may be to the world of science in the future, generally does not have the tendency to put a single cent into the pockets of their employers.

Perhaps business has learned to take long risks, but they must be calculable risks, and no risk, by its very nature, is less calculable than the risk of profit from new ideas.

This is an age in which the profit motive is exalted, often, indeed, to the exclusion of all other motives. The value of ideas to the community is estimated in terms of dollars and cents, yet dollars and cents are fugitive currency compared with that of new ideas. A discovery which may take fifty years before it leads to new practice has only a minimal chance of redounding to the advantage of those who have paid for the work leading up to it, yet if these discoveries are not made, and we continue depending on those which already exist, we are selling out our future and the futures of our children and grandchildren.

Like a tradition of scholarship, a grove of sequoias may exist for thousands of years, and the present crop of wood represents the investment of sun and rain many centuries ago. The returns of this investment are here, but how much money and how many securities remain in the same hands, even for one century? Thus, if we are to measure the long-time life of a sequoia grove in terms of the short-time value of money, we cannot afford to treat it as an agricultural enterprise. In a profit-bound world, we must exploit it as a mine and leave a wasteland behind us for the future.

There are scientific ideas which we can trace clearly to the time of Leibniz, a quarter of a millennium ago, which are just beginning to find their applications in industry. Can a business firm or a government department, moved primarily by the immediate needs for new weapons, compass this period of time in its backward glance?

The great grove of science must be left to long-time institutions capable of formulating and maintaining long-time values. In the past, the Church was one of these institutions, and, even though it has somewhat fallen from its high estate, it has given birth to the universities and other intellectual institutions, such as academies, which themselves have a continuous life lasting over centuries.

These long-time institutions cannot and do not ask for an immediate translation of their hopes and ideals into the small change of the present day. They exist on faith, the faith that the development of knowledge is a good thing and must ultimately conspire in the good of all men.

The problem of planning for a long future is not unknown to business, particularly to that most sophisticated one the insurance business. The art of the actuary is to estimate risks. But the insurance business in general is not concerned merely with protection against destructive risks. The same companies that sell insurance also sell annuities. In a similar way, any long-time planning for the future must involve the discussion of rare and incalculable favorable circumstances as well as rare and incalculable catastrophes. One of the rare and incalculable benefits for which we must provide if the race is going to survive is the sudden emergence on the scene of great and original intellects.

A policy which integrates the gifts of the intellect into a long-time policy must transcend the lifetime of short-time institutions such as everyday business, and must be transferred to more stable institutions, like the foundations and the universities, which at least contemplate such a continued existence.

I am not alone in saying these things, but I am swimming counter to the major currents of the times. It is popular to believe that the age of the individual and, above all, of the free individual, is past in science. There are many administrators of science and a large component of the general population who believe that mass attacks can do anything, and even that ideas are obsolete.

Behind this drive to the mass attack there are a number of strong psychological motives. Neither the public nor the big administrator has too good an understanding of the inner continuity of science, but they both have seen its world-shaking consequences, and they are afraid of it. Both of them wish to decerebrate the scientist, even as the Byzantine State emasculated its civil servants. Moreover, the great administrator who is not sure of his own intellectual level can aggrandize himself only by cutting his scientific employees down to size.

The limiting case of the great scientific institution, by which we may test the soundness of the principles on which it acts, is the writing shop of the monkeys and the typewriters which, in the course of the ages, will almost certainly succeed in making every possible combination of the letters of the alphabet and the words in the dictionary. What is the real value of the work of the monkeys and the typewriters? Sooner or later, they will have written all the works of Shakespeare. Are we then to credit this mass attack with creating the works of Shakespeare? By no means, for before writing the works of Shakespeare, they will almost certainly have created just about all the nonsense and balderdash conceivable.

It is only after the non-Shakespearean has been thrown away, or at least an overwhelming part of it, that Shakespeare will stand out in any significant sense, whether theoretical or practical. To say that the monkeys' work will contain the works of Shakespeare has no other sense than to say that a block of marble will contain a statue by Michelangelo. After all, what Michelangelo does is purely critical, namely, to remove from his statue the unnecessary marble that hides it. Thus, at the level of the highest creation, this highest creation is nothing but the highest criticism.

Of course, the large laboratory can make out a limited case for itself. However, it is perfectly possible for the mass attack by workers of all levels, from the highest to the lowest, to go beyond the point of optimum performance, and to lose many really good results it might obtain in the unreadable ruck of fifth-rate reports. This is a real observable defect of large-scale science at the present time. If a new Einstein theory were to come into being as a government report in one of our super-laboratories, there would be a really great chance that nobody would have the patience to go through the mass published under the same auspices and discover it.

The great laboratory may do many important things, at its best, but at its worst it is a morass which engulfs the abilities of the leaders as much as those of the followers.

I have not found the great laboratory the medium in which I can develop my work with the freedom which I need to express its particular message. This may well be a limitation of my own nature, but my experience with young men has shown me that it is a limitation shared by many of those who have much to say. I hope and pray that the value of this important stratum of scientific workers will not be thrown away on the basis of short-sighted considerations of facility of administration and the trend of the times. Certainly I owe my own continued ability to do useful work to the cordial spirit of the administrators of M.I.T. and to their habit of protecting me from unwarranted claims on my time and from those who may have a narrow conception of my function.

After thirty-six years of functioning in the free atmosphere of M.I.T., and at the age of sixty, I do not find myself at the end of my scientific interests nor, I hope, of my achievements. My collaborative work on brain waves seems to me about to blossom out into a considerable science. Similarly, my joint studies with Armand Siegel on the Brownian motion and on time series is leading me to a reconsideration of the relative parts played in this world by cause and by chance. How many years may be granted me, if not to carry out this program of work myself, then at least to see that it is being carried out and to understand the share in it of my past ideas, I do not know; but even now I can feel reasonably sure that my scientific career, though it began early, is lasting late.

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