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Models of My Life

Herbert A. Simon

Printed on recycled paper and bound in the United States of America

To Dorothea,
so aptly named

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PREFACE TO THE MIT PRESS EDITION

A continuing interest in my autobiography, which has been out of print for several years, brings forth this new edition under the imprint of The MIT Press. I am especially pleased that the book has struck a responsive chord in people, some of them friends, some of them strangers, who find interesting and informative its picture of a life in science. The account of what I have done with my life (and what it has done with me) represents one scientist's view of the academic enterprise in general, and the research enterprise in particular: how one pursues a life in science, and the satisfactions (and occasionally frustrations) it can provide. Notice that I say, "a life *in* science," not "a life of science," for I have tried to tell the whole story of growing up and living, a story in which science has been a vital part but, for all that, only a part.

A number of readers have told me that the book's picture of a scientist's life resonated sufficiently with their experiences that they presented copies to their children or their students who were just embarking on such a life or considering it. I hope that those who find it among their holiday or graduation gifts and those who encounter it in their bookstores will be informed (and entertained) but not misled by it. Science has been very good to me, and if they have a vocation for it, it will likely be good to them.

The path I have followed has exposed me to a broad panorama of science. I have wandered widely (always for reasons that, at the moment, seemed compelling, you understand!) from political science and public administration, through economics and cognitive psychology, to artificial intelligence and computer science, with side excursions into the philosophy of sciencesometimes pursuing two or more of these simultaneously. My

central interest in decision making and problem solving led me to study the psychological processes of scientific discovery, which, in turn, induced explorations of many domains, especially in mathematics and the physical and biological sciences, that go well beyond the areas mentioned above. So I hope that readers will not find it hard to relate some of the events they meet here to their own favorite sciences. I have also spent many hours and days engaged in the politics of science (I hesitate to call it "statesmanship"), and particularly in helping to bring the social and natural sciences together so that they can discharge their joint responsibility for informing public policy.

Apart from correcting errors of fact and typography, I have not revised the text of the first edition. The five years since I wrote *Models of My Life* have been busy and exciting years, in which I have continued to pursue my (lengthening) research agenda, but their flavor is not unlike that of the years just preceding them. (Pages 327 331 still describe quite well the domains in which I am active.) I have no startling new truths about Life to announce; my adventures continue to be mainly adventures of the mind, as I am curently eschewing new adventures by airplane. Hence I am content to leave the story in its original form. I hope you enjoy it.

> HERBERT A. SIMON
> PITTSBURGH, 1996

ACKNOWLEDGMENTS

Parler sérieusement, c'est parler comme on se parle à soi-même. Comme l'on parle au plus près.
Paul Valéry, *Instants*

INTRODUCTION

Proust titled the final volume of his lifework "The Past Recaptured." But, of course, the past cannot be recaptured. Memory is overlaid with later memory, mangled by self-justification and self-pity, guarded by self-interest, rent by great gaps of forgetfulness. Proust did not recapture his past, but reconstructed it, marvelously, with an insight he most surely did not have as he lived it.

It would be ridiculous for me to imagine that I could imitate Proust, and stupid of me to try. A novelist gives us metaphors and concrete instances; almost no axioms, no theorems, certainly no proofs. It is true that a Tolstoy sometimes violates the rule, delivers sermons, and offers generalizations. Even Proust musesfor example, in his peroration on memory. But by and large, it is the business of the novelist (and perhaps also of the autobiographer) to give us the data; it is up to readers to induce the theory from them.

As I am a scientist, a theorist, I will violate the rule even more often than Tolstoy did. Rereading my manuscript, I find that the offenses become more frequent as time gains on me. How does one age, gradually replacing action by reflection?

I have encountered many branches in the maze of my life's path, where I have followed now the left fork, now the right. The metaphor of the maze is irresistible to someone who has devoted his scientific career to understanding human choice. And if I had not encountered labyrinths early in life, I would have met them later in the stories of Jorge Luis Borges, as we shall see.

In describing my life as mazelike, I do not mean that I have made a large number of deliberate, wrenching decisions to go off in one direction or another. On the contrary, I have made very few. Obvious responses to

opportunities and circumstances, rather than studied decisions, have put me on the particular roads I have followed.

Unlike the original labyrinth of Minos, my mazes were without minotaurs, were, in fact, benign, never putting me to a life-threatening or even a career-threatening test. The reader will encounter surprises along the path and, I hope, many scenes of interest, but if you are looking for dangerous adventures, or minotaurs, or the heroism of a Theseus, you will have to pick up another book.

When I first came to write a chapter-length autobiography, I called it "A Theory of My Life." Now, enlarging and extending it, I give it the more accurate title *Models of My Life*. The change from singular to plural eliminates any promise of unattainable scientific truth. Lacking that, why not offer several explanations of events, when none seems certain? The reader may wish to provide others.

But there is a further reason for using the plural. It is a deniala denial that a life, at least my life, has a central theme, a unifying thread running through it. True, there are themes (again the plural), some of the threads brighter or thicker or stronger than others. Perhaps clearest is the theme of the scientist and teacher, carrying on his persistent heuristic search, seeking the Holy Grail of truth about human decision making. In my case, even that thread is woven of finer strands: the political scientist, the organization theorist, the economist, the management scientist, the computer scientist, the psychologist, the philosopher of science.

Then there is the theme of the private person: of growing up, of love and family and friends and travel and leisure. A third thread traces the university politician, seeking to build and shape the environment for his scientific work. A fourth winds its way through New York City and Washington, even Beijing and Moscow: the theme of the science politician, concerned with the health of social science, with science as adviser to the *polis*even (grandiosely) with the possible contributions of science to maintaining world peace and preserving the global environment.

These four themes (and a few others as well) each place me in a rather separate or separable role, with its own maze as setting. The stingy paymaster of time, doling out only twenty-four hours a day for everything, binds the roles together as they compete for those hours to pursue their searches.

Which of the wanderers through these different mazes will step forward at the call for the real Herbert Simon? All of them; for the "real" self is an illusion. We live each hour in context, different contexts for different hours. To say, truly, that we are actors does not make us "unreal" or hypocritical.

We act out our lives within the mazes in which Nature and society place us.

It is true (as a *Vanity Fair* cartoon once mischievously pointed out) that the stage and early film character actor George Arliss, in all the disguises of the characters he played, was still a recognizable George Arliss. But which was the real George ArlissShylock or Disraeli or the familiar nose and eyes and chin that peeked out through the wigs and makeup in all these roles?

I have been a scientist, but in many sciences. I have explored mazes, but they do not connect into a single maze. My aspirations do not extend to achieving a single consistency in my life. It will be enough if I can play each of my roles creditably, borrowing sometimes from one for another, but striving to represent fairly each character when he has his turn on the stage.

Nor will the characters speak with a single voice or style. The simple tale teller's account of a boy's day or a China adventure or an affair of the heart will not do for setting out a piece of intellectual history or explicating the Byzantine politics of the National Academy of Sciences. So it will not be a single drama at all, but twenty-three one-act plays, some sequels of others, some unattached. So much for the unities.

The "models" of my title is consistent with earlier titles of my works. *Models of Man* was a collection, published in 1957, of mathematical theories of psychological, sociological, and economic phenomena. Then, in 1977, I published *Models of Discovery,* a collection of my papers in philosophy of science. The first volume of *Models of Thought,* my papers in cognitive psychology, appeared in 1979, the second in 1989. Finally, the two volumes of *Models of Bounded Rationality,* collecting my papers in economics, were published in 1982. Even though this volume contains no mathematics or reports of controlled experiments, it is intended in the same spirit as those others.

In my earlier attempt at describing my past, undertaken at about age sixty, I organized the story as a triptych. The first panel stretched from June 15, 1916, to June 15, 1937; the second, to December 15, 1955; the third covered the remaining twenty-odd years up to December 10, 1978. These dates, we shall see, were selected for good reasons: they coincided with my twenty-first birthday (an especially eventful moment), the invention of our first artificial intelligence program, and the conferring of my Nobel Prize.

Triptychs are more conventional, and perhaps easier to design, than tetraptychs (a word my dictionary does not deign to recognize). But now, at age seventy-four, I have to add a fourth panel, narrower than the others, and undoubtedly throwing the whole scheme out of shape. I am somehow comforted by the knowledge that there is a celebrated tetraptych at the altar

of St. Mary's Church on the central square in Krakow.* But it is wholly symmetrical; and besides, the two central panels can be folded back to reveal that, underneath, it is a triptych after all. The whole of this account may have more the appearance of a sequence of snapshots than of a moving picture. But it is mainly as snapshots that I remember my life, a few brilliantly sharp and clear scenes that punctuate the continuous journeyAndo Hiroshige's drawings of scenes along the Eastern Road from Edo to Kyoto.

But all of this will become clear in the telling, and I have said enough about my intentions and how I will organize the story. Let me conclude with a brief outline of the four panels that can serve as a thread to guide the reader through the maze.

The first panel, "Journey to a Twenty-first Birthday," carries me from birth through my undergraduate education at the University of Chicago and my first job, ending on my twenty-first birthday. It is divided into scenes from Wisconsin (chapters 1 and 2) and scenes from Chicago (chapters 3 and 4). Milwaukee was my home through my seventeenth year, and the University of Chicago campus for the next six.

The scenes of the second panel, "The Scientist as a Young Man," have a more varied geography than those of the first. After marrying in 1937, my wife and I remained in Chicago until the autumn of 1939 (chapter 5). We then moved to the University of California at Berkeley for three years (chapter 6). In 1942, we returned to Chicago for another seven years, during which time I served on the faculty of Illinois Institute of Technology (chapter 7). Chapter 8 tells a story of loyalty and loyalty investigations, running from the 1930s to 1963; the events are collected in one chapter for coherence.

We left Chicago, in 1949, for Carnegie Institute of Technology in Pittsburgh. The first seven years in Pittsburgh saw the founding of the Graduate School of Industrial Administration (chapter 9), a research program in economics and organization (chapters 10 and 11), and the birth of artificial intelligence (chapters 12 and 13).

The third panel, "View from the Mountain," runs from the beginning of the artificial intelligence research to my Nobel Prize in Economics in 1978. Chapter 14 discusses the progress of our research, while chapter 15 picks up the thread of my personal life. Chapters 16 and 18 recount events on the Carnegie campus. Chapter 17 reviews some of my scholarly controversies.

Chapters 19 and 20 describe my life away from the Pittsburgh campus,

* The guidebooks call it a pentaptych, counting the two side panels, the pair of folding panels, and the fixed central panel. The Oxford English Dictionary *does* recognize *pentaptych*.

the former dealing mainly with science politics in New York and Washington, the latter with my wider-ranging travels about the world.

The fourth panel, "Research After Sixty," begins with an account of the Nobel award (chapter 21). This is followed, in the same chapter, by an account of my research since 1978, of developments at Carnegie Mellon University, and of my continuing activities in science politics. Chapter 22 tells the story of travels in the past decade in China and the Soviet Union. Then chapter 23 says something about the general views that have guided my choices in life.

In the Afterword I reflect on the methods I have used in my research. It may be taken as a warning that I don't intend the last chapter to be the final one; if permitted, I will stay at this pleasurable, exciting work a while longer.

PROLOGUE

Dorothea and I climbed off the plane in Frankfurt early one foggy June morning, claimed our luggage from a long bench in the gloom of the shedlike airport building, passed the perfunctory customs check, and signed the documents for our rented Volkswagen. Darmstadt was our first destination. Two decades had passed since the end of World War II before I felt I could be comfortable visiting Germany. Now, in 1965, we decided to spend a few days searching out my ancestral places in the Rhineland, prior to visiting the Schwarzwald and Switzerland. Our search centered on the fifty-mile strip of the middle Rhine, from Mainz south to Mannheim.

By the time we started south on the Autobahn, the mists had lifted above the road, disclosing the serried tree trunks of neat German forests on each side. As we reached Darmstadt, the sun was making an occasional appearance through broken clouds. In the square where we stopped to buy the makings for our lunch, and a paring knife and can opener we had forgotten to pack, the Saturday market was in full bustle. I was pleased that my Milwaukee German was mostly understood, and I had only occasional recourse to my pocket dictionary. A knife was *ein Messer,* but what was a can opener?

Our immediate goal was the campus of the *Technische Hochschule,* close by the castle. There my father, Arthur Simon, had received his engineering diploma in 1902, and there he had returned once more in 1912, a successful alumnus, to deliver a lecture on his work in electric motor design.

We wandered among the buildings, and through the largest of them, not looking for anything or anyone in particular but hoping to catch a glimpse or a sniff of that sixty-year past. The chunky stone-and-brick main classroom building looked exactly as it had on the 1897 postcard, *Gruss aus Darmstadt,* I found in my father's desk drawer after his death. Inside, the building

was austere and bleak, like any old-fashioned engineering school building, whether at Illinois Tech or Carnegie Tech. The bulletin boards expressed mostly the political concerns of 1965. I was moved, but more by my sense of the occasion than by anything I saw. It was solely in my imagination that the mists lifted a little to let me peer into the past.

My father remained only about a year in Germany after earning his engineering degree, spending the time partly in postgraduate study at Darmstadt and Heidelberg, and partly working for the Siemens firm. Why did he leave? The official story, told by both my mother and father, was that grandfather Joseph had promised his son Arthur a trip around the world after his graduation. Setting out, he got as far as Milwaukee, where he had cousins, found a job with Cutler-Hammer, an electrical manufacturing concern, and, seven years later, a wife, and settled down. He got only as far west as San Francisco, and that only many years later.

When I was a boy, that story seemed straightforward; it now seems problematic. Given that Milwaukee was a German city, why did Arthur prefer it to the homeland? And why did he trade his German identity as rapidly as he could for an American one, especially in a city that admired German culture?

German was never spoken in our home (although my mother was ungrammatically fluent in it) except when something was to be kept from my brother, Clarence, and me, and during my fifth year, when Grandma Simon paid us a year-long postwar visit. My father, justifiably proud of his accurate command of English, was only occasionally betrayed by an idiom or verb tense"Today I go to the office early."

He was scornful of "hyphenated Americans"German-, Italo-, Polish-, or whatnotwho divided their loyalty between two countries. During World War I, he tried to volunteer, but was rejected, ostensibly because of poor eyesight, probably in fact because of his recent immigration. Since America had not attained its modern sophistication in security checks, he spent the war designing battleship gun turret controls.

At the same time, no bitterness tinged his accounts of his German boyhood. His nostalgia for the family vineyard was transmuted into a loving and energetic pursuit of gardening in our small yard and in a solarium constructed by enclosing half our front porch. Occasionally he would spend Saturday afternoon pruning the grapevines of a landed suburban friend. He was not uncritically admiring of the American way of life, hardly viewing autos and jazz and baseball as progress over the cultured Germany of his youth.

But Prussia was not Germany, and *Prussian* was for him an epithet that conjured up militarism and mindless nationalism. My father sometimes told

of a relative, a baker's apprentice, who had avoided conscription by floating across the Rhine on a bakery breadboard and had found his way to America (the last leg of his journey by boat, I think). Although my father was generally fond of Wagner, he could not endure the nationalism of *Tannhäuser*.

Perhaps this was the clue to his flight from Germany, if flight it was. But another possibility came to my mind some years later, while I was sorting his papers after his death. He had kept only a few mementos of his German youth. Apart from the postcard view of the Darmstadt campus and his small sliderule, which he used all his life, there was just one other item from the college: a formal note, dated June 15, 1899. Translated into English, it read as follows:

> Explanation
>
> In my two meetings with Herr Student Arthur Simon, I have let words fall from which the aforesaid gentleman felt that his honor was wounded.
>
> I hereby explain that it was absolutely not my intention to insult Herr Simon or in any way to wish to spite him.
>
> I present therefore to Herr Student Arthur Simon my apology for the insult offered to him.
>
> In attestation of which my own signature,
> Ernst Wassermann

The signature was witnessed by a second student.

What had the insult been? And why had my father, who was not a feuding person, kept that note in his desk drawer for forty-nine years? Perhaps it was just college high jinks. Perhaps he was proud that, as a young man, he had been willing to challenge a fellow student to a duel. His sabres and masks were stored in our attic, where my brother and I had discovered and often played with them (apparently with caution, for there were no bloody accidents). All of this conformed with the romantic etching of the Heidelberg castle that hung on the wall near our front staircase in Milwaukee. My father's college days were confounded, in my boy's mind, with scenes from *The Student Prince*.

But even student duels have ugly causes. Remarks about women can cause duels. So can ethnic slurs. The sight of that note of apology stirred memories of an anecdote my father had once or twice told about a student who thought he was paying a Jewish comrade a broadminded compliment by calling him a "*white* Jew." That circumstantial evidence is certainly not

enough to identify Herr Wassermann's offense or to convict him. But what did students in the Rhineland in 1899 talk and quarrel about during the evenings in the *Bierstube?* History tells us of one topic that came up often and hotly. In Paris, in May 1899, the Court of Cassation had ordered a new trial for Captain Alfred Dreyfus. The second court-martial convened in August; its ambiguous decision was handed down on September 9. Herr Wassermann's apology is almost synchronous with the final burst of public turmoil, in the Rhineland as in France, that the affair of Alfred Dreyfus had sustained for many years.

What was the situation of a Jewish engineering student in Darmstadt at the turn of the century? What echoes of the Dreyfus case reverberated on the campus and in the society? And what were the employment and career prospects of that Jewish engineer when he graduated? (Recall that a young Albert Einstein was seeking a university post and encountering anti-Semitism at nearly this same time.)

Here the mists closed in again. When the questions first occurred to me, Arthur Simon was already dead. Nothing we could now seek out in Darmstadt would reveal the secret of that student quarrel. Probably it is best, in any event, to leave intact the official version of the migration, a trip around the world that was aborted. The mists perform an important social function when they place a statute of limitations on man's memory of ancient wrongs.

Turning west from Darmstadt, and stopping to eat our bread and cheese on the now sunny roadside flanked by vineyards, we reached Mainz in the early afternoon. There Arthur Simon had attended high school, living with his retired grandfather, for the small farm village of Ebersheim could support only a grammar school. Mainz's past was easily captured in its half-ruined Romanesque cathedral, stripped almost bare of ornament by fire bombings during the war. The very austerity of its vast interior, emphasized by the symmetry of the two altars facing each other from the ends of the long, stark nave, called back a remote pre-Gothic time, yet a time when Mainz had already lived a thousand years.

Jews had come to Mainz, it was said, with the Roman soldiers. Jews were there, convenient victims for pogroms, when hordes of Crusaders flowed up the Rhine valley on their way to the Danube and the passes of the Alps. Jews were there still to enjoy the protection of the Holy Roman Emperor and to loan their funds to him, and to the Archbishop. I have no notion of whether my ancestors were among those Jews, or whether they came to the area much later, driven by harsh conditions from France or the Netherlands. The solid river mists in Mainz allowed no glimpse of a connection with that Roman and medieval past. I like to think that, if they parted, they would

show me a homeland nearly as ancient as Israel. But that is just romantic fancy; there is no evidence.

At the hotel desk in Mainz, it was not easy to get directions to Ebersheim. The name of the town was familiar, but the clerk and the manager could not quite agree on which road we should take. The town was a few kilometers to the south (a half-day's wagon trip, my father had said). We set out on a southbound highway. Repeated questions along the way and several detours brought us across an open rolling plateau covered with vineyards, into steep little hills, then, turning back in a northeasterly direction, across a narrow bridge into the south end of the village.

We were on the *Römerstrasse*—the road of the Romans—the main and almost only street of Ebersheim. Along that street, as my father had related the town legends, Roman legions had marched into the hills from the Rhine valley, starting on the long trip across the *Hunsrücke* that would take them from Mainz to Aachen.

Somewhere on the *Römerstrasse,* near the Catholic church, we expected to find the house of the Simon family, with a little roadside shrine in the garden wall. The shrine was there, as was the house—or at least one that could pass for it, with the same gray stucco walls as all the other houses of the village. A few yards up the street, in front of the Catholic church, a long stone wall was carved with the endless lists of sons of the village who had fallen in World War I, and the somewhat shorter list of their sons and successors in World War II.

A block beyond the church we found the town hall. In a small room on the second floor, a clerk brought out the black, bound books of births and deaths. Again the mists parted a little, allowing us to gaze back a century and a half, to Napoleon and the Confederation of the Rhine, to records written in French for those early-nineteenth-century years. We found the entry for Arthur Simon: May 20, 1881, son of Joseph Simon, *Weinhändler* (wine merchant), and Rosalie Herf. Joseph Simon's entry was there, too, but now the mists were descending again, and we were lost in a profusion of Simon and Bernays ancestors and cousins, never quite certain that we had traced the right line. There had been much intermarriage between these two Jewish families of Ebersheim, and marriage with the similarly scattered Jews of nearby villages.

The designation *Weinhändler* was a surprise, for my father had never described the family as being merchants. He had always talked proudly of the vineyard that had been in the family for seven generations (a century farther back than even these books could carry us). Was "wine merchant" used as a subterfuge at a time when Jews could not own land? Was Joseph Simon both vintner and merchant? He was, after all, able to send his son,

Arthur, to the university. No one to whom I could put these questions was alive.

The clerk at the town hall directed us to the mayor's house. Yes, the mayor's father, a man in his seventies, remembered Frieda Simon (my father's sister); she had been his classmate in the grammar school. (Their ages didn't exactly bear this out, but the intent was friendly.) We were directed to the Friedhof, the Jewish cemetery, on a steep little hill nearby. The grass was neatly trimmed, and a dozen tombstones, many with the Simon name, stood in irregular rows. It was difficult to decipher the Hebrew inscriptions, so we did not learn much more about the tangled genealogy of the Ebersheim Simons. But it was pleasant enough to walk among them on the sunny, peaceful hillside.

The attempt to peer through the mists did not end in Ebersheim. Rosalie Herf had come from Wörstadt, a few kilometers to the south. As we again followed the route through the vineyards, we stopped to buy our usual cheese and bread at a little crossroads store (a mom-and-pop store, we would call it here). It was owned by a cordial elderly couple. They had relatives near Chicagoname of Bekenhof. Did we know them? We got involved in the usual friendly chitchat among travelers, except for one quick moment when a question crept uninvited into my mind, and stayed there because it could not be asked. Who had owned this little shop in 1930? Wasn't this kind of enterprise usually Jewish? What had become of the owners? It was an unfair suspicion, no doubt, but it settled a chill onto a warm human interchange. We paid for our purchases and continued down the road.

Wörstadt, a more substantial town than Ebersheim, gave us further glimpses of the past. The medieval city wall had been leveled to form a semicircular park and promenade through the town. We found the Jewish gravestones in a corner of the Protestant cemetery (much like Jewish Greenwood Cemetery, next to Protestant Forest Home in my native Milwaukee). One grave belonged to Aron Herf, a great-great-grandfather, who was born in Partenheim on March 9, 1797. So we had reached back to the eighteenth century. But further exploration took us no farther back. Inquiries in Partenheim, a tiny village on the plateau to the west, yielded neither town hall records nor Jewish cemeterybut a denial that the latter had even existed.

On only one other occasion have I tried seriously to raise the mists for a moment. Many years after our Rhineland journey, in the summer of 1977, we visited the wonderful, shabby city of Prague. In the Jewish cemetery, almost all that remains of the Prague ghetto, I walked along the narrow paths amid the crowded tombstones, fifteen feet or more above the surrounding streets, treading softly on the bones of centuries of anonymous

ancestors. They were goldsmiths, the family tradition said, and Alexander Goldschmidt had left with the '48ers, those who were unhappy at the failure of the revolution of 1848, for Chicago and the Civil War, and later a comfortable living as a wholesale whiskey salesman. Coming out of the cemetery, I visited the synagogue at its gates, now a museum of the Holocaust. How lucky were the descendants of Arthur Simon and Alexander Goldschmidt to have had these footloose ancestors, and to have been spared the anguish experienced by their kinsmen who remained behind.

Some speak of search for their roots. But that is a false metaphor. For these strands that we trace backward through time have no root tips. Each ancestral name and date we identify points back toward two more still hidden in the mists, and behind each of these, yet another pair. The mists that hide the distant past are no less dense, no less impenetrable, than those that shroud the future.

THE FIRST PANEL
JOURNEY TO A TWENTY-FIRST BIRTHDAY

Chapter 1
The Boy in Wisconsin

As my boyhood, reckoned through high school, ended nearly sixty years ago, I find it hard now to think of the boy as me. It is not that we are enormously different in our values or our personalities. (Physical appearance, alas, is another matter.) He was already pretty well shaped and even aimed before he left Milwaukee, beyond major redirection. But now, at this great distance, I see him in my mind's eye from outside, as "the boy." It will be easiest if I talk about his first seventeen years that way.

Answers to a few passport questions will introduce the boy and his family:

Born: June 15, 1916, Milwaukee, Wisconsin, in a rented flat, but soon (1918) moved with his family to a modest frame house owned by his parents in a middle-class neighborhood of the West (that is, the German) Side.

Father: The boy's father, Arthur Simon, was born on May 21, 1881, in Ebersheim, Germany, to Joseph Simon and Rosalie Herf, the seventh generation of a line of vintners and wine merchants; Jews, but by some dispensation, landowners (or landholders) a century before Napoleon overran the Rhineland. Arthur graduated in electrical engineering from the *Technische Hochschule* of Darmstadt and emigrated to Milwaukee in 1903. Employed as an engineer by Cutler-Hammer Manufacturing Company, and later also engaged in private practice as a patent attorney. Active in professional and civic affairs; awarded honorary doctorate in 1934 by Marquette University.

Mother: The boy's mother, Edna Marguerite Merkel, was born on January 20, 1888, in St. Louis, Missouri, a second-generation descendant of German '48er immigrants from Prague (Goldsmith, Jewish; Dahl, Catholic) and Cologne (Merkel, Lutheran). Her grandfather Alexander Goldsmith was a Civil War veteran, wounded at Chickamagua, afterward a whiskey salesman. Her grandfather Louis Merkel was a piano builder. His son

Charles, after the family business failed, became a piano tuner, who migrated, during the difficult economic times at the turn of the century, first to St. Paul and then to Milwaukee. Edna attended Milwaukee public schools and the Academy of Music. A piano teacher until she married in 1910, then a homemaker. Active in local musical clubs.

Other Close Relatives

Brother Clarence Joseph, five years older, graduated in law from the University of Wisconsin and practiced law in that state. Fond and usually protective of his little brother, he was absorbed in sports and restless at school. In these things, he was not at all the younger boy's role model.

Grandma Ida Merkel and Grandpa Charles lived at first two blocks away and then in the boy's homegrandfather until his death in 1928, grandmother through a long life to 1943.

Great-grandmother Anna Goldsmith (Omah), who died in 1921, played checkers, dominoes, and Old Mill (a slightly complicated form of tic-tac-toe) with the small boy for hours on end.

Uncle Harold Merkel, Edna's younger brother, who died at age thirty, in 1922. Graduated with distinction from the University of Wisconsin in law. A student of the economist John R. Commons and a La Follette Progressive, he worked subsequently for the National Industrial Conference Board. The copies of *The Federalist Papers* and William James's *Psychology* on the bookshelves at home had been Uncle Harold's. When the boy later was elected to Phi Beta Kappa, he inherited and wore Uncle Harold's key.

The Boy at Four

The boy was standing not far out on the wooden pier at the resort of Washington Island's West Harbor. Washington Island is the dot off the end of Door County peninsula, the thumb of Wisconsin that separates Green Bay from Lake Michigan. He had come on the overnight Goodrich Line steamer from Milwaukee, with his father, mother, brother, and his German grandmother. That summer, 1920, he had his fourth birthday. They had landed at South Harbor, for even then West Harbor was too shallow to accommodate the large lake steamers. Now, several days later, he was playing by himself on the small pier.

So far, all of this is reconstruction. The next is a vivid memory, his earliest. He was falling; he was falling off the pier and would surely drown. A memory of sheer terror. Then he was standing waist-deep in the water, undrowned and unhurtbut sobbing. Of course, the whole, thing is no longer a memory, just a memory of having once had such a memory. Mem-

ory and memory of the memory have probably been confabulated for at least sixty years.

Other memories, or memories of memories, of the Washington Island holiday remain. The family took a walk from the resort into the woods, which were dense and forbidding, the father in the lead. After some time, when they must have penetrated deep into the forest, the road took a turn and the resort buildings loomed up just ahead. They had never been more than a quarter of a mile away.

If the memory of the memory were not so clear, we would suppose that the whole story was made up after a reading of Proust's account of the Sunday walks in Combray. The memory even seems to incorporatebut that is surely a fabricationthe father's quiet satisfaction at having produced this surprise to cap the expedition. Perhaps that detail was added to the memory many years later. Perhaps all fathers smile with quiet satisfaction on safely and suddenly bringing their flock home again.

Then there was the strawberry patch episode, which also seems to belong to Washington Island. Whether during his fourth summer or on some later occasion, the boy was among a party picking wild strawberries. The others filled their pails in a few minutes; there were only a few strawberries in the bottom of his. How could the others see the berries so easily amid the closely matching leaves? That was how he learned that strawberries are red and leaves green, and that he was color-blind.*

Since the year of Washington Island was the year the boy's Grandma Simon visited, her presence tags other specific memories of it. Knowing no German, he memorized a little German poem that he recited to her, sitting on her bed, on the morning after her arrival. During the course of the year, he heard lots of German in the home, learned a little of it, and acquired the ability, which he retained, of speaking German without too strong an American accent.

Grandma Simon was a kind and genial woman, not at all religious. But she was a little shocked that the boy was allowed to play with the Russian Jewish girl next door. In spite of the language gap, the two boys became very fond of their German grandmother. One day, she took them to the circus ground, where the tents were being erected for the next day's performance. A roustabout offered Clarence a dime to carry two pails of water to the elephants. Not understanding the conversation, Grandma Simon dragged Clarence away, and eventually brought two howling children back

* In a surprising coincidence, a biography of the distinguished geneticist Hermann Muller mentions that the geneticist A. H. Sturtevant was also color-blind, and had learned of his defect in the same wayat a strawberry patch (Carlson [1981], p. 55).

home. Uncle Max made up for it by taking them to the circus the next day.

And a final memory. The circus parade always passed their house on its way from railway to circus ground, the elephants and a steam calliope bringing up the rear. The military parades also came down that street (I don't know why; it wasn't a main thoroughfare): the still-young veterans of World War I, then the middle-aged Spanish-American War veterans, and finally, the blue-coated veterans of the Civil War, some walking and some riding in cars.

The House

The house in which the boy lived from his third year until he left for college was a modest wooden structure on Juneau Avenue, in a thoroughly middle-class neighborhod, but just a block from Highland Boulevard, where stood the large mansions of the Davidsons (Harley-Davidson motorcycles), the Pabsts (beer), and other leading industrial families. The children of these wealthy families mostly attended the same public schools as the boy and his middle-class cronies, played football and baseball with them, and otherwise participated in the same classless (juvenile) society. But the boy's parents and their neighbors had no social relations with the Highland Boulevard families.

The long-time Socialist mayor, Dan Hoan, lived in a small house just around the corner, an acquaintance of the boy's parents. Since no horns emerged from Dan Hoan's head, and the Milwaukee city government operated precious few Socialist enterprises, the term *socialism* took on a very mild and benign meaning for the boy.

On its second floor, the Simon house had five bedrooms, one each for the boy's brother, his parents, his grandmother, his grandfather, and himself. On the third floor was the maid's room. There was always a full-time maid, the only departure from middle-classness and a concession to the father's European past. The maid was always a country girl, usually from a northern Wisconsin farm. If she stayed any length of time, and most did, for the boy's mother was a kind and democratic person, she became a member of the family, which assumed a certain responsibility for her morals and her future. Several of our maids married well and corresponded with my mother to her last days.

The maids baby-sat for the boy and his brother when they were small, sometimes adjudicated their quarrels, and occasionally had to take them down a peg or two when they became uppity. One farm girl, when exasperated beyond endurance, sometimes used a pungent retort that has become

a part of my democratic credo: "*Your* shit don't smell like ice cream neither." But mostly they were warmly liked aunties or older sisters to the boys.

In the winters, the second floor bedrooms received only such heat as escaped from the first floor; it was healthy to sleep in cold rooms and with the windows *always* open at least a crack. On arising in the morning, one made a quick dash from the icy bedroom to the heated bathroom. Homework (it never took him very long) was usually done in the warmth, downstairs at the dining room table. (Many years later, the boy's children had to endure unheated bedroomsto be sure, in a climate milder than Milwaukee'sbecause it never occurred to the boy, now a man, that people might sleep in heated rooms.)

The boy's room faced south, on Juneau Avenue, shaded by the great elm trees that arched over the street. It was a pleasant, small room, but he does not remember spending much time in it except to sleep. On summer mornings, he would often waken at or before dawn, dress, and trot out to Washington Park, a half-mile away. There he would find a crotch in a huge willow tree in which he could sit and read his book until it was time to return home for breakfast.

In addition to the holiday parades that passed along Juneau Avenue, there was also the daily parade of Harley-Davidson workmen who lived in the neighborhood and walked the half-mile to the company's main plant. The preschool boy liked to sit on his front steps and watch them, each with a metal lunchbucket and many smoking pungent pipes. They trudged westward at 7:30 A.M., then back again at 5:00, occasionally in pairs but often solitary. On Saturdays, they worked only until noon.

The blocks had alleys, many of them wide enough for baseball and touch football games, and there were still a few vacant lots in the neighborhood. The boys did not have to travel far or be accompanied by adults to play their games. Cars had mostly replaced horses and buggies for private transport, but Frankie Faulkner's grandfather (real estate) still drove his buggy, housed in the stable along the alley (and suspected by the neighbors of attracting rats). On a few occasions, the grandfather took Frankie and his friend Herbert for a buggy ride.

Horse-drawn carts delivered milk, ice, and breakfast rolls. Joe Rinello, the vegetable man, also came on his daily rounds with his wagon. In summer, his horse, Nellie, wore a straw hat with holes poked through it for her ears. In any season, Nellie was fond of sugar lumps, nuzzling the boy's palm with her wet nose as she took them. At Christmas, Joe brought a jug of red dandelion wine as his gift (these were Prohibition days).

For a few months, Joe's brother Frank appeared instead. A car had hit the wagon and injured Joe. He sued for damages, but was cheated by an

ambulance-chasing lawyer, and then arrested and jailed for a short time after he appeared at the lawyer's office carrying a gun. That was the only break I remember in his long and reliable service. The gun incident was almost the sole "criminal" event that affected the neighborhoodexcept, perhaps, for the rare stolen bicycle. Houses were not often burglarized, pedestrians were not robbed, violent family brawling did not seem to have been frequent.

Of course, not all of Milwaukee was quite so peaceful. It was not supposed to be safe to walk at night through the Third Ward, three miles from his house. The boy never tested to see whether that warning was valid.

School

At school, the boy soon learned that he was smarter than his comrades, and that became important to him. Although conscientious in his studies, he never had to work hard at them or to neglect friends or sports for schoolwork. I have two sets of recollections of him that are hard to assemble into a single, consistent piece.

There was the introverted boy, who had no difficulty amusing himself with books or toys or collections of stamps or (later) beetles. I see him sitting in the living room on a fall Saturday afternoonall his friends are at the movieswith a chessboard and a chess book in front of him, feeling rather lonely. He spent many hours that way. With a five-year age gap between his brother and him, he was nearly an only child. Although the boy was affectionate, he did not share his thoughts much with adults. He preferred to ask questions of them and listen to them. Suppertime was a time for conversation. His father enjoyed serious table talk, and the rules permitted vociferous argument. Politics or scientific subjects were frequent topics.

I can also see the boy with his father at the basement workbench, "helping," but mostly watching, while his father constructed a radio, the first in the neighborhood, or a model sailing sloop. Perhaps the father was impatient, or his youngest son lazy, for the boy never acquired any great skills in such crafts, although he liked to watch.

I see him curled up on the leather couch in his father's den, at the age of perhaps ten, proving to himself that he could understand *The Comedy of Errors*. The eleventh edition of the *Britannica* was there for him, too. The long bookcase in the den also contained many shelves of *Proceedings* of the American Institute of Electrical Engineers and the American Society of Me-

chanical Engineers, which the boy never tried to explore; and the multiple volumes of *The Historians' History of the World,* of which he read a little but which he did not find terribly interesting.

He kept his education entirely in his own hands, seldom asking for advice. The encyclopedia had an index, and the public library, a card catalog. In books left by his uncle or his brother, he studied economics, psychology, ancient history, some analytic geometry and calculus, and physics. Well before he was twelve, he had discovered the public library and museum, housed together in a building about three miles from his house. On Saturdays, he often trotted (no one jogged then) down and home againallowable as long as he returned in time for meals. He knew every exhibit in every room of the fine museum, and in his high school years received permission to enter the stacks in the science room of the library.

Summers were spent mainly in Milwaukee, except for a two-week family vacation, often in the North Woods. Since many of his friends were away during the summer, he was even more solitary than usual then. One summer, when he was about fifteen, he set himself the task of reading Dante's *Inferno* (with Gustave Doré's haunting illustrations), Milton's *Paradise Lost,* and a textbook on ethics; and he translated into wretched English doggerel Schiller's *Das Lied von Der Glocke*.

Many other solitary summer hours were spent collecting and identifying insects. He began to focus on beetles (color wasn't important in identifying them). Somehow, he became acquainted with the entomologists at the public museumhe probably took some specimens to them for identificationand received the privilege of working with them "backstage." His special sponsor was young Hy Rich, still completing his studies at the university, whose particular interest was an obscure family of tiny water beetles, the Haliplidae. Often during the summer, the boy accompanied Rich on collecting expeditions along the streams near Milwaukee.

Hy Rich was a semi-pro hoofer, having financed much of his education during the Depression by dancing in vaudeville. One noon he was giving a demonstration of his skills when the redoubtable department head (Reptiles and Invertebrates), Mr. Tower, returned early from lunch. I believe that Rich was forgiven, but the moment was vividly embarrassing. The boy's work as a volunteer at the museum continued for several years, especially summers, but produced no memories more indelible than that one. The boy, to his disappointment, discovered no previously unknown and unnamed species of beetle.

In fact, in all these activities there are few examples of what could properly be called "creativity." One summer the boy wrote some adolescent essays

on "infinity" and "the existence of God," but there was only one brief period of poetry writing, no attempt to compose music, no stab at the great American novel. He did not draw or paint.

The boy did not often take clocks apart, much less put them together again. By the time he finished high school, his chess game was moderately strong, but bookish. He was a student, who prided himself on being able to master anything, independently, and who was often able to make good his (private) boast.

His schoolwork was mostly enjoyablethere were almost no courses he actually dislikedbut it did not call for much originality. He did know the difference between memorizing and understanding, and did not give up on a topic until he understood it clearly. Physics and math were probably his favorite high school subjects. The physics was classical physics, well taught by Mr. Ehlman, but it seemed to him a "finished" subject, not leaving room for new discovery.

Two incidents in algebra left clear memories that may shed some light on his attitudes toward knowledge. While working some examples in his first algebra class, he discovered inductively the formula $(x + y) (x - y) = x^2 - y^2$. The beauty of it delighted him, and he showed it proudly to his teacher, Miss Thomas. When she challenged him to prove that his inductive formula would work for all x and y, he did not succeed.

In a subsequent algebra course, he was troubled by the fact that some quadratic equations had two solutions, some had one, and some none. The irregularity seemed ugly, although he could see the reason for it graphically. He was pleased to learn a little later that by adding complex numbers to the reals, all quadratic equations could be provided with exactly two solutions.

Similarly, he was at first bothered to learn that equality in the numbers of equations and variables was not a sufficient condition for a set of linear equations to have a unique solutionconditions on the rank of a determinant had to be added. These aesthetic reactions to regularity and irregularity seemed to reveal something of a Platonist within him, a desire to find pattern, and preferably simple pattern, in the world around him.

If the boy had a claim to creativity, it lay in the realm of politics. In about the fourth grade, he drew up a school constitution (students' rights!) and presented it, with the greatest trepidation, to stern Miss Walsh, the principal. Much to his surprise, he was praised rather than punished for his attempt at insurrection. After that, he was a great reviser of club constitutions and bylaws.

The boy knew that his father was an inventor and held many patents. He did not once ask, "Papa, how do you make inventions?" Perhaps he

thought it would be "cheating" to ask, that it would be no fun to invent if you had been told how. Being told how was different from reading in books; in the books, you had to dig it out for yourself.

He did often dream of discoveryor perhaps it was only the glory he dreamed of, and not the discovering. Such dreams captured much of his thought as he walked the half-mile to and from grammar school and, later, the mile to high school. For a long time, Napoleon was his greatest hero. And he resented Columbus for foreclosing the discovery of the New World.

Although he didn't ask his father how to invent, he enjoyed accompanying him on Saturday visits to industrial plants arranged by the Engineers' Society: to the Allis Chalmers factory, where the great generators for Hoover Dam were made; to the electrical generating station at Port Washington; to a research laboratory where a Tesla coil produced high-voltage electrical sparks across a twenty-foot gap; to a steel mill; and to a coke oven.

These visits left indelible impressions on him of modern industry, of the heavy, sometimes dirty and dangerous, tasks of blue-collar labor, of the cleverness of machinery, of the deftness of skilled machinists, of cranes lifting heavy weights and wafting them the length of the factory aisle. Did these visits put him in awe of engineering? Despite his enjoyment of them, they never suggested to him the idea of becoming an engineer.

Friends

This picture of the introspective, bookish, and sometimes lonely boy who was proud of his independence in learning shows nothing of the sociable boy who appeared at other timesfond of games and sports and his friends, a great joiner and organizational politician. During grammar school years, the time after school was for sandlot baseball and football during good weather, and for indoor games or sometimes skating during the cold and dark of the Wisconsin winter.

He was not much of an athlete. Because he had been "skipped" three half years, most of his friends were several years his senior; besides, he was more left-handed than right-handed and, as he often said by way of apology, "ambiundextrous." When teams were being chosen for baseball, he was among the last to be called by one of the captains, usually to inhabit right field. This often embarrassed him, but did not keep him away from the field.

His sociability did not often extend to fistfights. Since he was both small and rather timorous, he seized upon a saying attributed to the Revolutionary War guerrilla General Francis Marion: "He who fights and runs away will live to fight another day."

During high school, the sociable boy expended his energy in two directions. He was an active and enthusiastic Boy Scout, with a special love for camping and all manner of outdoor activities, summer and winter. The student clubs at his high school provided his second social arena. He was active in a debating society, a science club, the Christian Endeavor (while insisting on his agnosticism), the Latin club, and the student council, serving as president of most of them at one time or another. He took no part in high school team sports.

He was much attracted to girls, an attraction he experienced even as a kindergartener. The earliest in his memory was Mary Mueller, a blonde-haired, round-faced little Saxon girl who could have been the model for an angel in an Albrecht Altdorfer nativity scene. One of her successors was Mary Mitchell, by whom he was smitten at age thirteen on a visit to her family estate in Virginia, a visit I will describe shortly.

The boy's other grammar school memories have less to do with girls than with a little boy's discovery of his own sexuality, and of the proper street language to use in referring to it. Girls were theoretical rather than actual objects; when the game of "spin the bottle" was proposed at birthday parties, it produced more embarrassment than pleasure. (He was only twelve when he finished the eight grades of grammar school, and this was a time when children still came equipped with much ignorance and many inhibitions in this regard.)

High school was another matter. He could not keep his eyes off pretty girls. At first most of the worship was from a distance, because he was too shy to get on a dance floor with them and, usually, to date them. Besides, he was two years younger than most of his classmates, so that they tended to treat him as a younger brother.

It was easiest to talk to girls who were outgoing and intelligent but not pretty, because he could behave in a natural way with them, much as though they were boys. But in the presence of a pretty girl, his social skills fled immediately. Conscious of his (undefined) desires, he could not treat a pretty girl as a person.

In high school, imagination had to stand in at first for actual amorous experience. He became a voracious consumer of the vapid love stories that filled *Collier's* and the *Woman's Home Companion*. The heroines were usually wholesome (but very pretty) girl-next-door types with "tip-tilted" noses. The illustrations taught the boy how a tip-tilted nose looked. These heroines kissed the heroes (at the end of the story), but never climbed into bed with them, certainly not before marriage.

There were two girls in his school, both very pretty and one also very bright, who had the reputation of being "fast." Oddly, one of the pieces of

evidence usually cited was that they did not always go home from parties with the same boys who brought them. The reputation, quite apart from any facts, was enough to single them out for his distant attention. He hardly ever spoke to them, or had occasion to, and was understandably startled when, at a school meeting, during a vigorous dispute about arrangements for the June commencement, the bright one turned to him and said intensely, "You don't really hate me, do you?" Somehow, she had noted that the boy paid attention to her when they passed in the halls, and mistook the meaning of his glances.

The boy's first real love was Ginnie, whom he met on a nature hike when he was about sixteen. She was a couple of years younger than he, just entering high school. Ginnie had Irish beautyblack hair, blue eyes, and a boyish, athletic figure, just budding. Perhaps because she was young and natural, not yet fully aware of her beauty, he did not freeze in her presence. They had a fine, close friendship for about two years, in which they hiked, swam, went to parties (no dancinghe didn't dance) and meetings of church and social groups, and even took an occasional horseback ride together.

He had no exclusive rights, and Ginnie's mother had larger aspirations for Ginnie's upward social mobility than she thought it likely the boy could satisfy. But the relation (there were no "relationships" then) was smooth and extremely pleasant. The boy especially liked helping Ginnie with her homework, sitting close to her on a sofa or porch swing with an arm around her, occasionally glimpsing firm young breasts as she leaned forward to write something. It never went beyond that and some kisses, but both of them evidently found that just fine. He learned Mozart's "Piano Sonata in C Major," the familiar and easy K545, because he could then imagine Ginnie, who was a good pianist, playing it.*

How was he viewed, this sociable boy, by his friends and classmates? First, he was a "brain," but evidently modest enough about it so as not to offend. Besides, it counted for him that he was not a "grind." But he had a sharp tongue and an appetite for verbal swordsmanship. It was unpredictable whether one would get a compliment or a jab from him. He grew reasonably thick skin, to take the blows, verbal and physical, that were offered in return, but he became unsure about others' feelings toward him and shy about opening himself to them. For a time, he took as his hero Coriolanus, the Roman too proud to court the "mob."

In many respects, his youth was an asset. He could be forgiven many

* To this day, the man whom the boy became cannot hear or play that sonata, especially the slow movement, without a pleasant memory of Ginnieor, more likely, a memory of a memory of a memory.

things in his role as younger brother that he would not have been forgiven otherwise. Throughout the first half of his life, he retained a special affinity for working with men a little older than himselfSydney Kalmbach, his hiking companion, Milton Chernin at Berkeley, Don Smithburg and Victor Thompson at Illinois Tech, Lee Bach at Carnegie Tech. He was usually comfortable in the second-in-command role, which he preferred to think of as the "power-behind-the-throne" role or, more accurately, the "idea-man" role. In adult life, as he began to work with younger associates, he became an older brother.

I have said that the boy was a good listener. He was often sought out as a confidant, even by adults and even when he was quite young. When family feuds occurredand his grandmother had some talents in this directionhe often heard each side of the story from the principals. Before he was twelve, he had learned that quite reasonable and truthful people could perceive the same set of events in remarkably different ways. Sometimes he found himself the mediator, interpreting for each protagonist the viewpoint of the other. Whatever view was presented to him, he could see the merits in the opposing view, and often took it up.

In high school debates, the boy enjoyed taking the unpopular, underdog positionfree trade, unilateral disarmament, the Single Taxusually with conviction. His opponents could seldom match his logic or his careful preparation of evidence. Here he learned another important practical truth: You do not change people's opinions by defeating them with logic. People do not feel obliged to agree just because they cannot reply at the moment. The boy later used that insight many times in his own defense. He never felt obliged to assent to doctrineswhether Platonist, Thomist, Behaviorist, Libertarian, or Marxistjust because the arguments advanced for them seemed, at the moment, unanswerable. He learned to mistrust human real-time logic.

The debates contributed to his education in another way. They led him to read widely and deeply in economics and the other social sciences: Henry George's *Progress and Poverty,* Richard T. Ely's *Outline of Economics,* Norman Angell's *The Great Illusion*. He did not understand all of it, but he learned to read critically, using one book to argue with another.

One unpopular position the boy took in debates, but from conviction rather than cussedness, was in favor of disarmament and strengthening the League of Nations. As a small boy, he was as ready as any of his friends to play cowboys-and-Indians and cops-and-robbers. His father's .22 rifle and Colt automatic pistol fascinated him, and he was allowed to shoot them a few times in the North Woods. For a time, he had a soldier suit, complete with World War I leggings, which he could never keep properly wrapped.

He wore it when his father took him on a visit to New York and left him for a few days at the army post on Governor's Island, where a noncommissioned officer of the family's acquaintance lived.

He had many lead soldiers, his favorite Christmas gift, and marched and fought them interminably on the living room floor. He became a battle buff, with a thorough knowledge of Waterloo and of Gettysburg and other Civil War battles (more comprehensible than those of World War I). Even in high school, a vocal pacifist, he wrote a term paper about Civil War battles. (His history teacher penciled a surprised comment on the paper.) His first ambition was to attend West Point, an ambition thwarted by his color blindness.

Some realism was injected into his dreams of military glory in a curious way. The uncle of Allen Schultz, a neighbor boy who was for some time the boy's best friend, was a surgeon who had served in World War I. He had kept an album of photographs (before and after surgery) of the most gruesome wounds he had encountered. It was a record, I suppose, of his and his colleagues' surgical skills. The boy and his friend discovered the album, were horrified almost to illness, but could not take their eyes off it or banish it from their nightmares.

The album presented a new view of war, soon eroding the boy's fascination with military affairs in the concrete, if not in the abstract. It made him receptive to antiwar books, such as *The Great Illusion,* and set him on the road to pacifism. The pacifism lasted, in turn, until the advent of Hitler. But now I am ahead of my story.

On Being Parochial

Through his vacations with his family and his own hiking and camping trips with his friends, the boy became familiar with most of eastern Wisconsin, but had rarely strayed out of the state before setting out for college. Once, when he was very little, grandmother Merkel had taken him to Chicago by boat. What he chiefly remembers (or was later teased about) was his puzzlement at not seeing the state line when the boat crossed it, and his whining complaints on the return trip that he was being kept up beyond his bedtime.

When the boy was seven or eight, his father took him along on a week's business trip to Washington and New York. The boy set out to keep a diary of the trip, but either he found himself too busy or the scenes were too hard to describe, for there are hardly any entries beyond Chicago. On the Pullman to Washington, he awoke in the mountains of Pennsylvania, which were

far more rugged than any Wisconsin hills he had seen, but still not real mountains, for there was no snow on the peaks.

In Washington, his father did his business at the Patent Office, and then they saw all the usual sights. The boy was especially impressed by the State Department building at 17th and Pennsylvania (now the old Executive Office Building), where the office suites had slatted doors to permit any breeze that stirred in the humid Washington summer air to pass through, and in front of each was posted a dignified black doorman. Nowadays, he recalls with wonder that they passed freely in and out of these buildings with no security checks. They stayed overnight in the Willard Hotel, still in the height of fashion.

In New York, he went to the top of the Woolworth Tower and visited the Statue of Liberty. As I mentioned, during the days his father was busy, he stayed with family friends at the army post on Governor's Island. It was a trip to remember, which apparently satisfied most of his needs for distant travel almost until his college days.

He strayed once more from Wisconsin, during the summer of 1929, months before the stock market crash. On a Caribbean winter cruise his parents had taken a year or two earlier, they had befriended a couple named Noble, belonging to a branch of the Reynolds tobacco family, I believe. The Nobles invited the Simons to visit them at their Virginia home, near Hampton Roads, and this was the boy's introduction to the Very Rich.

They were met, the boy and his brother and parents, at the train station in Norfolk, and taken by chauffeured car to the wharf, where they were ushered onto a sizable steam yacht. The yacht proceeded directly to the dock at the Nobles' bayside estate, where several sailboats and other craft were also moored. The Nobles had a son who was brother Clarence's age and a daughter, Mary Mitchell, who was the boy's age, and with whom he fell immediately in love.

The children enjoyed two idyllic weeks of swimming and sailing in Chesapeake Bay and a posh birthday party at a neighboring estate. They were chauffeured to Yorktown and to Williamsburg, just then under construction. Servants, all black and respectful, of course, were everywhere in the house and on the grounds of the Nobles' estate. With no trouble at all, the boy could transport himself backward a hundred years and experience the Old South. It gave him a queer feeling. It would be nice to report that he was morally offended by the plutocracy and racism that surrounded him, but there is no evidence that he was. What he mainly remembered were the pleasant times, especially the sailing and the trips on the steam yacht.

That same summer the boy and his parents drove to the Black Hills, on the dusty, partially paved roads of that era. (Those mountains didn't have

snow either, but at least they were officially called "hills.") It, too, was a pleasant excursion, but not nearly as dramatic and eye-opening as the trip to Virginia. He did not leave Wisconsin again until 1933.

On Being Different

Even when the social activist held ascendancy over the loner, the boy saw himself as different from his friends. His left-handedness, his brightness, and his color blindness set him a little apart.

One other difference was that the boy was conscious of being a Jewnot a religious Jew, for he had not been inside a synagogue, and he attended Congregational Sunday school through his grammar and high school years* but a Jew nonetheless. Sometimes he wished he weren't, although he hardly admitted that to himself, but mostly he felt proud of it and was always careful that his Jewishness be known to others around him. He didn't want to "pass." If there was any disadvantage or penalty in being a Jew, he would accept it rather than deny his Jewishness.

Coming from a minority culture, he could not be ethnocentric. And, since his red and green were not the red and green other people saw, he understood that the real world out there was not identical to the perceived world. Hence both ethical and epistemological relativism came easily to him.

His feeling of being different did not translate into rebellion against authority. Although corporal punishment had not been abolished in either his home or his school, he did not experience it often in the former or ever in the latter, and his memories of it are not vivid. It could usually be avoided by staying out of serious mischief. Being a good student gave him a large credit balance that could be traded against minor misdemeanors. It also gave him freedom to use his time as he pleased, even in school.

On a few occasions he felt his principles challenged. During the years he attended the Congregational church, communion services were held monthly. The minister would ask, "Will all members of this church please rise?" Two-thirds of those present would get to their feet. Then the minister would ask, "Will all Christians please rise?" Only the boy remained seated. It was very difficult, but he would have felt ashamed if he had falsified his beliefs publicly.

His high school class voted for the boys to wear white flannels at the

* The Grand Avenue Congregational Church was a social center for many of his schoolmates, especially during the boy's high school years, and he early became a regular participant in activities there.

graduation ceremony. He decided (it being during the darkest days of the Depression) that this was unfair to the boys from poorer families and persuaded many students to sign an agreement to wear their regular suits. On graduation night, he and two classmates appeared in their dark suits; all the others had somehow forgotten their pledges and found means to buy the white flannels. He felt a little betrayed, but proud that he had not yielded.

Graduation

The boy graduated from high school in 1933 at age seventeen. It had been, on the whole, a happy boyhood and adolescence. He had had to make almost no decisions, except to take what the world offered him, and the world had been generous. There were no hard choices at the branches of the maze.

But his picture of the future was vague. His answers to "What do you want to be?" had gone from "soldier" through "forest ranger" and "lawyer" to "scientist." His private answer was "intellectual." He calculated that if someone were to endow him with $50,000, he could live quite comfortably for the rest of his life doing what he did bestlearning. Perhaps the most unusual feature of his boyhood, as it influenced his future career, was his exposure, thanks to the books and the example left behind by his long-dead uncle, to the idea that human behavior could be studied scientifically. He saw, if dimly, the challenge of bringing to social science or biology the mathematical thinking that had been so powerful in physics.

The next branch in the maze, the choice of a college, was not much of a decision, either. The University of Chicago's New Plan for general education appealed to the generalized intellectual in him. Students at his high school had a fine record of success in its competitive scholarship exams, which he took in physics, math, and English. One of the teachers from his school always went to Chicago for the ceremony at which winners were announced, and this time somehow (momentary inattention?) she did not hear the boy's name when it was called.

The news that he had not won a scholarship crushed him. He had thought he had done very well on the examinations and was usually a good judge of his performance. Apparently, he could not compete in the league to which he had imagined belonging. He spent a week in despair, and poured out his anguish to his friend Syd Kalmbach on a long walk they took together. A few days later, he participated in a formal debate between the school's two teams, in the school auditorium. Just before the debate, the principal,

having received a wire from the University of Chicago, announced to the assembled students that the boy had won a scholarship, a full scholarship, to the University. What he felt at that moment he does not now recall.

Although I have tried to describe the boy in terms of concrete events in his life, generalizations and interpretations have crept into the account. I have an excuse for this: The boy himself was incorrigibly introspective. The generalizations and interpretations I have set down are almost all the boy's own generalizations, quite self-conscious and explicit, and arrived at during the time when these things were happening to him.

The Boy's Father

Until the boy was about a dozen years old (the term *teenager* had not yet been invented), he felt closer to his mother than to his father. She was less stern, enjoyed being hugged and kissed, and in winter wore warm fur coats and carried fur muffs that were beautifully soft when he buried his face in them. He loved his father, but also feared him a little. When discipline was needed, it was the father's role to supply it, and although the boy seldom received physical punishment or even severe scoldings, he witnessed his brother's frequent bouts with domestic authority. Although his father had resolutely turned his back on his German past, in subtle ways he was still German while his mother was American.

What was it to be German? A certain formality in manner. Sternness and a belief in discipline. Cultural breadth, with an interest in all things intellectual, artistic, and political. Professional work was important and challenging, but life was more than work. It even included skillful carpentry and other handiwork about the house, and serious gardeningtastes and talents inherited from the parental vineyard.

While there were many aspects of contemporary American life that drew the father's criticism, and even ire, he did not indulge in "in-the-old-country" comparisons to which émigrés are so often prone. If he did not much like American movies or automobiles, living on borrowed money, or the flapper style of the 1920s, it was because they conflicted, in some way, with his basic values, not because they violated his memories of Germany.

Excesses of patriotism, especially when excited by selfish motives, angered him. He could inveigh against "two hundred percent Americans," and he liked to point his finger at the veteran who rode a white horse in the Memorial Day parades, who in fact had been recruited into the navy in order to keep him on his job in Milwaukee as a skilled machinist. He never

belonged, I believe, to an ethnic organization, or a Jewish one, unless its purpose was to aid refugees.

The grammar and style of his written English was impeccable, as was essential for his patent work, and the grammar of his spoken English was nearly perfect. A couple of years after his death, his son (now a grown man) had a dream of him, in which he spoke with a distinct German accent, to the son's immense surprise. The next morning he mentioned it to his wife. "Of course he had an accent," she said, "a strong one." As a boy, his son had never noticed it.

The boy's father was an intellectual. His German engineering curriculum had a somewhat stronger mathematical and scientific foundation than American curricula of that era. Beyond that, as a young engineer, he sought to extend his scientific knowledge, studying the books of General Electric's engineering genius Charles Proteus Steinmetz and the idiosyncratic English electrician Oliver Heaviside, and a textbook on vector analysis. His main professional work was designing complex switchgear (terms like *servo-mechanism* and *control theory* had not yet come into vogue) to control mining machinery, theater lighting systems, lathes, and battleship gun turrets. Among the papers he left behind were several dozen patents granted for his inventions.

At Cutler-Hammer, he constantly urged the company to do more research as a basis for new product development, and, at about the time of World War I, he was put in charge of a newly created Research Department. The department seems to have been abolished during the deflationary crash that followed the war, whereupon he moved over to the Patent Department, where he remained. Although the boy never heard him speak about it, this history having been created from indirect evidence, the dismantling of the Research Department was evidently a traumatic episode in his life that turned his deepest interests away from Cutler-Hammer. In later years he encouraged companies with whom he consulted to set up research programs, but his advice was not often followed.

Professionalism, for him, went far beyond employment. He was very active in the local Engineers' Society and its community programs, for example, helping design the first electric street-lighting system for Milwaukee. He was active, too, in engineering education, serving in advisory capacities at Marquette University and the University of Wisconsin. He opposed vocationalism in engineering education, fearing it would make engineers no more than hired hands, and as a result his attitudes toward employers and business generally were equivocal. Whether he was a liberal or a conservative is hard to say; he would probably have been most comfortable as a Bull Moose Republican, but whether he voted for Bob La Follette for president

in 1924 is unknown. (The boy was certain that La Follettea long-time figure in Wisconsin politics, and besides, a hero of his unclewould win in 1924, because everyone in Wisconsin seemed to be for him. La Follette did in fact carry Wisconsin, but no other state. The interminable balloting at the Democratic Convention that year, transmitted by the new radio, made a great impression on the boy, who listened to it by the hour, like a new kind of baseball game.)

Finally, the boy's father was sociable, if not social. He never joined a social club in Milwaukee, other than the Professional Men's Club, a luncheon club that met weekly. Probably he was not welcome in the Aryan clubs and had no interest in the Jewish ones.

The family entertained and was entertained fairly often, and the parents went frequently to concerts and plays. Mother Edna's friends were often fellow musicians, and Arthur's, engineers or patent attorneys. Doc Stoeckel, a talented chemist who developed his own business, was his closest friend, and there were others, like the patent attorney Edwin Tower, and the owner of the Milwaukee School of Engineering, Otto Werwath. But none of these men could be described as cronies.

Early on, the boy's father had a passion for yachting, which proved incompatible, apparently, with raising a family. So his small sailboat, the Cricket, was sold before the boy was born. But poker and golf were not among the father's recreations. As his patent practice developed (his contract with Cutler-Hammer permitting him an outside practice), he spent many evenings at home in the den doing legal work. Gardening and house improvements occupied much of the rest of his leisure; the sun porch he had built onto the front of the house allowed his gardening to continue through the Wisconsin winter. And he always enjoyed a foray into the countryside.

Perhaps his most unaccountable behaviors were the Sunday rides that often replaced visits to friends and neighbors after the family acquired a Studebaker in 1928"unaccountable" because it is hard to see how this pointless roaming on crowded highways meshed with his other values. Perhaps this was his way of spending time with his wife without needing to converse. But Edna was a timid person (who succeeded in transferring many of her fears to the boy), in a state of continual tension while riding in traffic.

How did the boy's father appear to other people? Clearly, he had respect in the community, and his high moral standards were evident. His community service and character account for the honorary degree bestowed on him by Marquette, a Catholic university. By all accounts, he was universally trusted, perhaps even regarded as a bit "innocent." He was easy in his relations with people, not stand-offish or shy, but his intellect showed through a little too much for people always to be at ease with him. After

he received his honorary degree, people found it natural to address him as "Doctor." He was not quite one of the boys.

It is most unlikely that he was ever unfaithful to his wife, and most probably the thought never entered his mind. Since sex was not a topic of conversation when the boy was growing up, he had few clues about his father's views toward it. He gave the boy only one piece of advice before he went off to college. Standing on the front porch of the house as a dog ambled down the street, his father said suddenly, "Don't feel that you have to be like one of those, and chase every bitch you see." The boy was too embarrassed to reply. His father had a repertory of anecdotes, but none of dirty jokes, and he had never before used such language in front of the boy.

Arthur was very protective of his wife, who was more than a little neurotic (especially when conjoined with her mother, grandmother Merkel). In the 1920s there was a period of strained relations between Arthur and Edna, caused by the temporary residence in their home of an egotistical young man from Germany, the son of one of Arthur's college friends, who was doing his apprenticeship in Milwaukee. Edna thought (probably correctly) that he was trying to demean her in Arthur's eyes, and there were stormy scenes and threats of suicide. Except for this interval, the marriage was serene, and Arthur and Edna behaved like people who loved each other deeply.

In 1936, a diagnosis of cancer followed by colostomy made Edna a permanent semi-invalid, staying at various times in a sanitarium. She was continually worried about the prospects of social embarrassment from her infirmity, but actually managed quite well with it and traveled extensively by herself for years after Arthur's death. During the twelve years from the onset of her illness to his own death, Arthur never complained, and was wholly supportive, often urging his two sons to be patient with their mother. He was Spartan in accepting a burden that she bore less quietly. On the few occasions when he was mildly critical of her in conversations with his sons, he attributed her deficiencies to her mother's influence.

This was, of course, a generation that had not heard of feminismor had heard of it only in its 1920s guise. Conversations at social affairs separated men and women, and Arthur probably never expected, or even dreamed of, intellectual companionship with his wife. Edna was a good homemaker; they raised two boys together. They enjoyed concerts and plays. There were all sorts of things to do at home or elsewhere that didn't call for much conversation. At the dinner table, two conversations could always go on at onceone between father and sons, the other between mother and grandmother. Certainly the boy's father had little patience with the gossip that formed much of the women's conversational stock.

Was the father lonely? There is no way to answer that question. His generation (and the boy's) did not wear hearts on sleeves. They did not search, at least openly, for their "identities." They did not demand "self-actualization." In the privacy of their diaries, some people expressed these needs. Stendhal's self-questionings, echoing nearly two hundred years down the corridors, sound wholly contemporary. But Arthur kept no diary. He went his equable way (especially in later years), kept his financial and other worries to himself, died instantly in 1948, at the age of sixty-seven, while seated in his office chair conversing with a friendand probably led at least as happy a life as the great majority of mankind.

Did he have financial worries? Just before the Great Depression, his salary rose as high as $7,500a good upper-middle-class income, approximating at least $75,000 in 1991 dollars. But when the Depression hit, he came very close to losing his Cutler-Hammer job (after thirty years' service). Only the intercession of influential friends persuaded the company to retain him, at a much-reduced salary.

The boy was more than a little innocent of these matters. When he won his scholarship ($300 a year), the University of Chicago suggested that he decline it if he did not need it; the boy thought he should decline and was a little embarrassed that his father insisted that he accept it. He wasn't aware that his father was putting two sons through college on less than $4,000 a year. (Similarly, I had no reservation about asking help to buy a house in 1948, and was shocked, after my father's death later that year, to learn his true financial status and his worries about being able to support himself and his wife in retirement. Fortunately, he carried a good deal of insurance, and his estate was sufficient to permit my mother to live comfortably and independently after his death.)

He was a very private man. I hope he was mostly a happy man; I know he was an uncommonly good one.

I have painted my father as an admirable person. As I matured, my respect and love for him deepened, while I became more distant from my mother. Today, I think often of him, less often of her. My values today are hard to distinguish from my father's values as he expressed and lived them while I was growing up. My brother, too, for all the conflict during his school days, became more and more like my father in his adult years. Although his interests were narrower and he was less genial as a person, he held similar basic valueseven a love of fishing and carpentry, which I never acquired, and an unmovable honesty, which I hope I have also.

Chapter 2
Forests and Fields

In the first third of this century, Wisconsin still had a frontier, not of farms or range land but of forests: the North Woods. Lumbering was the major industry of the northern part of the state, and there were still substantial stands of virgin pine and much larger areas of second-growth birch and aspen that supported a paper industry. On the southern fringe of the forest, the north-central portion of the state, much of the cleared land had been sold for farms, where poor, mainly Polish, immigrants scratched out a living raising potatoes and milking dairy cows.

Since the boy's father was an enthusiastic fisherman and a lover of the outdoors, the family spent a number of their summer vacations in the North Woods, living in rented cabins on the shores of one or another of the beautiful, isolated, deep lakes of the area. His father's paid vacations lasted exactly two weeks. The boy looked forward to these tripsthe early ones by train (the first trip was in 1925), later ones by automobileespecially to the clear lakes, the dark surrounding forests, and the possibility, by a ten-minute walk or a short pull at the oars of a rowboat, of achieving complete isolation from the rest of mankind.

The small boy treasured isolation during the day, but not at night, when the forest could be not only lonely but also menacing. It was inhabited by wolves, whose grim howls often broke the peace of the cold nights, and by bears. Of course, the wolves stayed well clear of human invaders, and the peaceable black bears were seen only rarely. But the boy had a good imagination, easily stimulated by sudden noises in the woods, especially at night.

The boy learned to fish, but never seriously. Probably patience was lacking, for he did enjoy gathering in the easily caught pan fishbass, perch, and sunfishthat could always supply a dinner. The more elusive game fish, the pike and pickerel, the trout and muskies, that required patience

and craft, he left mostly to his father. He was glad enough, however, to row the boat when his father trolled. That did not interrupt one's thoughts and enjoyment of the scene. And getting to the day's fishing spot often meant rowing to the next, even more remote, lake by way of a stream that had been partially blocked overnight by the persistent dam-building beavers.

In a rowboat one could stalk the loons that would dive deep as the boat approached and reappear in a few minutes a quarter-mile away. Their mad, raucous cries, less menacing but as lonely as the wolves' howls, echoed from the lake in early morning and at evening.

On the cool days of late summer, whitecaps advanced across the deep blue lake, driven by a sharp wind that twanged the ropes on the tall flagpole near the dock and rocked the moored rowboats in cadence with the twanging. It, too, was a lonely sound, warning of the coming winter when this whole scene would be deserted.

Hiking Trails and Canoes

What the North Woods cultivated in the small boy was a general love of the outdoors, and especially of wilderness. He treasured Ernest Thompson Seton's *Two Little Savages,* which provided him with an exciting tale of out-of-doors adventure and a compendium of camping lore, and helped tide him over during the city-bound winters. When he joined the Boy Scouts, at twelve, he quickly acquired the skills needed for hiking and camping, and his knowledge of plants, birds, animals, and insects grew. Soon, he was camping and backpacking with his close friend Syd Kalmbach, and often with a third boyLewie Wrangell or George Johnston. One trip took them to Sheboygan; another to Door County, where he visited Washington Island for the second and last time, hitching a ride on the combined mailboat and fishing trawler. The island was filled with Syd's Icelandic relatives, so the boys were well received. West Harbor had completely silted in and the resort was gone.

A more ambitious hike began at the Wisconsin Dells, headed down through the Baraboo Range to Devil's Lake, then turned west to Mount Horeb and the Blue Mounds. (These names will mean nothing to most readers; I mention them for the sheer pleasure of hearing them once again, and seeing the scenes before my eyes: canoeing through the Dellsthereby providing local color for the tourists in the excursion steamers; hiking on a hot, dusty gravel road bordering a tobacco field on the sandy plain north of Baraboo; climbing across the tumble of great granite blocks on the west shore of Devil's Lake; discovering just below the Blue Mounds a family of

wary hillbillies, only a track leading up to their rough, lamp-lighted cabin.)

The boy also took a lengthy canoe trip with Syd that started near the headwaters of the Rock River, at West Bend, and continued down through Lake Koshkonong and the great marshes surrounding it, almost to the Illinois border.

These were not wilderness trips; we traversed mainly farming country. Because backpacking was not a common sport in the Midwest before World War II (and perhaps not even now), the boys were objects of some curiosity, the curious sometimes including the local sheriff or constable. The hikers always managed to convince people that they were not vagrants and to find comfortable campsites in woods or pastures. When offered the hospitality of the Mount Horeb jail for sleeping quarters, they politely declined.

At some early time, and out of some undefined impulse, the boy acquired a strong affinity for the numerous tamarack bogs that dot the glaciated terrain of southern Wisconsin. They, along with the Wisconsin lakes, are the products of the most recent glaciation, which has not left time to develop drainage channels for all the rocky depressions it produced. The shallowest of these depressions gradually accumulated great beds of peat and a covering forest of tamarack trees with a dense undergrowth of plants of every variety that would flourish in that wet, acid soilladyslippers and Jack-in-the-Pulpits and other rare flowersas well as a splendid display of insects, including, unfortunately, an unextinguishable supply of vigorous Wisconsin mosquitoes.

But it was worth fighting off the angry mosquitoes. Forcing your way into the borders of the marsh, you were almost instantly surrounded by a lush dark jungle, in every shade of green, pierced here and there by pencils of bright sunlight that were intercepted at every moment by darting dragonflies. If mountaintops were Arctic wastes, tamarack bogs were tropical forests. The dense growth around you took you wholly out of your familiar world.

The boy spent many hours in the bogs, collecting plants and insects, and just enjoying their beauty and the sense of his own immersion in it. Complete insulation from the world had a special meaning for himand for the man he grew into. The fascination with isolation carried into his reading preferences. *Green Mansions,* by William Henry Hudson, was a favorite story, and even more, Hudson's tales of his life on the Argentine pampas, recorded in *Far Away and Long Ago*. Admiral Byrd's story, in *Alone,* of his three months of solitude in the Antarctic was another favorite. In dark moods the boy resonated also with Ole Rölvaag's *Giants in the Earth,* the story of loneliness and isolation among the Norwegian settlers of South Dakota.

Wilderness and Mountains

By the time the boy experienced the genuine wilderness, he was already a young man, that is to say, he had become me. In 1936, Syd and I took a wilderness trip by canoe down the Flambeau from Park Falls to Ladysmith, shooting all the rapids but one. On one dark rainy morning, after a soggy night in which we had suffered several minor disasters, including breaking the eggs that were bundled in our mosquito netting, we went to the riverbank to scout the rapids that lay just below us. There we found a large granite rock with a brass plate affixed to it, memorializing the canoeists who had drowned there a year or two before. We portaged our canoe around those rapids.

My other principal wilderness experiences came later, in the Sierra Nevada. In 1941, the summer before the U.S. declaration of war, Dorothea and I hiked with Dan and Louise Arnon and two mules into Evolution Valley and up into Evolution Basin. The calendar indicates that our daughter Katherine was conceived there, an excellent place to start a life. In Evolution Basin, nestling beneath the Darwin Range, I found myself for the first time not only above timberline but completely surrounded by mountains and cut off from the lowland world. It is the nearest I shall come to visiting the moon.

In 1963, we had a second Sierra trip with a somewhat larger group. And I have found myself atop other mountains (reachable without rock climbing), mainly in Colorado. I remember especially a very cold, foggy morning with my son, Pete, atop Mount Audubon, huddling behind some rocks to shield us from the wind until the fog was blown off sufficiently to let us view the surrounding peaks and valleys.

Mountain peaks make the spirit leap. Leaving a peak, losing foot by foot the altitude that has been gained so strenuously, is always sad and even a little depressing. I take each step reluctantly, almost resentfully.

My most recent mountain experience was in the Alps. Finding that I would be in Geneva on my sixth-fifth birthday, I decided to spend the next week in the Valais, taking the train to Sion and a bus to Arolla in the Val d'Herrens, as my starting point. I no longer aspired to wilderness backpacking, with a forty-pound pack, and I reasoned that the Alps, where I could stay overnight in an inn and need not carry food or bedding, would be less strenuous.

I omitted only one crucial fact from my calculations. Once in the high country of the Sierra, you can camp at 10,000 feet, and climb only an additional thousand feet to cross the next pass. In the Alps, where the highest

inns are typically at 7,000 feet, you awake in the morning with the prospect of an all-morning climb to the 10,000-foot pass. When this dawned on me, and when I saw that there was still deep snow in the high passes, I revised my itinerary, but spent a delightful solitary week in the Val d'Herrens and the high country surrounding it.

Rockmarsh*

To understand Rockmarsh, you must keep in mind the bookish life I have led. I have sailed through the Strait of Magellanin a book. I have climbed Nanga Parbatin a book. I have swum the Hellespont, with the popular traveler-writer Richard Halliburton. I have been terrified by the cinematic dinosaurs who inhabited the Lost World; I have tilted at windmills with Don Quixote; and I have bound myself to the mast with Odysseus.

After adventures like these, any that occurred to me outside my study or the movie theater have been tame indeed. To be sure, I reached mainland China in 1972 shortly after the Nixon visit. And, aside from the hiking and canoeing already mentioned, I spent my sixty-seventh birthday with Dorothea and friends at Qifu, the birthplace of Confucius, and, in Greece, explored with Dorothea the Spring of Sybaris at Delphi and the Corycian Cave, sacred to Pan, on Mount Parnassus, where we ate a picnic lunch. In India, we flew in an elderly plane across the Western Ghats into Aurangabad to see, during the festival of Holi, the caves at Ellora and Ajanta. We drove the unpaved mountain roads of Montenegro to find Durmitor, the wartime hideout of Tito, and listened to a contest of Yugoslav troubadours at Zabljak, a nearby market town. We crossed the southern Andes in a torrential rainstorm on the mountain route from San Carlos de Bariloche to Porto Montt, and spent Christmas Eve 1970 at the border inn of Peulla amid German Christmas carols,** the only guests apart from one couple from Lima. New Year's Eve that same year we spent in the public square of Cuzco, and the next day and night among the magnificent ruins of Machu Picchu. In June 1989, I spent three days in Beijing just after the tragedy at Tiananmen.

I could go on almost endlessly with this list of mild adventures and, had I been a photographer (I almost never carry a camera on my travels), could

* The experience I am about to relate, together with experiences at the University of Chicago, all occurring in the period from 1932 to 1937, transformed the boy into a young man, and then into an economically Independent married adult. Since I can no longer distinguish his identity from mine, I will continue the tale in the first person.

** Much of western Argentina and southern Chile was settled by German immigrants.

undoubtedly have prepared myself for a second career as a traveloguer. None of it sounds like a best-seller; none of it, in our world of seasoned travelers, would keep readers on the edges of their chairs, or perhaps even awake.

The real adventure of my life was Rockmarsh. It was real to me, perhaps surreal, for the same reason that Don Quixote was real to Cervantes—more real than the Battle of Lepanto that cost him an arm. It was real because it existed in the imagination. Do not misunderstand me: Rockmarsh does exist in the everyday world; so did Maurice Davis, who imagined it, and Hank and George and Shockey, who toiled in it. You can find it on a Wisconsin map, and I can give you directions to get there by car.

The story of Rockmarsh must begin with a geology lesson, because that is part of the imagined scene. A portrait tells the truth only if it reveals the bones under the flesh. The foundation of the state of Wisconsin is formed of ancient Laurentian granite. Above it (except where the granite is exposed, in the northern part of the state) lie alternating layers of Cambrian and Ordovician sandstone and limestone. And above these, a long, calcified, curved spinal column of Silurian limestone runs from eastern Wisconsin up and around the Great Lakes, accounting for many of their familiar features. Its sharp outer edge is called the Niagara Escarpment, and it first makes its appearance above the glacial debris just south of the entrance to the Door County peninsula, the "thumb" of Wisconsin that separates Green Bay from Lake Michigan. It is a great ridge, running north and east through Door County to provide Green Bay with its steep eastern shore.

Temporarily submerged beyond the north end of Door County and Washington Island, the ridge reappears as part of the Lake Michigan coastline of the northern Michigan peninsula, then swings eastward near the junction of Lakes Superior, Michigan, and Huron. The Niagara limestone then turns southeast, forming the islands and peninsula that divide Lake Huron from Georgian Bay. Continuing across the tip of Ontario, it turns east near Hamilton, intersecting the Niagara River on its northward flow from Lake Erie to Lake Ontario to form the great falls. Still continuing eastward, it defines the south shore of Lake Ontario and finally disappears again somewhere near the western edge of the Adirondacks.

In Wisconsin, a lobe of the glaciers scoured out Green Bay to the west of the hard ridge of the Niagara Escarpment; another lobe excavated the basin of Lake Michigan to its east. The interglacial moraine, a rugged hilly ridge of glacial gravel, follows the line of the escarpment after the limestone has disappeared below the surface south of Green Bay. The east branch of the Rock River drains the west slope of that moraine, running mainly west through the little town of Theresa, and continuing until it joins the west

branch of the Rock near Horicon, whereupon the waters continue south and west to flow into the Mississippi at Rock Island, Illinois.

Thus the great ridge of the Niagara limestone impresses its form on the entire landscape from east central Wisconsin to the east end of Lake Ontario in central New York State. Throughout almost all its course, it is clearly visible from the air. It is the spinal cord of my native land.

The upper courses of the Rock River, both east and west branches, are marshland: great bogs of peat that forms in many places a layer twenty or more feet thick above the glacial gravels. At the head of the west branch of the river is the famous Horicon marsh, for more than fifty years reserved for hunters (during the season) and game birds (out of season), both of whom flock there in enormous numbers. The marsh at the head of the east branch, which was to become Rockmarsh, was smaller and less well known, although it attracted many local hunters, too.

Rockmarsh, originally covered with a heavy growth of the swamp-loving tamarack, became part of a land grant to the Soo Line Railroad. At some time, the timber was harvested over most of the marsh and the land fell idle, its ownership residing in the estate of a lumbering family. It extended over some 3,000 acres, nearly 5 square miles, most of it now open peat land, marshy, and largely flooded each spring, rimmed almost solidly by a dense border of willow and alder brush. The two-thousand-acre portion of it owned by the estate was the site of my adventure. For me, it was a sacred place that focused in one spot the great forces that had made the land: the Niagara limestone, the glaciers, the ancient granite Laurentian Shield underlying all.

The adventure took place mostly during the summers from 1932 through 1936. It began when Maurice Davis arrived in Milwaukee in the depths of the Great Depression, probably in 1931 or 1932. Davis was a man in his late thirties or early forties, slight of build, rather dapper, balding. I know only a little about his origins; perhaps my father and his friends knew more. He had been an officer in World War I, and had been shell-shocked. After the war, he had spent some years, in an undefined capacity, around several state agricultural experiment stations. There he had made the acquaintance of a tall marsh pasture grass, reed canary grass (*Phalaris arundinacia*), and had built a careful plan around that grass.

The plan was to purchase a large tract of marshland not too far from Chicago, plant it in reed canary grass, purchase yearling beef cattle, grow them, and ship them to Chicago for resale and finishing. The cheapness of the land and its proximity to the marketsaving the cost of transporting the cattle from the Great Plains and avoiding the danger of their losing weight while being moved to market over long distanceswould make the

scheme highly profitable. The experiment stations had had great success in pasturing dairy cattle on reed canary grass.

Davis came to Milwaukee with this scheme, which he proposed to a group of engineers, my father among them. If you scratch an engineer, you will find a physiocrat beneath. Engineers believe in real things like machines and bridges and land. They are less confident that intangibles like money and organizations really exist, and the Great Depression enforced their skepticism. The Depression had hit Milwaukee very hard, and, as I mentioned, there was a time in 1932 when it appeared that my father, after thirty years at Cutler-Hammer, might lose his job.

Everyone had a different diagnosis of the illness that was the Depression, and how it could be remedied. In my father's den at that time sat a large chart depicting a hydraulic model of the economy, fashioned by an engineer-economist named Dahlberg. Flows of money were red; flows of goods, green; labor flows, blue. Since Keynes's General Theory had not yet been published, the model was undoubtedly classical rather than Keynesian, but I have no recollection of the arrangement of the valves that controlled the flows. There was an engineers' discussion group associated with that chart, and it was probably that group to which Davis was introduced by an engineer-manufacturer named Walter Ferris.

The engineers (augmented by a couple of physicians) agreed to invest a portion of their modest savings in Davis's scheme. If worst came to worst, they would grow their own food on the land. That idea appealed to my father's rural boyhood and gardening propensities. A search for an appropriate site revealed Rockmarsh, 2,000 acres, 3 square miles, in the hands of the estate, lumbered but never plowed. It was just 40 miles northwest of Milwaukee (U.S. 41 to Fond du Lac now crosses the southeast corner of it).

Rockmarsh was purchased for $2 an acre. (This was, after all, the Depression, and the land had no use but to pasturewithout permissionthe neighboring farmers' young cattle.) I became Davis's right-hand man, or boy, in getting the venture under way. We walked the boundaries of the entire tract, often cutting through the dense willow and alder brush with brushhooks while fending off the plentiful Wisconsin mosquitoes. We broke down the crude stone dam that duck hunters had thrown across the river to flood the upper part of the marsh.

In the main part of the marsh, the glacial gravels were covered by at least 20 feet of peat, but near the dam we discovered a gravel island, three or four feet above the level of the peat and immune to spring flooding. It was located, however, about a half-mile from the solid upland, hence sometimes inaccessible in spring. In preparation for our agricultural activities, I made

a careful ecological survey of the marsh, identifying the main plant species and marking out the areas where difference in plant cover seemed to indicate differences in soil or drainage. I even did some laboratory studies, complete with Petri dishes, to cultivate the soil bacteria and find out something about the available nutriments. Since I did not understand enough botany, much less chemistry, to know what I was doing, I discovered nothing of value.

Meanwhile we had recruited help. While introducing ourselves to the neighbors who owned the hillside farms surrounding the marsh (and who were a little unhappy, though not sullen, about losing their free marsh pasture), we met Hank Sauder. About eighteen and unemployed, he was living with his sister and her husband, earning his keep by working on their farm. We hired Hank (who may never have worked for a cash wage before), and he brought in his brother, George, who brought his friend Shockey. George and Shockey were in their mid-twenties, also unemployed. They had tried their hands at just about everything, in farm and factory, and if there was anything agricultural or mechanical they did not know how to do, they would not admit it. Shockey also claimed experience as a cook. He was the flamboyant one, as extroverted as a Texan, with a round Clark Gable face; George was homely, quiet and steady, full of intelligence and good common sense. Hank resembled his brother, but was more outspoken and probably brighter.

Davis was manic-depressive, a condition that may have been produced by his wartime experiences. In his manic moods, he would buy everything in sight that he thought was a bargain and possibly useful at the marsh. He could not leave a Sears, Roebuck store with less than a truckload of booty. Among his treasures was a large quantity of second-hand corrugated iron sheets that ripped gashes in our hands as we used them to build houses and sheds and barns in the following years. Another "bargain" was a team of horses whose teeth had been so worn, chewing South Dakota grit during the drought years, that they could not eat the lush pasture at the marsh and literally starved to death. They were replaced by a team that turned out to be afflicted with the heaves. We were not a very good match for the local horse traders, who were everything that horse traders (the ancestors of second-hand car salesmen) were supposed to be.

Soon we had driven a 20-foot artesian well (using a sandpoint and sledgehammer) on the gravel island. We built a one-room house, measuring about 14 by 16 feet, around the well (running water!). In half of the house, bunks were installed, in the other half, a sink, stove, and large table. A large coal-burning stove sat along the middle of the wall opposite the door. The galvanized iron roof and walls were caulked and, after a year or so, lined with beaverboard. The house was lighted by a gasoline mantle lamp, and,

needless to say, there was neither electricity nor indoor plumbing (except for the water). There the three boys (joined later by their father) lived year-round, and there I lived with them through most of the summers and for briefer periods in winter when I could get away from school and catch the Soo Line train up to Theresa Junction, where it arrived at 4:00 A.M. in the winter blackness.

Two or three years later, we bought Murphy's upland (or, more accurately, hillside) farm of some fifty acres, adjacent to the main body of the marsh. There we erected a more substantial house and barn that was not marooned annually by the spring flood. It was much less romantic and considerably more comfortable than the shack on the gravel island.

When the land we had purchased was officially surveyed, I served as lineman for the county surveyor. He had learned his business from his father, who was trained, in turn, in England. The surveyor operated mainly by locating old trees mentioned in his inherited logbooks, or by finding old boundary stones (when farmers hadn't borrowed them to prop up their barns). With the others we had hired, I cleared stumps from forty acres of land and pitched hay up onto the high wagons (no hay loaders there). I learned to drive the Model T truck and the caterpillar tractor and to plow (a rifle on the seat beside me, in case a pheasant appeared on the scene). In the summer of 1935, I went on a half-time schedule of farm work for a time in order to spend the afternoons studying calculus in the hayloft of the barn.

Some evenings, especially Saturday evenings, we would all pile into the truck to go into Theresa, a town of two hundred inhabitants, or perhaps as far as Kewaskum, with one thousand people. Hank, George, and Shockey (who were always "the boys") liked old-time dances, that is, German folk dancing, and I was soon agile, if not graceful, in the *hop schottisch*. We danced with the local girls and made passes at them, drank far too much (mostly boilermakersschnapps followed by beer), and then carefully steered the truck home at a wobbly walking pace, trying to stay out of the roadside ditch.

The population of the surrounding farms was mostly second-generation German, but some of the old immigrants were still alive. There was also an enclave of Irish, and some intermarriage, as both groups were almost all Catholic. Occasionally at the Saturday dances, the ethnic lines showed through. I recall one evening when a young Byrnes heard or imagined an obscene remark directed at his sister by a Schneider, at which point a number of young men went home for their guns. Mediators interceded before there was any bloodshed. Generally speaking, however, there was cordiality among neighbors and an absence of long-standing feuds.

When I first went to the marsh, I made a resolution. I was going to watch my P's and Q's in my relations with the boys. I was not going to acquire the sort of wise-guy reputation I had had with my friends in Milwaukee. It was just as well that I adopted this strategy, because the boys, older and bigger than I, would have been unimpressed by the wise guy. Instead, I moved quite naturally into a younger-brother relation with them, learned enormously from them, lived down my rich-guy, city-slicker origins, and felt myself liked and respected. Such interpersonal skills and understanding of people outside academe and the professions as I possess owe more to Rockmarsh than to any other single experience I have had.

It was a wonderful imaginary adventure: the frontier restored in the twentieth century. The boys did not quite believe in its reality, either, but it kept them warm (more or less) and fed until jobs would be available again. Besides, they entered into the spirit of it with enthusiasm, taking great pleasure in playing cowboy, even in wearing six-shooters and Texas hats when they rode on horseback around the marsh. There was an outside chance that Rockmarsh would become a large and successful operation, and they its managers. Hank, by priority of hiring, remained the official boss, though the youngest. George was elder statesman, and Shockey displayed his talents as cook and extrovert.

The neighbors, of course, didn't know what to make of it. When we first began plowing and brought in a huge cylindrical boiler, which we filled with water and used as a roller, they came from miles around on Sundays to see what the city slickers were up to. Their skepticism was frank (and well justified, as it turned out), but they were good-humored about it and were fine, friendly neighbors who accepted us as part of the community.

I won't recount the whole long, and ultimately sad, history of Rockmarsh. Not long after we started, Davis, in one of his depressed periods, registered at a hotel in Minneapolis, put a revolver to his head, and pressed the trigger. That was far more shocking to me than any of the deaths in my family: my grandmother, uncle, or grandfather. The news was brought to me in my high school trigonometry classroom; I can see the room before me now, the teacher, Mr. Kingsbury, and my fellow students, at the moment I received the message.

Davis's death took much of the spirit and initiative out of the venture. None of the shareholders had much time to devote to it. There were other setbacks, too. A fire destroyed a portion of one year's harvest of grass seed, then highly marketable. An epidemic of pinkeye spread among our cattle, who became warier, more ornery, and leaner each day as we rounded them up to put stinging drops of medicine in their eyes. In fact, what really put an end to it, finally, was a decision the cattle made. We had sowed 600

acres of grass and stocked it with Herefords. They would have none of it. They would eat anything but reed canary grass, and broke down every fence we constructed, including barbed-wire and electric, to get out of the pasture.

In essence our failure was a vivid demonstration, which I have never forgotten, that theories, however plausible and "obviously" valid, can be destroyed totally by the obstinate facts of the real world. Davis had brought us an unbeatable scheme for raising cattle profitably. The cattle had a different scheme. No doubt my later deep skepticism of the *a priorism* of mainstream economics had some of its origins in this experience.

I need not expand further on what the marsh meant to me, not only intellectually but emotionally. In chapter 13 I will claim December 15, 1955, as the climactic day in my life, but perhaps that is wrong. Perhaps it came during a week in 1932, while I was making the ecological survey, living completely alone in the shack on the gravel island, wholly surrounded by the lush acres of grass and the border of brush, talking to no one but the birds and insects, and seeing no one except an occasional farmer plowing on the hill two miles away. Far off to the north, across the great expanse of grass, I could see the little spire of the church at Lomira. It was my nearest approach to a mystical experience, precious and indescribable.

Or perhaps it was the cold moonlit winter night, a couple of years later, when Harold Guetzkow and I skied to Rockmarsh from the Theresa Junction depot, packs on our backs, the stars shining, and the snow deep. It was long past midnight and the sharp clear air was totally still except for the occasional distant sad hoot of the train we had left, as it proceeded to the north.

For more than fifty years I've carried in my wallet a snapshot of the marsh. The land is now, along with the Horicon, a state-owned preserve for game birds, the river dammed near the Soo Line tracks at Theresa Junction, and much of the lush open pasture covered with water. Hank, when I last saw him, perhaps twenty years ago, was prospering as a farmer near Kewaskum. I don't know what has become of George or Shockey, but I think of them often.

Chapter 3
Education in Chicago

The Milwaukee I grew up in was hardly a backwater, but neither was it an avant-garde center of the arts or of intellectual adventure. A solid manufacturing city, it had a long history of good local government in the social democratic tradition of the German '48ers who had helped found it; and it was embedded in a state known for its progressive social legislation.

My schools were excellent, but their excellence lay in providing solid classical foundations rather than in exposing students to novelty. The music we heard was Beethoven rather than Stravinsky; the paintings we saw were Millet rather than Picasso, or even Cézanne. I had never read a word of Marcel Proust or of T. S. Eliot, nor heard their names, when I left Milwaukee.

Even in the depths of the Depression, with unemployment in Milwaukee devastatingly high, disenchantment with existing social institutions seldom expressed itself in strident or violent Marxism. *Socialism* was not a fright word, as in neighboring Chicago, but was associated with the liberal, reformist social democracy of western Europe. Perhaps for this reason, political debate, at least in the circles I knew, was carried on in calm and reasonable voices. In depression as in prosperity, Milwaukee taught its children to expect and work for progress in human affairs, and did not immunize them against new ideas. But change, in the spirit of the '48ers, was evolutionary, not revolutionary.

When I arrived at the University of Chicago in 1933, it was all the thingsartistically, intellectually, and politicallythat Milwaukee was not. Nothing was too new, too arcane, or too absurd to excite passionately the bright minds of the students and faculty assembled on the Midway campus. Everything had to be explored, tested, before it could be accepted or rejected.

Life As a Freshman

From my brother's accounts of his experiences at the University of Wisconsin in Madison, I had expectations of what a university was like. I knew about the importance of sports and the inevitability of freshman hazing, and, though neither of these activities filled me with enthusiasm, I was prepared to accept them as part of the bargain.

Shipping a small trunk ahead of me, and carrying two large bags, I boarded the interurban North Shore electric train in September 1933, to undergo the initiation of Freshman Week. En route, I struck up a conversation with another young man, similarly encumbered, and we found that we were heading for the same destination. The young man, Harold Guetzkow, became and remains my close friend. Together with a third passenger, Norman Pearson, who boarded at Waukegan and was recognized as a fellow pilgrim, we transferred to the Chicago El, which ran on the same tracks as the North Shore, disembarked at 63rd Street and Greenwood, and lugged our heavy bags to the Burton-Judson dormitory of the university, three blocks away.

As we approached the dormitory door, a man in a tweed jacket (a graduate student, appearing *very* mature) came out, introduced himself, and asked, "Wouldn't you gentlemen like a spot of tea before you carry your bags upstairs?" A little stagey, perhaps, and conspicuously Oxbridge, but it immediately and radically reconstructed all my expectations. Hazing and sports were forgotten; I had found a genuine university.

Because the dormitories were quite new, and because the Depression had cut undergraduate enrollment, all the rooms were singles, and the university had rented many of them to graduate students. To the best of my recollection, there were eight of us who inhabited the fourth floor of our wing of the dorm: a student of Sanskrit (who hoped that one of the two or three chairs in that subject in this country would be opened by retirement or death before he received his degree); a Latinist writing a dissertation on Catullus; a medical student (who had good access to pure laboratory alcohol, which we cut with water and grapefruit juice and drank); a graduate student of English literature (very avant-garde); an undergraduate studying ancient Middle Eastern languages; a law student; a freshman chemist; and myself. The rest of Burton-Judson dormitory had a similar variety of residents.

It was the best possible environment for growing up—no artificial generation gap. The graduate students treated us, if not always as equals, at least as sentient beings. They tolerated our ignorance and corrected it when

they could. "Role model" is what we now call it, but I don't think the phrase existed then.

From time to time there were others who also instructed us, but who were not exactly role models. One was an older fellow (in his thirties?) who held a captaincy in the army reserve. In addition to his university studies, he was taking army correspondence courses to advance to the next rank, and since the lessons did not interest him much, he sometimes let me do them for him. I had great fun moving divisions across South Mountain for a campaign near Gettysburg. Regrettably, the Captain also turned out to be a kleptomaniac, and had to move out.

An older man moved in from Kansas City, a villainous-looking lean fellow with a great scar across his throatthe gash that caused it must have been nearly fatal. The story was that he had been sent by the Missouri state government (that is, the corrupt Tom Prendergast machine in Kansas City) to study forestry, or some such unlikely subject. More likely, he had been sent out of Kansas City until whatever scrape he was in had quieted down. In any event, he holed up in his room and established a floating blackjack game, with an adequate supply of liquor. I expect he earned his keep that way, but I was never tempted to join the game.

I do not mean to give the impression that the colorful aspects of life at Chicago overshadowed our studies. We lived in an adult environment, and adulthood meant serious work. By watching the graduate students, in particular, we could see what it was to be an educated person. The English Lit student (his name mercifully escapes me) never ceased to taunt me about how conventional I was, especially with respect to my musical, artistic, and literary tastes. He assured me that I would never in my whole life do anything creative.

In self-defense, I had to spend time listening to Stravinsky, and looking at Picasso, and reading James Joyce until, of course, I came to enjoy them all. I expect that would have happened even without the goading, but we did not run a controlled experiment. In the environment at the University of Chicago, exposure to everything new and modern was massive.

Part of that exposure was arranged by the university, through the distinguished visitors it brought to the campus. The English mathematician and philosopher Alfred North Whitehead was the first of these that year. I repaired to Mandel Hall, the first row of the balcony, to hear his public lecture. I listened intently for an hour, and didn't understand a word. I attributed my difficulty, modestly, to my lack of education, but after reading several of Whitehead's books in later years, I now wonder whether that was a wholly correct assessment.

Several years later it was the turn of Jacques Maritain, the great French

Catholic philosopher. He gave his lecture in English, with which he undoubtedly was thoroughly familiar in its written form. But evidently he had had little experience in speaking English, and perhaps had even asked someone to mark his manuscript phonetically. In any event, he pronounced everything with perfect French intonation and elision, pausing in the middle of words and phrases. Again, I didn't understand a word, but clearly the reason for my failure was different this time.

Many other public lectures were far more rewarding, but somehow these two occasions stick in my memory.

Undergraduate Studies

My freshman year at the University of Chicago was the third year of its New Plan.* Most requirements for the Bachelor's degree could be satisfied by taking comprehensive examinations; all students were required to pass exams covering the major fields of knowledge: humanities, social sciences, physical sciences, and biological sciences; and class attendance was not required. These specifications were exactly to my taste, enabling me to earn my Bachelor's degree in three years, in 1936.

I attended very few classes. Since my excellent high school training had nearly prepared me for the examinations of the first two years, I was soon auditing upper division and graduate courses. Ralph Gerard taught, socratically, a stimulating freshman biology course. Although I was still deeply interested in biology, my-color-blindness and awkwardness in the laboratory made me decide against following up that interest professionally.

On the other hand, Henry Simons, on price theory, teetering on two legs of his chair, gave me a glimpse of the applications of rigor and mathematics to economics. I resolved to major in economics, until I learned that it required an accounting course. I switched to political science, which had no such requirement. (A strange beginning for someone who was later to be a founding father of a business school and a Nobel Laureate in economics.) As a result of that rather casual decision (a genuine fork in the maze), I did a great deal of work in both political science and economics. The price theory course, following my extensive high school reading of economics books, prepared me well for graduate courses (and even research) in neoclassical economics.

* For accuracy, I should specify: The *Old* New Plan had been developed before President Robert M. Hutchins arrived on campus, and was instituted in 1931. Hutchins gradually converted this into his *New* New Plan, which focused on the Great Books and an accelerated (two-year) Bachelor's degree. Fortunately, it was not instituted until I had completed my studies.

Early in my second year, I terminated my formal education in mathematics when a calculus professor insisted that I attend class. From then on, almost all my knowledge of mathematics was self-taught, some of it while I was at the university, but continuing fairly intensively until the early 1950s, carrying through most of the subjects in a doctoral curriculum of that time (lots of higher algebra, analysis, and function theory; little topology). Self-instruction gave me the courage and skill to master new areas of mathematics whenever I needed them for my research. It also left me with mathematical skills that are more rough-and-ready than polished.

The same strategy of self-instruction worked well for languages. In high school I had had a thorough two years of German from the redoubtable Fräulein Ruschaupt, which gave me a reasonable speaking and reading knowledge of that language. I had had four years of Latin, which gave me considerable knowledge of classical history and civilization but no ability whatsoever to read Latin for pleasure: We had spent all of our time translating and fussing over grammar, instead of reading.

In the first year at Chicago, I enrolled in a French class, which I almost never attended. We were given some simple reading books that introduced new vocabulary gradually, as children's primers do. I found that I could read them with little attention to formal grammar, and almost no reference to the dictionary. Then I progressed to serious French books in political science, Rousseau and Montesquieu, and found that, though they were more difficult, I could understand them. Since then, I have never ceased reading French fluently for pleasure, with only that single course, which I didn't even attend, as foundation. (I received a B for the course, my oral examination not being impressive.)

From French, I went on to other languages, studying them independently in exactly the same way, so that today, I can read professional books and papers in more than twenty languages, and can read literature for pleasure in half a dozen.

Writing and speaking are another mattertwo other matters, in fact. One can learn to read a language fluently only by spending many hours reading; one can learn to speak only by spending many hours speaking and listening. Unfortunately, most Americans have little opportunity (compared, for example, with most Europeans) to speak another language. The fact that English has become the language of international communication aggravates the problem.

Thus I reached the conclusion (too hastily, say my colleagues who teach languages) that it is nearly futile to teach oral skills in foreign languages in American schools and colleges. Language courses should concentrate on

developing reading skills and, above all, on teaching students how to build these skills themselves, simply by reading.

But how will one order breakfast when traveling abroad? The first step is to buy a Berlitz phrase book and a good pocket dictionary and use them diligently. The second step is to buy some tapes and listen to them. Then insist on using the language on every occasion until you are understood.

Following these methods, anyone who lacks shame can speak any foreign language, badly but understandably. The great enemy of foreign language learning is a sense of shame, an inability or unwillingness to become a child again and to let one's inadequacies show. I have found the method workable in thirty foreign lands, some using languages, such as Hungarian or Chinese or Turkish, usually regarded as esoteric. My shame returns, and I become speechless, only when I am in Paris, where the reputation of the natives for rudeness to those who do not speak their language perfectly inhibits me.

Playing this game has its dangers, however. If you ask questions in a foreign language, the natives will suppose that you will understand their answers, often a false assumption. Another danger is that you will gain the reputation of being an amazingly accomplished linguist on the basis of a vocabulary of a hundred words in each of twenty languages. But the game is enormous fun. I sometimes suspect that it is my main pleasure in travel, hence my reluctance to go to such places as England and Australia, where a sort of English is spoken. But watch out in Paris.

College Friends

During my first two years as an undergraduate, and a little less austerely during the third year, I lived as an intellectual. From 6:00 in the morning, when I rose, until 10:00 at night, when I went to bed, seven days a week, I was immersed in books and talk of books. That included mealtimes and most of my socializing hours. The survey courses provided a conversational common denominator, so that there was always shoptalk at the refectory table.*

On most Saturday evenings, a half-dozen of us would get together to drink cheap muscatel and read plays or argue philosophy. We were a varied

* In more recent years, at Carnegie Mellon University, I have been a strong advocate of establishing a core curriculum not just a distribution requirement in order to give students common topics of conversation in addition to pop music, TV shows, sports, sex, and the weather. My belief in the value of a core curriculum came directly out of my Chicago experience.

crew. Milton Wolford, coming from a wealthy family in southern Illinois, was pushed and pulled by existential concerns to philosophy. Because he was moody and was often victimized, like A. A. Milne's little donkey, he soon acquired the nickname "Eeyore." I was fond of him, but probably teased him far too much, and he was closer to some of the others in our group than to me.

Eeyore was susceptible to hypnotism. In fact, if one caught him sitting with his hands clasped, one could sometimes suggest to him quietly that he could not take them apartand then, in fact, he could not. But we all soon agreed that that was not a nice trick. Eeyore had more and more difficulty making his peace with the world and suffered frequent bouts of depression. He died, while yet an undergraduate, ostensibly of heart failure but really of *Weltschmerz,* or what today we might call existential angst.

Leo Shields, a lively, attractive Catholic boy from Salt Lake City, was brought to Chicago by the Thomism of its president, Robert Hutchins and the philosopher Mortimer Adler. There was something birdlike about his movements, always quick and alert. He was exceptionally bright, interested in literature and writing as well as political philosophy. His brother, Fracis, joined him on campus for a time. They looked so much alike and were so similar in manner that Francis instantly became a close friend of mine also. He and I took several pleasant hitchhiking trips togetherhitchhiking wasn't particularly dangerous then, and our absence from classes went unnoticed under the Chicago policies.

Leo, after graduating from Chicago and completing a doctoral degree in political philosophy at Notre Dame (with a thesis on social justice), was commissioned an infantry lieutenant in World War II. He died on D-Day, on a beach in Normandy.

The rest of us have been more fortunate. Winston Ashley arrived from Oklahoma, carrying a draft of his novel. The university brought him to Aristotle, St. Thomas Aquinas, and the Catholic Church, thence to a priesthood in the Dominican order and a life of service, teaching, and administration in the church.

Milton, Leo, and Winston were the unfettered spirits of our band. They soon concocted, or were introduced to (I never knew where it came from), a brand of Aristotelian-Thomist-Catholic-Trotskyism. The first three terms of the expression weren't hard to hyphenate together, but the last one gave it a strange turn. For them, it was very serious but also very playful, not beyond some posturing. At one point all three shaved their heads, in protest, I guess, against the iniquities of the world. Some time later, a lovely female student, Helen Ehrlich, whether by imitation or independent inspiration, did the same thing. In all four cases, the hair took a long while to grow

back. Helen looked charming at each stage of the process, but I can't say as much for the others.

Leo's and Winston's beliefs gradually grew into a more conventional Catholicism, but not until after they left Chicago. Meanwhile, all three were seriously pursuing their literary studies, both with Thornton Wilder, then a member of the faculty, and with the younger Norman Maclean. Maclean was an alumnus of the Beta Theta Pi fraternity, which had fallen on hard times. Encouraged by the fraternity, he sought new recruits among his students, innocently gathering up a considerable clump of the campus Trotskyites, including Leo and Winston. So Beta Theta Pi, for some time, was a citadel of Trotskyism, a very literary Trotskyism, to be sure.

At just this time, Manley Thompson joined us. His father owned a Cadillac agency in Zanesville, Ohio, and was disinclined to send his son to the University of Chicago, a well-known hotbed of radicalism and intellectualism. Manley, who had somehow been exposed while in high school to Whitehead and Russell's *Principia,* and had become infected with philosophy, was insistent. Father and son reached a bargain: Manley would go to the University of Chicago, but he would join his father's fraternity, Beta Theta Pi, to protect his American values. That is how we came to know Manley, who, after his studies at Chicago, divided his loyalties among Aristotle, Kant, and Peirce, and went on to a distinguished career in the University of Chicago's Philosophy Department.

Ellis Kohs, who also lived in the dormitory, began in sociology because "you couldn't earn a living in music." He was wronghe soon switched his major and became a composer of note and a professor of music. At our Saturday evening meetings, he often instructed us in music theory. We have followed his career over the years and enjoyed his music, and we continue to correspond with him.

I have already mentioned Harold Guetzkow, and since he played an important role in my education and my life for a quarter-century, I will say more about him than about the others, and even carry his story forward to foreshadow later events in my life.

Although Harold and I both grew up in Milwaukee, our homes located less than a mile apart, and although we are almost the same age, we did not meet before our encounter on the North Shore train carrying us to Chicago, because we attended different public schools. His father, already deceased when I met Harold, had been a businessman, but in other respects we arrived at college with similar backgrounds, and reacted to our new experiences in similar ways, neither of us being swept up by the theologies we encountered.

Our frequent mealtime conversations and bull sessions were, as I perhaps

fallibly remember them, most often about our common scientific interests, epistemology, and ethics. I had already embraced a logical positivism that I have never relinquished (I would prefer to call it *empiricism* now), while Harold at that time believed that a Darwinian teleology of evolution could furnish axioms for ethics. That disagreement supported many hours of talk. If Harold and I had visions of changing the world, it was by way of understanding it, of contributing to the scientific knowledge that could point to solutions of some of the world's problems. Harold focused his attention on psychology and education, while I studied my economics and political science.

Harold was rather earnest in those yearsnot lacking in humor, but not inclined to tomfoolery or pranks. I would have to rank him in the lowest quartile of college students for frivolity. He had a remarkable capacity for expressing moral indignation (real or assumed), which he used sometimes to silence us temporarily, coming out of his room and staring us down when we interrupted his studies with too much noise in the corridors. His manner reflected his sense of purpose, and during his undergraduate career, he never deviated from his goal, which was to prepare himself to teach high school.

Because we were following different curricula, Harold and I were able to point each other to ideas we might otherwise have missed. It was he who first called my attention to the psychologist Jean Piaget, who at that time was hardly mentioned by American behaviorists. He also told me of the work of Professor H. C. Morrison, in the School of Education, who had developed, as a basis for curriculum design, a form of what would now be called task analysis.

After graduation from Chicago in 1936, Harold went off to teach high school, but decided in 1940 to return to graduate school at the University of Michigan. By then, I was employed at the University of California in Berkeley, and our friendship was continued by exchange of letters. With the rapid movement of international events in those years, each of us had to make difficult personal choices about our prospective roles in the coming war.

I early became an interventionist, but by an irony of circumstances (see chapters 6 and 7) remained a civilian through the war. Harold became a conscientious objector, devoting more than four years of his life, the entire period of American involvement in the war, to civilian public service. As I write these lines, I have before me a letter from him, written in October 1940, at the time he was submitting to the draft board his request to be classified as a C.O. The letter was evidently part of a continuing correspondence, for it refers to previous discussion about nonviolent response to conflict.

I have also a very long letter of January 1942, to which I replied with an even longer one in March. (His was seven pages, mine twelve, typewritten and single-spaced!) There we debated passionately the relative merits of nonviolence and war as responses to Nazism. Rereading those pages today re-evokes the mental and moral struggle of those years, when each of us had to examine his most basic beliefs in order to make a personal commitment on one side or the other. I am a little ashamed, now, of a couple of the more flamboyant passages of my letter, but by and large, Harold and I argued it out rationally on pragmatic grounds: What kind of a world would result from war or from a Nazi conquest, and over what period of years did we measure the consequences? What were the prospects for nonviolent resistance to tyranny?

The arguments were the same as those echoed on college campuses in the years of the Vietnam War—not at all alien to the rhetoric of "Red or dead." Rereading them, I get fresh glimpses into the minds of later generations of students. Not that student concerns are different from mine, but at the age of twenty-five such concerns are examined from a different angle, from the perspective of a life still to be lived. I am reminded that the writers of those letters were dealing with their own future prospects, which is a little different from concern for the prospects of one's children and grandchildren.

Harold's January letter (more than my reply, I am afraid) exemplifies his lifelong willingness to examine and defend his positions thoughtfully and openly, tenaciously but not stubbornly. He never needed the admonition that Oliver Cromwell once addressed to the Church of Scotland: "I beseech you, in the bowels of Christ, think it possible you may be mistaken." While fully willing to act on his belief in nonviolence, putting his own life and career on the line, Harold never imagined that his conclusions on this or other topics were infallible. In his letter he not only set forth the arguments for his position but also stated fairly the specific objections his friends had raised against them. If my letter has the tone of a believer, seeking to convert, his conveys a real sense of an inner debate—albeit one that had by then largely been resolved.

As matters turned out, Harold's wartime public service did not require a complete abandonment of his scientific goals. At first he was attached to a forestry camp in the North Woods of Michigan. But the last years of the war were spent at the University of Minnesota, studying the effects of six months of starvation diets upon the behavior and physical and mental performance of a group of C.O. volunteers. Characteristically, Harold, with his colleague Paul Bowman, in addition to publishing scientific reports on the experiment, prepared a booklet called *Men and Hunger,* to help relief workers dealing with populations starving in war or famine. Basic and

applied science did not exist in separate compartments for him; each could contribute to the other.

During this same period, Harold performed a very valuable service for me. At Chicago I had discovered that he could be a tough critic, and we often read each other's essays in a no-holds-barred mode. Later, when I was revising my doctoral dissertation, looking toward publication, I sent it to Harold, whom I counted on to be its first real reader. The criticisms he supplied, then and later, made an important contribution to the final document.

The divergence of our political views and actions did not cool our friendship during the war years, although we met and corresponded infrequently. On his return to doctoral study at Michigan after the war, Harold undertook research with the Gestalt psychologist N. R. F. Maier on problem solving. His dissertation, which demonstrated greater stereotypy in female than in male subjects in problem-solving processes, produced no complaints of sexism in the 1940s. But then, Harold was wise enough to draw no conclusions about whether the differences he found represented nature or nurture.

Since cognition was at the center of research I was conducting in organizational decision making at Carnegie Institute of Technology, our professional interests began to converge. When Harold joined a research project that the social psychologist Roger Heyns had organized on communication and problem solving in groups, I began to consult with the project, making frequent visits from Carnegie Tech to the Michigan campus.

Soon, I succeeded in turning the movement in the other direction by persuading Harold to join the Carnegie faculty, where he served from 1950 to 1957, with joint appointments in business and psychology. (In chapter 9, I will describe the new Graduate School of Industrial Administration that we founded at Carnegie Tech in 1949.) Harold was a principal figure in all of our research on organizations and management. During the period, as we shall see, Carnegie was highly visible as having the largest single group of organizational researchers in captivity, but for a variety of reasons, the group began to disperse in the mid-1950s. Harold was seduced for a year by the Center for Advanced Study in the Behavioral Sciences, near Stanford, and departed permanently from our campus in 1957.

Harold's departure rested on such sound personal and professional reasons that I did not have it in my heart to oppose it strongly; I would not have been successful in changing his decision even if I had. For research on management had little direct relevance to Harold's central concerns with peace and international relations. The position he took at Northwestern University permitted full-time devotion to the questions of peace and war.

Although he was wholly committed to scientific inquiry as the route to effective social action, he wanted to apply his skills and energies to studies that bore directly on the conduct of international affairs.

After 1957, an occasional letter and even more infrequent visits replaced our almost daily conversations. I will not try to carry on his story beyond that time. His life exhibits a rare unity and consistency: from the beginning to the present, a concern with central human problems, especially the problems of peace; a belief that application of human reason and the methods of science offer the only real hope of grappling successfully with these problems; and a persisting adherence to that belief in both his personal and his professional life.

Hardly anyone would disagree that maintaining a peaceful world stands high on the agenda of urgent tasks for humankind. Yet few of us spend any large part of our lives addressing the questions of peace. In my own case, I know one of the reasons: the feelings of frustration and futility that are easily aroused when I try to think about problems that have no visible solution. The mind strays from unpleasant quandaries to more pleasant topics. Harold has never been a starry-eyed optimist, but at the same time he has never lost faith that reason, applied tenaciously, can make a difference.

These were the companions, then, with whom I spent my undergraduate years at Chicago. If my friends sound awfully highbrow, the truth is that they were. Even the horseplay was highbrow, perhaps sometimes precious. A friend of Eeyore, a student at the University of Illinois, was considering transferring to the University of Chicago. When we learned he was going to visit the campus, we invited him to attend one of our Saturday evening sessions. Then we thought we should do something special, so we organized a seance.

When Eeyore's friend arrived, we were already installed in a room illuminated by only a few candles. At one end was a volume of T. S. Eliot on a kind of altar, flanked by two candles. To make sure that our guest would not doze, we provided him with a chair with a broken seat. We then proceeded to read a series of learned papers on the relation of Tao to the music of Stravinsky, to the painting of Van Gogh, and so on. These were interspersed with readings from the *Cantos* of Ezra Pound and genuflections at the altar.

After the entertainment had gone on long enough, a couple of hours, we turned on the lights, brought out the wine, and tried to make our guest welcome. He seemed unaware of the change in pace, and remained in a sort

of dazed state. The next day, starting off for home, he boarded the wrong train. I don't say there was any causal connection, but it did happen. I don't believe that he transferred to the University of Chicago.

The caper was sophomoric enough, but sophomoric in a certain stylea style probably peculiar to University of Chicago sophomores.

Young Love

My life at the university was wholly monastic, but that does not mean I had lost my interest in girls. My affectionate relation with Ginnie was continuing by mail, but shyness kept me from seeking out new feminine companionship on the campus. Besides, I didn't dance.

After a few months in college, I began to suffer from considerable depressionprobably homesickness, although that diagnosis didn't occur to me. Life just seemed meaningless. By some tortuous mental route, this led me to review my love life and to decide that Ginnie was not enough of an intellectual to hold my interest, certainly not to spend a life with. (Did I want an intellectual communion with women that my father had not had with my mother? Did the heady atmosphere of the University of Chicago revise my standards of intellectuality?)

I went down my lists of female acquaintances. None near the top of the class at Chicago met my standards of beauty. More and more, my mind returned to Mary, whom I had known for years in the youth group of my church in Milwaukee, but who had never attracted mestrangely enough, because she was a beautiful girl, and certainly an intelligent one. The problem, I think, was that in her early teens she often dressed dowdily, her appearance hiding her beauty. Somewhere along the way she acquired taste. She was a year younger than I, and still in high school.

When I returned to Milwaukee in the spring of 1934 and met with our close-knit youth group (we called ourselves "The Heretics"), I immediately laid siege. There was the complication that she was then seriously dating my best friend, two years older than I, but I paid no attention to that. When I sat next to her at a party and took her hand, she did not snatch it away, and we were almost immediately on hand-holding terms. Her experience having surpassed mine, she soon introduced me to the pleasures of hands caressing smooth, bare flesh, innocent petting by today's standards but more thrilling to me then than I had imagined possible. Soon we were going steady. The friend I had displaced in her affections was a little sad, but not visibly angry. Since my relation with Ginnie had been undefined, there was

no official parting, and she remained a friend, though rarely seen, until I lost track of her whereabouts many years later.

On graduation from high school, Mary went to a nearby college, close enough to Chicago that I could occasionally visit for a weekend. This time it was I, however, who failed the intelligence test. The simple, though relatively chaste, pleasures of the flesh were so novel to me that I wanted to enjoy them to the point of (her) boredom. At her college, she found another boy who shared her interest in drama, and before a year had passed, I received a "Dear John," or rather a "Dear Herbert," letter.

I did not accept defeat gracefully. Since she was starring as Lady Macbeth in the college play, I learned by heart the entire role of Macbeth so that I could help her rehearse. (I can still recite large chunks of it, and could probably relearn it within a week.) But that did no good; I had broken the spell. During the sudden spring thaw of an exceptionally snowy winter, I visited her campus a last time, which, quite apart from the discomfort of wading through streams of icy water, was a dreadful, painful, embarrassing mistake.

Whether by reason of unrequited love or damaged pride, I then went into a state of rage and despair, in which I tortured myself with the thought that I would never again embrace her. Most of us have had such experiences; at the time, I was aware that mine was not unique, but that made it no less intolerable. When my pain became too bad, I drank my troubles away for a few hours.

As is well known, such despair is not sustainable indefinitely, and after some months I reached the point where I could apply my mind to the problem. The obvious solution was to find a new girl as desirable as Mary, and preferably on the Chicago campus where more continuous attention could be provided. Coeducational dancing lessons in the college produced a minimum level of a new social skill and, more important, an opportunity to meet girls.

I had a footloose senior and postgraduate year, with no permanent attachments, some pleasant times with a variety of cheerful girls at the university and with girls near Rockmarsh, from time to time too much drink, and enough stability so that my studies and work were not affected. (Somehow, I managed to maintain an A average through all the turmoil. Perhaps study and work were my real opiates.)

Philosophy and Politics

The University of Chicago community offered a number of philosophical and political options that were religious in their intensity. There were Aristotelianism and Thomism, in either their secular or their Catholic versions. There was Marxism, either Stalinist or Trotskyite. And, of course, the Aristotelian-Thomist-Catholic-Trotskyite sect, to which I was strongly exposed.

Aristotelianism and Thomism were also being introduced into the college curriculum through a "Great Books" course taught jointly by President Robert Hutchins and Professor Mortimer Adler. Adler conducted the sessions in a manner that he imagined was Socratic, but that I thought was hectoring. (Hutchins was relatively passive, possibly because his presidential duties did not give him time to prepare adequately. When he got into deep water, Adler bailed him out.) I did not enroll in the course, but several of us sometimes attended to cheer on the students as they tried to defend themselves against Adler. I remember especially an occasion when Adler was held off for two whole sessions by Helen Ehrlich (the girl who had, on some whim, shaved her head).

Alas, I had not the qualities of a true believer. I had arrived on campus a Socialist. From my Milwaukee background, *socialism* meant good city government and comfortable living for all. Soon after my arrival in Chicago, a friend took me to what he said would be a Communist meeting. There we found some middle-aged men in a shabby room above a storedenouncing Stalin! It was incomprehensible; I had not until that moment heard of Trotsky.

While I was intrigued by the arguments of my Trotskyite, Stalinist, Thomist, and Aristotelian friends, my distrust of long chains of logical reasoning kept me from belief. So I went out not far from where I came in, but I learned an enormous amount about these important social movements and their philosophical bases.

Although I steered clear of the Trotskyite and Stalinist organizations on campus, the political scene did not go without my attention. The continuing Depression and the rise of Nazism were particular objects of our concern. But most of my political activity as an undergraduate took the form of conversation.

One exception: As an assignment in one of our political science courses, we served as watchers, representatives of the county judge, at polling places in several elections. The political ward-heelers spent little effort trying to conceal from us the blatant fraud they perpetrated in many of the Chicago precincts we visited. Voters were coached; ballots were carried into the back

room, then returned. Most of us observed quietly without attempting to interfere, and reported our findings (futilely) afterward. One or two students who intervened, or tried to, returned to the campus with bloody noses or other minor injuries. No one, I believe, was shot. We did not inflict any mortal wounds on the political machine of Boss Edward Kelly, Chicago's mayor.

Graduate Studies

By the end of my second year, I had passed all the survey courses and most of my upper-division requirements in social science and political science. I had read widely in the humanities (even earning the second highest score among the students who took that comprehensive exam in 1935), and had strengthened my high school training in the physical and biological sciences. Behind me were good introductions to sociology and anthropology, a rather inadequate one to psychology, and somewhat more extensive training in economics and political science, so that I could devote part of my second year and almost all of my third year to work at the graduate level, which I pursued by sampling here and there of the university's many wares.

I tried to join my economics work with my interest in raising cattle, for I was then much involved in Rockmarsh (see chapter 2). At that time, the enormous Chicago stockyards still dominated the meat-packing industry, and I arranged to visit them on several occasions, making the acquaintance of cattle buyers and price experts. I found the drama of cattle trading fascinating, but achieved no new insight into it.

On the wall of my dormitory room, I maintained a chart, in color, of daily livestock prices. I thought hard but unsuccessfully about how to build a theory of supply and demand that would take into account the price relations among the different quality grades of cattle. That was probably one of the reasons I enrolled in Henry Schultz's econometrics courses in my third year.

As I finished the college requirements, and began to move into my major studies, the quasi-religious issues of the first year gave way to more technical ones. I found several faculty members from whom I could learn how to apply mathematics to empirical matters. Three persons outside the Political Science Department played especially important roles in this stage of my education: Nicholas Rashevsky, Henry Schultz, and Rudolf Carnap.

The mathematical biophysicist Nicholas Rashevsky had a marvelous talent for building simple assumptions into models of biological systems. Because he had convinced a wealthy Chicagoan that his models might unlock

the secrets of cancer, he was given substantial research funds, which he used to support graduate students in physics and mathematics, including at that time Alvin Weinberg, who later had a distinguished career at the Oak Ridge National Laboratory, and A. S. Householder, the numerical analyst. I learned a great deal from both Rashevsky and his brilliant students.

In spite of his skills in building mathematical models, Rashevsky was rather cavalier in his attitude toward data. He never acquired a deep command of the biological phenomena he was treating and, as a consequence, was generally ignored by biologists. They could have learned a great deal from him, and a few of them did, but the mutual respect was largely lacking on which effective interdisciplinary communication must build.

Rashevsky taught his seminar in the afternoon, and on warm days my drowsiness sometimes caused me to hallucinate. Rashevsky was a tall reddish-blond, blue-eyed man, as was the political scientist Harold Gosnell, with whom I also studied. The difference was that Gosnell was clean-shaven, while Rashevsky had a splendid long blond beard. I imagined, in my half-awake state, that if I pulled off Rashevsky's beard, I would find Gosnell underneath. Fortunately, I never carried out the experiment.

Years later, I was at a party at the home of the economist Jascha Marschak that Rashevsky was also attending. They began to compare notes on their respective whereabouts in 1917. Marschak (at about age eighteen) had been the Minister of Economics of the new socialist republic in the Caucasus, with his headquarters at Batum, on the Black Sea. Rashevsky had been a naval officer on a Russian cruiser that received orders to bombard Batum. By great good fortune, a storm came up and the cruiser never reached its destination. Their first meeting was postponed for many years, until both were washed up by history on the shores of Chicago.

Henry Schultz, unlike Rashevsky, was not a natural mathematician. His mathematics came hard to him, but he mastered it with great determination and thoroughness. His book *The Theory and Measurement of Demand* and his seminars provided me with a deep and thorough view of the uses of mathematics in economics and of modern statistical theory. Three things, in particular, I owe to him: an understanding of the general equilibrium theory of Walras; knowledge of the (then new) Neyman-Pearson theory of statistical tests; and an appreciation for the importance and difficulty of what we later learned to call the identification problem. He was always interested in the philosophical underpinnings of what he was doing, and referred his students to many interesting and valuable books, Alfred Lotka's *Elements of Physical Biology* being one of his favorites (and mine).

On one occasion in his econometrics class, Schultz gave us some data to

which we were to fit an equation. The data compared the heights with the weights of infants during their first six months of life. My Platonist instincts immediately suggested to me the formula $W = aH^3$. I reasoned that if the infants retained essentially the same shape (probably approximately true) and the same density, then their weights would have to grow as the cubes of their lengths.

My curve, fitted by this equation, explained the variance very wellmore than nine-tenths of it. Schultz, however, gave me only a B on the paper, insisting that I should have used the more general equation $W = aH^k$, and then tested whether k was significantly different from 3. Although I did not immediately agree with him, the incident stuck in my memory and perhaps helped convince me that in empirical science the final test is not mathematical elegance or *a priori* plausibility, but the match between theory and data. I certainly learned that lesson somewhere, ultimately overcoming my innate Platonism and armoring myself against the aesthetic lures of neoclassical economics, so responsive to mathematical elegance and so indifferent to data.

In 1938, Schultz completed his magnum opus, *The Theory and Measurement of Demand,* bought a car, learned to drive, took his family in the new car to California, and accidentally drove off a mountain. None of them survived. I had been fond of this shy, dedicated man, and felt the loss keenly. His place at Chicago was filled by Oscar Lange, subsequently vice president of Poland, who administered a statistics exam I took in 1940 as part of my doctoral requirements, and whom I came to know after my return to Chicago in 1942.

I attended, with more diligence than usual, several of Rudolf Carnap's courses on logic and philosophy of science. All three men communicated to me in their lectures something of how scienceat least science involving the applications of mathematicswas done.

Carnap was particularly important to me, for I had a strong interest in the logic of the social sciences. My thesis project (later published as *Administrative Behavior*) started out as a study of the logical foundations of administrative science. My files have yielded several early outlines and prospectuses of such a work, which I began planning in 1937. It would have been well if someone had sat me down and forced me through a formal course in symbol logic, but I followed my usual path of self-instruction, with the usual mixed results.

I wasn't totally ignorant of logic, however. A careful study of Carnap's *The Logical Syntax of Language* convinced me that his definition of *analytic,* a central term in his system, would not do what it was supposed to do. I

drafted a brief paper on the topic, which I shipped off to Carnap during the summer of 1937 (he was in Europe at the time). It was accompanied by this letter:

Dear Professor Carnap:

You will perhaps remember me as one of the auditors of your course last winter in "Logical Foundations of Mathematics." I was very much interested in the possibilities of applying your methods, and those of Professor Morris, to an analysis of the social sciences, and I am now writing a thesis in the Department of Political Science on "The Logical Structure of an Administrative Science."

The distinction of analytic from synthetic sentences plays a very important role in the thesis, and I was hopeful that I could employ the definitions which you develop in *The Logical Syntax of Language* for their rigor would make it possible to reach much more definite conclusions than if a less formal idiom were used. However, in the attempt to apply the definitions, I encountered some difficulties which I am unable to resolve. Therefore, I have drawn up the accompanying memorandum with the hope that by carefully setting out the problem and submitting it to you for your consideration, some light might be shed upon it. I would appreciate any comments you may have. . . .

He answered me courteously, and we had a conference on his return, in which he tried to show me that I was mistaken. However, in his 1942 book on semantics, he retreated from his earlier position in exactly the direction I had pointed. The faulty definition and its consequences are discussed on pages 177 to 179 of *The Logical Syntax of Language* (1937); Carnap's recantation is on pages 247 to 248 of *Introduction to Semantics* (1942). I think that by the time he wrote the second book he had forgotten our conversation, and certainly did not connect it with the revision of his position.

I have mentioned most of the important studies of my graduate years except my work in political science. That work was important for its content, but even more for the exposure it gave me to an ongoing scientific revolutiona preview of the revolutions I was going to observe and participate in during my career in several fields of science. It requires a chapter of its own.

Chapter 4
Encounter with a Scientific Revolution:
Political Science at Chicago

In addition to mathematics, logic, and economics, my other principal area of attention at the University of Chicago was political science, my major subject. Traditional political science consisted mostly of constitutional and administrative law, political philosophy, political institutions (mainly description of formal structures), public administration, international relations, and some history. At Chicago political science was different.

It introduced me not only to the ferment of intellectual life on a university campus but also to the larger drama of the great doctrinal struggles in the sciences as they move forward, encountering new phenomena and creating new paradigms. For the Chicago Political Science Department in which I found myself as an undergraduate and then a graduate student was the vanguard of the behavioralism that erupted in political science in the second through the fourth decades of the century and transformed that discipline.

Merriam and the Chicago School

The story is in considerable part that of Charles E. Merriam, chairman of the department through this whole period. He served as commander-in-chief and master strategist of the revolutionary forces. I was just a private in his army, attaining a commission (and drawing enemy fire) only after the publication of my revised dissertation, *Administrative Behavior,* in 1947. Having participated in these events for more than a decade, however, did much to form my views about how scientific disciplines develop, to teach me the strategies of subversion I later employed in attacking orthodoxy in economics and psychology, and to focus my sights on the phenomena of

human thinking and problem solving as the essential core of both organization theory and economics.

In giving an account of the Chicago School and my experiences with it, I have drawn on Barry D. Karl's biography, *Charles E. Merriam and the Study of Politics,* to remind me of events that I would otherwise fail to retrieve from memoryalthough I disagree with Karl on some matters of interpretation.

My relation to Charles Merriam, the Chief, was that of a studentand a very young onein his department. When I approached him, which was not often, it was with a certain trepidation and even awe. The relation became slightly more elaborate when, as we shall see, I subsequently wooed and wed his secretary, who was simultaneously a graduate student in the department.

A man of large enterprises, Merriam aspired to the mayoralty of a great city, and perhaps to still higher political office. Like any person of vision, he built organizations and won followers so that his personal energies could be multiplied and serve a larger purpose. The principal organization he built was the Political Science Department at the University of Chicago, from which his followers went out to revolutionize the discipline in the nation and in the world.

The goal of Merriam's enterprise was to bring intelligence to bear upon the political process, and thereby to ensure and accelerate human progress. Government and the political process were central to his vision of society. Political institutions provided the stage on which human life was acted, and set the boundaries and conditions that shaped economic and social institutions. He wrote: "It is a long road out of a slavery to inanimate nature, out of a slavery to human nature, up to the mastery of the dark and fateful forces around us and within; but the race is on its way. The future belongs to those who fuse intelligence with faith, and who with courage and determination grope their way forward from chance to choice, from blind adaptation to creative evolution" (Merriam 1936, p. 326).

Applying intelligence to the political scene requires an understanding of political institutions and processes. This understanding, Merriam thought, could be arrived at only through the scientific study of politics. And the scientific study of politics meant the study of human behavior, especially as carried on by psychology and her sister sciences.

When Merriam was learning his profession at the turn of the century, political science had little resemblance, in method or substance, to the natural sciences of the time, except possibly in the descriptive work of those scientists, even then becoming a bit old-fashioned, who were called natu-

ralists. But in the mainstream of political science, description was overshadowed by moral philosophy.

Yet the young Merriam in Chicago, recently emerged from his doctoral concerns with political philosophy, soon exhibited a strong empirical streak, investigating state and local governments in the spirit of the bureaus of governmental research that were then flourishing. His 1906 *Report of an Investigation of the Municipal Revenues of Chicago* was in this empirical tradition, as were the majority of his early journal publications. While all of this inquiry was tied closely to policy, he thought policy needed to be preceded by empirical inquiry into state and local government and his methods were reportorial.

A second phase of Merriam's research, foreshadowed by a paper in 1921, "The Present State of the Study of Politics," became wholly visible in his collaborations with Harold Lasswell and especially with Harold Gosnell. Of the origins of this shift, he says (in his third-person autobiographical fragment, "The Education of Charles E. Merriam"):

> But, alas, by this time he was profoundly dissatisfied with the basic methods of observation and analysis in political science. Systematic politics was again delayed in the search for firmer ground upon which to proceed. . . .
>
> Meanwhile, with a view to finding sounder methods, Merriam had begun . . . investigations in various lines of special significance: studies in non-voting and quantitative methods with Dr. Gosnell and others. There came studies in propaganda and then in political psychology with Dr. Lasswell, leading to more elaborate inquiries in the field of psychoanalysis on the part of Dr. Harold [Lasswell]; studies in the field of political leadership, running through a series of special monographs, by Gosneil, Johnson, Peel, Robertson, Cohen, on down to the present still unfinished study of leadership by Louis Olum. [Merriam 1942, pp. 9 11]

It is hard to exaggerate the novelty of the methods used by Merriam and Gosnell in their 1924 study *Non-Voting*, published at least a decade before public opinion polls became part of the U.S. presidential election process. The appendices to that volume contain an admirable discussion of the methodology of polling, and even explain the use of Hollerith punch cards, presaging our contemporary dependence on the computer. Just a few years afterward, Gosnell, in *Getting Out the Vote* (1927), followed up this pioneering polling study with one using the even more revolutionary technique of field experiment. None of Merriam's own subsequent publications employed methodological innovations of this, or any other, kind. His further

contributions to methodology were all achieved indirectly, through his department and its scholars.

Finally, we come to Merriam's systematic works, and especially *Political Power* and *Systematic Politics,* published in 1934 and 1945, respectively. I will focus on the former, which is the better book; but most of my remarks apply to both. The author himself explains his method in the introduction to *Political Power:* "It is not my purpose to repeat or refute the conclusions of the masters of political dialectics. Acknowledging my deep obligation to such thinkers, I propose to set forth what I have found out about the nature of political power during my years of reading, reflection, observation, experience" (p. 3).

The book is entirely consistent with the author's characterization"reading, reflection, observation, experience." It reports no new empirical results and, for that matter, almost no specific old empirical results. The references, only moderate in number, are mainly to books, and seldom to specific chapters. Thus the reader encounters a series of assertions about the nature and workings of political power set forth without any explicit evidential base.

But before condemning *Political Power* as a throwback to an earlier era, we must put it in its proper temporal context. In 1934 the outlier was not *Political Power* but the handful of revolutionary works by the Chicago School that had preceded it. A more appropriate comparison would be with Carl J. Friedrich's *Constitutional Government and Democracy* (1941), which has more ample references but essentially the same expository style. From an even broader standpoint, *Political Power* belongs to a respectable tradition of empirical work in the social sciences. Even in experimental psychology, the most "scientific" of the social sciences, William James's *The Principles of Psychology,* based on the same kind of common sense and common experience as *Political Power,* was still an important and respected book (and remains so today).

The social sciences have simultaneously suffered and benefited from the fact that many of the phenomena of human behavior are open for all of us to see and hear as part of our daily experience. We do not need telescopes, microscopes, Geiger counters, or radio detectors to observe the overt aspects of human behavior. (On occasion, electronic bugs might be helpful, but in most circumstances they are frowned upon as research instruments.) As a consequence, much knowledge about human societyeven knowledge that might be termed "scientific"has been derived from observation and experience.

William James was this kind of naturalist, an observer of himself and others, who performed almost no experiments in the laboratory. Economics,

too, has made almost a positive virtue of avoiding direct, systematic observation of individual human beings while valuing the casual empiricism of the economist's armchair introspection. The authors of the great classics of political science were also naturalistsbeginning with Aristotle, proceeding through Machiavelli, to Alexis de Tocqueville and James Bryce, who produced classic descriptions of the social and political systems in the United States.

Such works are no less empirical for being based on common observation and experience. Their methodological deficits are far more subtle than a departure from empiricism would be, and it is these deficits that Merriam sensed in his "modern" period of the 1920s: lack of concern for sampling and the representativeness of data, lack of access to thoughts not immediately expressed in action (for example, voting intentions), and lack of access to behavior that is not a part of everyone's experience (such as the decision-making processes of high government and corporate officials). And because of these deficits, it was difficult to carry on social science research with the same standards of objectivity and public reproducibility as are demanded in the natural sciences.

While Merriam vigorously promoted methodological innovation aimed at removing these deficits, he himself was too intrigued with the Big Questions to wait patiently until the tools were in place. Hence, *Political Power* and *Systematic Politics* both belong to the classical, but empirical, tradition of observation, experience, and reflection.*

The influence Merriam exercised through his department and his colleagues and students seems less inscrutable than does Merriam the man, whose mystery was enhanced by his heavy-lidded eyes and the mischievous teasing manner in his conversation and his autobiographical writing.

Student Life in the Department

I do not know when Charles Merriam became the Chief. He already had that title when I came upon the scene, and he had become chairman of Chicago's Political Science Department much earlier, in 1923. As much as any academic department I have known, this was the chairman's department. The principal faculty members were his appointees, pursuing lines of research that, for the most part, he had initially envisaged, perhaps in collaboration with them.

* I am all too aware that my own *Administrative Behavior,* while almost wholly empirical in intent and content, lies within this same classical tradition. However behavioral its content, the "facts" in that book are derived largely from observation and experience.

While I was a student in the department, the "faculty" meant primarily Charles Merriam, Harold Gosnell, Harold Lasswell, and Frederick Schuman. It was Lasswell's psychologizing and Gosnell's quantitative and empirical methods that most specifically symbolized the Chicago School. But what characterized it even more fundamentally for me, and I think for a number of other graduate students, was its commitment to the proposition that political science is science. Along with that commitment went a dissolving of departmental boundaries that made the whole university, and all of its methodologies, available to the students of political science.

The alliance with sociology and survey methodology was, of course, close. So was the relation with L. L. Thurstone's factor analysis in the Psychology Department. It was thought only slightly peculiar that I studied mathematical economics and econometrics, logic, and applied mathematicsalthough Professor Marshall Dimock did once ask me accusingly, "Are you an intellectual fly-by-night?" The question would never have occurred to Merriam, or Gosnell, or Lasswell.

Obviously, not all the political science students tasted every delicacy in this large and diverse cafeteria. Some tried one dish, some another. But the openness of the department made it natural for its faculty and graduates to play leading roles in the assimilation of political science with the other social sciences that characterized behavioral science development after World War II. Conversation with sociologists, social psychologists, anthropologists, and economists came naturally to them.

The Merriam department did not long survive, without transformation, Merriam's departure. Most of its stars had left before Merriam's retirement: Schuman, Lasswell, Gosnell. The reasons for their departure were complex. Certainly there had been harassment from the Hutchins administration, wrapped in its own dreams of Aristotelian and Thomistic glory and wholly unsympathetic to behavioralism in political science. There had also been a damaging attack on the department as communist, initiated by Charles Walgreen of drugstore fame, whose niece had been a student in one of Schuman's courses. Merriam defended fiercely, with an eloquent statement to the committee of the Illinois legislature that investigated the matter, and the university administration behaved properly. But undoubtedly this affair cost Merriam some of his brownie points with Hutchins, and Schuman, somehow, left soon afterward. (Walgreen later apologized to the extent of liberally endowing in the University a lectureship in American Institutions.)

The department rapidly and radically changed its characterperhaps *declined* is the wrong word, an alumnus's expression of nostalgia for things as they had beenand Yale soon became the flagship of behavioralism in

political science. Organizational Golden Ages, whether in government, universities, or business firms, seldom endure beyond the generation of people who create them. Although the Chicago School flourished on the Chicago campus for only about two decades, its life expanded and spread over the whole academic world, and it still represents a mainstream, if not *the* main-stream, of political science today.

It would be hard to arrange a greater diversity in personality and talents than prevailed in the faculty. They were as often rivals as friends, and not above giving students glimpses of their (not always complimentary) views of one another. (The shy Gosnell, for example, relieved some of his aggressions by drawing and privately exhibiting uncomplimentary caricatures of his colleagues.) But there was among the students a great pride of belonging to a brilliant enterprise and great camaraderie. Although each student had a thesis adviser, I don't recall them as being sharply divided by these attachments. Merriam, Lasswell, Gosnell, and Schuman were all, in some way, common property, important parts of the scene for all of us.

Central to the scene, but also a little above it, was Merriam, larger than life. I found his classes rather dull, while other students were stimulated by them. But his conversationwhen he held forth in his office, or at the Shoreland bar, on the occasions when I was invited to join him there, or at dinners with the graduate studentswas always fascinating. He was always in command, and one always had the sense of being in the presence of great events, intellectual and political.

We would get glimpses of the National Resources Planning Board, or the President's Committee on Administrative Management, or the doings of the Public Administration Clearing House, managed by his close associate and crony Louis Brownlow. Merriam was a genial Olympian figure sitting in our midst, powerful, sometimes a little unkind in his teasing (more so to his faculty than to students), but generally a source of benevolence.

I retain a vivid memory of one of my last meetings with him. It must have been around 1947, and we had invited to Illinois Tech, where I was then teaching, a distinguished political scientist, Merriam's junior by perhaps twenty years, to give a lecture. After the lecture we had arranged a small dinner at the University Club, in downtown Chicago, to which Merriam was invited. Toward the end of the dinner, our lecturer began to tell us how he had managed Germany for General Clay during the postwar occupation, while a second distinguished guest had comparable tales to tell about how he had managed Japan for General MacArthur. It seemed all wrong to me: Merriam should be telling the stories, we should be listening. It seemed wrong to him, tooafter about half an hour, he turned to me and said, "Come on, Simon, we're going home." And we did.

Influence of the Chicago School

Who were Merriam's followers? "Followers" is not quite the right word, but "disciples" would be even less accurate. To attract disciples, one must provide certainty, and a catechism from which there can be no deviations and which can be recited to solve nearly all problems. Neoclassical economics provides that kind of certainty. So do Skinnerian psychology, Chomskian linguistics, Piagetian developmental psychology. There is no Merriamic political science. Political science of the Chicago School provided a goalto understand political behavior and political processesand some directions from which to approach it: data and theories in psychology, economics, and the other social sciences and modern techniques of experimentation, statistical analysis, and mathematical modeling. There were plenty of problems to which these data, theory, and techniques could be applied, but no simple template for applying them and no guarantee of the form the results would take. Hence *followers,* but certainly not *disciples*.

In assessing the influence of the Chicago School upon political science, one must avoid the fallacy of *post hoc, propter hoc*. Political scientists today make great use of polls and opinion and attitude studies. Among the first studies of this kind in political science were those done at Chicago. Therefore . . . ? Today, loyalties are a common topic of political science research. Among the first thoroughly psychological and empirical studies of nationalism were those done at Chicago. Therefore . . . ? The syllogisms are almost irresistible, but probably wrong. One could just as easily find the first cause in Columbia's Sociology Department (Paul Lazarsfeld), Harvard's Sociology Department (Samuel Stouffer), the Survey Research Center at Michigan, or the pioneering, and very early, contributions of Franklin Giddings, Stuart Rice, and Malcolm Willey.

The true cause is probably the gradual and steady advance of both social science methodology and social science concepts in the first three decades of this century in several disciplines and on many campuses, the advance that produced *Recent Social Trends* (1933) and *The Encyclopaedia of the Social Sciences* (1935). That advance prepared the great burst of activity, soon nourished by Ford Foundation largess, witnessed after World War II. Political science was just part of the total prewar movement in social science, and the Chicago School found itself at Chicago partly because a good deal of the early activity had taken place on that campus, in both sociology and psychology, and partly because Charles Merriam's openness to novelty enabled him to take advantage of it and import it into political science, to which it was not indigenous.

The new behavioral political science evoked great resistance from traditional political scientists. The noise of battle at times grew loud, and this struggle, like most, produced more than a little confusion. Political theorists felt threatened and took to the barricades, not recognizing that there was room for more than one venture in the domain of political science.

The stridency of the rhetoric against the behavioralists probably reached its peak in a set of essays written by disciples of Leo Strauss, edited by Herbert Storing and published in 1962 as *Essays on the Scientific Study of Politics*. One chapter of the book was devoted to flaying my *Administrative Behavior,* and Lasswell and other leading behavioralists were disposed of similarly. I felt honored to find myself in their distinguished company. The Storing essays were such egregious examples of the practice of reading texts unsympathetically and without a genuine attempt to understand them that I never felt an urge to respond to them.

The battle is long over, and we are all winners. Mathematics and statistics were brought to political science by the summer workshops of the Social Science Research Council and later, the Mathematical Social Science Board. The economists subsequently arrived on their proselytizing mission, bringing the strict rationality of public choice and game theory. The methodological sophistication of papers in the *American Political Science Review* has become more than respectable. Yet there is still room in that journal for papers on political theory, and even for papers on political behavior that do not flaunt path analysis or numerical data. Political scientists can all attend quietly to the business of advancing knowledge and improving techniques.

The Chicago School was probably not the final cause of these developments, but to an important degree it was the proximate cause. For, to a considerable extent, the explorers and pioneers were the followers of that school. We can read a long list of their names among the presidents of the American Political Science Association. And their attainment of that position provides some measure of the stunning impact the Chicago School has had upon the profession.

As for myself, there could have been no better school than the Political Science Department at Chicago to teach me about the march of ideas, and how the interplay of scientific research with the social organization of the disciplines determines its direction and pace. It helped me understand that new ideas do not fly solely on their own wings; the scientist is a communicator as well as a discoverersometimes even a missionary.

The Road to a Career

My career was determined for me in a very casual way. The branching points in the maze of my life offered easy choices. Other people in my environment presented me with opportunities. When the opportunities were attractive, I took them. Two or three such choices (hardly decisions, for I did not search for alternatives) set me on a definite path. For a term paper in an undergraduate course on municipal government in 1935, I wrote a description of the city government of my native Milwaukee. My professor, Jerome Kerwin, was then studying the relation between city governments and school boards. Liking my paper and recalling that the administration of recreation in Milwaukee involved cooperation between city government and school district, he suggested that I write a paper on that arrangement.

That paper was also well received. Kerwin wondered, however, why I had limited myself to describing the organization and had not evaluated it. As I had not the least idea of how to do such an evaluation, in 1936 I enrolled in Clarence Ridley's course on "Measuring Municipal Governments." My economics training suggested that the evaluation problem could be formulated as one of utility maximization subject to a budget constraint, so I wrote a paper in that vein. That led to an invitation to serve as a research assistant to Ridley on a large project he was undertaking.

By that unproblematic route, I found myself, in September 1936, in possession of a Bachelor's degree and supporting myself as a graduate assistant on a salary of $62.50 per month, which freed me from financial dependence on my parents. About midyear, I received a check for $83.33. Alarmed that it might be a clerical mistake, and that months later I might be asked to repay money I had already spent, I carried the check to the department secretary. She assured me that I had been promoted from half-time to two-thirds-time assistant and that the money was rightfully mine.

My new job virtually ended my formal graduate coursework. I continued to attend a few classes but my heart was in the research, the results of which later emerged as the monograph *Measuring Municipal Activities* (1938), co-authored with Ridley. By February 1937, I had my first publication, for we initially put our work out serially in the journal *Public Management*.

This was my first experience in scientific collaboration. Ridley's role in the project was to participate in planning the series, to review my plans for the individual papers, to help me find people and experiences that could inform me, and to review drafts of each chapter. The initiative was in my hands, and so was almost all the writing. It was an easy collaboration, the first of more than eighty research partnerships I would have, each quite different from the others.

Turning out a paper each month on the measurement problems of a different city department kept me busy, especially since I had had virtually no first-hand acquaintance with government when I started, beyond the two term papers I had written. That did not daunt Ridley, who shipped me out to Wichita, Kansas, just as the wheat was sprouting in the spring of 1937, to ride in police squad cars and study police records systems in the department there, run by Chief O. W. Wilson.

Although I looked no older than my actual twenty years, Clarence Ridley began to exhibit great confidence in me. When he was unable to speak on the measurement of municipal activities at the annual meeting of the Wisconsin League of Municipalities, the principal professional organization of local officials in the state, he suggested that they invite me instead. Too naïve and cocky to be either amazed or frightened, I took the train to Green Bay and gave my talk. I was treated cordially, the newspapers reported my speech without smirking, and no one in the audience seemed to notice that they had been addressed by someone who had become old enough to vote just three months previously.

A similar invitation took me to the national meeting of the Governmental Research Association on the Cornell campus in Ithaca. At least the participants there were mainly academicians, a familiar tribe. On a third occasion, Ridley delegated me to a conference with Professors Henry Beyle and Spencer Parratt at Syracuse University, who were approaching the topic of municipal measurement from a somewhat different angle than ours, and who wanted to discuss the relation of the two approaches. They took me to lunch with the entire political science department and subjected me to an oral examination. Feeling like the young Jesus in the Temple, I think I successfully acquitted myself. (By the biblical account, he did also.)

I should have asked Clarence Ridley in later years just what he had in mind in sending me on these missions, why he thought it appropriate. He was, after all, a man of wide professional and managerial experience, innovative but politically shrewd and not given to flights of fancy. Alas, I never put the question. Perhaps I was afraid that I wouldn't like the answer.

Dorothea

That brings me to June 15, 1937, my twenty-first birthday and the day on which the first panel of my tetraptych ends and the second begins. The Political Science Department secretary and graduate student who had reassured me about my raise was a lovely red-haired girl named Dorothea

Pye.* A friend (Bill Cooperyou'll come across that name again) arranged a double date for the evening of June 14. My salary permitted dinner and a play. We saw the Federal Writers' Project production of *Abe Lincoln in Illinois*.

Dorothea and I began to date regularly, then as we found more and more pleasure in each other's company, more often. I found in her someone with whom I could share my intellectual interests and collaborate in political activism. We had the same basic view of the threatening but exciting Depression world that surrounded us, and similar ideas about how it should be changed.

She was very beautiful, her curly bright hair framing a broad Scottish face and bright blue eyes. Tall and slim, she walked with a graceful, boyish stride. Best of all, she radiated friendliness, optimism, and sheer pleasure in being alive. What she saw in me, I will leave for her to tell. Soon, we decided we wanted to remain together permanently, and on Christmas Day of the same year we were married.

My memory brings back one other association with my twenty-first birthday. That morning I caught a plane at Chicago's Midway airport, a DC-3 that took me to New York, stopping en route at Detroit and Buffalo and giving me a beautiful aerial view, my first, of Niagara Falls. Ridley had arranged for me to present our project at professional meetings in New York and Washington, D.C.

The trip was full of exciting, inconsequential adventures. An indigent actor in Manhattan conned me out of $5.00 to pay his overdue hotel bill. I stayed up all night on Broadway and played chess, at 25 cents for each game I lost, with an unemployed chessmaster. In Washington, I was taken to lunch at the Cosmos Club, then at its original location on Lafayette Square. *Tobacco Road* was playing at the old Belasco Theater off Pennsylvania Avenue, but I had to miss the last act to catch the B&O train back to Chicago. It was a heady experience for a young man just turned twenty-one.

* "How did you know her hair was red?" the perceptive reader might wonder. Well, I had early been told that there were no green-haired people, nor any red lawns. Ergo. . . .

THE SECOND PANEL
THE SCIENTIST AS A YOUNG MAN

Chapter 5
A Taste of Research:
The City Managers' Association

By the time Dorothea and I married, my research assistantship had been transformed into a full-time job, at $150 a month, and I was sure it would continue at least one more month.

Our research budget, funded by a university committee, had nearly run out. Clarence Ridley sent me before the committee to justify a renewal; once again, I did not question his judgment that I was a suitable delegate. The meeting was at the Shoreland Hotel, at whose bar Charles Merriam, Louis Brownlow, and others of the Olympians often congregated after workan awesome place.

Merriam, who took me to dinner before the meeting, was at his genial, disarming best. But at the meeting, I was subjected to some pretty rough questioning. Louis Wirth, the sociologist, muttered in my hearing, as he entered the room late: "What are we discussing? Oh, the Ridley and Simon stuff. Not very good, is it?" My reaction was simply rage that these distinguished academicians could not recognize first-class work when they saw it. Of course, I did not express my anger during the meeting, but waited until I met with Dorothea afterward. She was properly sympathetic, although she may have found my self-assurance a bit startling.

The arrogance (or is it only confidence?) of the young can be impressive. But mine survived my youth. Even today, my knee-jerk response to referees and other critics is the same: How can they be so foolish? I sometimes need a cooling-off period of hours or days before I can make a rational reply.

Our project did receive additional financing (from that committee or another). When our research monograph was finished, I began to devote my time to Ridley's organization, the International City Managers' Association (ICMA), becoming a staff member in 1938. My duties were partly editorial (as assistant editor to the monthly *Public Management* and the

annual *Municipal Year Book*), partly statistical (I gradually assumed responsibility for the statistical sections of the yearbook), and partly auctorial (writing numerous chapters for training manuals for city executives). My job in ICMA was a marvelous school in administration, my tasks as challenging as I could wish.

An Encounter with Computation

Through a course I had taken in statistics, I was aware of IBM punched-card equipment and its labor-saving capabilities. It occurred to me to try to mechanize the statistical work of the *Year Book*. Having discovered that the University of Chicago Bookstore had keypunch, sorting, and tabulating machines, I mastered the technology with the help of some books I found in the university library, and arranged with the bookstore to use their equipment.

Thus, while preparing the statistical tables, I enjoyed my first, prehistoric experience with computers. The calculating punch was especially important, for it taught me that a machine could be programmed (rewired in that case) to make it do what you wanted it to do—to make simple arithmetic calculations and to rearrange the columns in which information was printed.

Of course, what the calculating punch could do was very limited, and it had no internal stored program in the modern sense. You had to insert plugs attached to wires into the side of the machine so as to make the right inputs connect with the right outputs. But the seed of an idea had been planted in my mind, and from that time on I was alert to any scraps of news I encountered about the progress of calculating machines. I had no idea that 1 would find a use for them; they simply fascinated me.

Clarence Ridley

I have already said enough about Clarence Ridley to suggest that he was not an ordinary man. Educated as a civil engineer, he had served as city engineer and city manager in several communities before returning to Syracuse University for his Ph.D. He then came to Chicago as adjunct professor at the University of Chicago and director of the International City Managers' Association. He saw this organization as pivotally located for improving local government administration, and exhibited masterly leadership and skill in exploiting the opportunity.

When he took charge of it, ICMA was a rather typical professional society, publishing a journal and holding an annual meeting that allowed city managers, always lonely in their posts (like all top executives), to come together and commiserate with one another. Ridley created a whole series of new services that would be of value to the members. The *Municipal Year Book* provided information that permitted managers to compare their cities with others. A set of textbooks on municipal administration, covering almost every city service, formed the basis for correspondence courses that ICMA offered. The association also provided limited consulting services to its members, among other activities.

Ridley never forgot that the city managers were his key clientele, and his past experience made him one of them; but he also had a much broader goal of improving city government generally. City manager government was still a minority form that had hardly penetrated the larger cities at all. The task was to build up cadres of effective professional municipal administrators, of which city managers would simply be prime examples. Fire chiefs could take the fire department administration course, whatever form of government their city had.

Ridley had an excellent sense of organizational politics. Like every professional association director, he wanted loyalty and active participation from his members, although not to the point where he turned the leadership over to them. But his skills and his rapport with members prevented any real difficulty on this dimension.

The morale of his own small staff—about half a dozen of us—was also high, for he was always presenting us with challenges and was generous in sharing credit. He had a temper, which was well under control and mainly used to express his occasional disgust with the world's foibles. He was more interested in solving problems than in placing blame on those who caused them.

He understood both the uses and the dangers of opportunism. After I had moved to Berkeley, we once had breakfast together when he was visiting the Bay area. He was describing to me some new activities he was planning. I asked, "Will the Spellman Fund approve of that?" (The Spellman Fund was, apart from membership dues, ICMA's main source of income.) He turned to me sternly, and said, "I earned my living before I ever heard of the Spellman Fund, and I think I could earn it again if they went away." I stored the lesson and usefully recalled it on a number of later occasions.

I don't know whether, or how often, Clarence Ridley was offered other positions during the years he directed ICMA. I cannot believe he did not have numerous alternative opportunities. But he understood clearly that

solid accomplishment takes timein an organization, usually many years. Solid accomplishment was what he wanted, and he was willing to take whatever time was needed.

For Ridley, the grass was always greenest on his own side of the fence. Watching him, I came to understand that well-managed organizations are powerful instruments for achieving socially important goals, and not yokes around the necks of their members. A few years later, in *Administrative Behavior,* I tried to explain how organizations can expand human rationality, a view quite opposed to popular folklore in our society, which commonly sees them as dehumanizing bureaucracies.

I have portrayed Clarence Ridley as the very model of an effective administrator, because he was just that. If he had any serious shortcomings, I cannot remember them. A photograph of him is one of seven I have hung on the walls of my study. The others are my father, Charles Merriam, Chester Barnard (businessman and author of *The Functions of the Executive* [1938]), Franklin Delano Roosevelt, Abraham Lincoln, and Albert Einstein.

The Science of Administration

At the age of twenty-two, I was writing a major part of a volume called *The Technique of Municipal Administration,* which was supposed to inform experienced city managers how to run a city. Since I had had no administrative experience and had hardly even observed organizations, except for my brief excursions to Milwaukee and Wichita and my position in ICMA, it was not immediately obvious to me what I should write. Of course, my task was not to invent a new theory but to assemble the existing knowledge, quite doable if one could write clear English.

In the field of public administration at that time (and in public and private management generally), there was nearly a consensus, nowadays referred to as "classical organization theory." You could find the core of it in Leonard White's *Introduction to the Study of Public Administration* (1926), and even more explicitly in *Papers on the Science of Administration* (1937), assembled by Luther Gulick and Lyndall Urwick as a bible for the staff of the President's Committee on Administrative Management.*

* This committee, consisting of Louis Brownlow (chairman), Charles Merriam, and Luther Gulick, was appointed by President Roosevelt to propose improvements in the organization of the federal government. Its report recommended, among many other things, the "six men with a passion for anonymity" who became the kernel from which grew today's Executive Office of the President. The report spawned a violent political battle between the president

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The classical theory put great stock in orderliness in organizations, for example, a clear division of labor and departmentalization based upon it, unity of command, a limited span of control for each manager. While one could find a few mild dissents, the classical theory dominated the literature and provided the main body of lore upon which I drew for the *Technique* book.

In fact, the occasional deviations in the literature from the classical theory were not so much dissent as the seeds of new paradigms, one of them focused on human relations and motivation, another on decision making. Both made sense to me, and the latter, especially, resonated with my previous studies of the Milwaukee recreation programs and the measurement of city services.

It was while I was engaged in this work that I first encountered Chester Barnard's newly published *The Functions of the Executive* (1938), which seemed to me wholly superior to the other administrative literature of the day and fully compatible with my preference for looking at management in decision-making terms. Aided by the insights gained from Barnard, I soon realized that a little administrative experience goes a long way. Life in organizations is not very different from life elsewhere. Most of the writing on administration, including Barnard's, was based on everyday observation, not on esoteric experimental or observational techniques.

Organizations, it appeared, could be understood by applying to them what you knew of human behavior generally. Where specific experience was lacking, metaphors and analogies might fill the gap. For example, the phenomena of loyalty and identification, so central to the working of organizations, were quite visible in every school I had attended, not the least at football games. It even occurred to me that the mediating role I had sometimes played as a boy, when misunderstandings arose between my mother and grandmother, was not wholly unlike the role of the foreman as "man in the middle" between blue-collar workers and management.

But this reliance of administrative theory on common sense was not entirely acceptable to me. Systematic observation and experimentation were badly needed if this field was ever to become scientific. But until someone built a satisfactory theoretical framework, it would not be clear what kinds of empirical studies were called for.

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and Congress, comparable only to the fight of the same period over President Roosevelt's plan to "pack" the Supreme Court by enlarging it and appointing new liberal members. These battles contributed much to the crystallization of the bloc of Southern conservatives that ended for many years the dominance of liberals in the House of Representatives.

These reflections planted the first seeds of *Administrative Behavior*. I decided to write a theoretical doctoral thesis on decision making in administration, thereby modifying my earlier intent to write on the logic of administration. The thesis would raise many empirical questions that could be explored subsequently in my research. This decision set the central strategy for my research in organizations over the next twenty years, but, busy with my daily tasks, I did not begin the work while I was at ICMA.

Sociability

In the summer of 1937, Ellis Kohs and I had found an apartment ($35 a month) a few blocks west of the university campus, near Cottage Grove Avenue, and had moved there from the dormitory. It was understood that when Dorothea and I married, she would move in and Ellis would move out. After our wedding at my home in Milwaukee, we had a week's fine honeymoon at Turkey Run, a beautiful state park in Indiana, where we met two other congenial newlywed couples and ate enormous amounts of food at ridiculously low pricesthat is, at prices we could afford. Then Dorothea and I set up housekeeping.

I had already begun to learn what a talent she had for friendship, and how much her smile made up for my sometime prickliness. We soon had a wide circle of friends, many drawn from the graduate student group in political science and other social science departments, some from the Public Administration Clearing House, where ICMA was situated, some from my friends of undergraduate days, some who shared my philosophy of science interests (mostly people I had encountered in Nicholas Rashevsky's classes), and some whom we met through our liberal political activities. Our political science friends included a half-dozen married couples, a surprising fraction of whom remained married during the succeeding half-century.

Not many of the specifics of our social life in those years remain in my memory, and those that do are a haphazard sample. Since both of us had jobs, we generally ate out, and for a time belonged to a cooperative eating club. We often ate lunch at the university's Commons, where we met David Rockefeller, then a graduate student in economics. As we paid the cashier, David would sometimes buy us each a chocolate mint, keeping up the tradition of the dimes his grandfather, John D. Rockefeller so regularly passed out to strangers he encountered. David was a pleasant, bright, and unpretentious person.

We soon established a kind of salon, taking the place of the Saturday

evening discussions at Burton-Judson dormitories. More or less weekly, those friends who were especially interested in philosophy of science gathered in our apartment. Occasionally, we invited an outsider to join uson one occasion, Carnap camebut most of the participants were young graduate students, including the philosopher Herbert Bohnert, another philosopher named Carl Lienau, who carried in his coat pocket a small Hindi stone idol, a biology and medical student named Brucer, the physicist Al Weinberg, and others. Logical positivism was the dominant, perhaps exclusive, religion in this group, and we took turns talking about our special interests or projects.

Both with and without our friends Dorothea and I also enjoyed outdoor activities, which on weekends were pretty much limited, by reasons of geography and the lack of a car, to the local Chicago beaches, the Indiana Dunes south of Chicago, and the Waukegan Dunes to the north. I have already discussed some of our more ambitious hiking ventures, such as the trip in Door County during a summer vacation. Occasionally we rented horses for a ride in Jackson Park. In the winter we sometimes skated.

In retrospect, this period in our lives was a busy but uncomplicated time, during which Dorothea and I learned to know and enjoy each other, and through which we lived pleasantly without any master plan or serious concern for the future. There were no important branch points in the maze until 1939.

A California Junket

Ridley and I had become nationally known authorities on measuring public services, a topic of considerable interest because of the difficult financial plight of the citiesthen as now. Samuel May, director of the Bureau of Public Administration at the University of California in Berkeley, looked for Rockefeller Foundation support to continue some studies of local government that had been started in the Bureau with federal work relief (WPA) funds.

Sam May conceived the idea of getting a small grant from Stacy May[*] of the Rockefeller Foundation to bring me to Berkeley in the late spring of 1938 to plan such a study and write a proposal to the Foundation. The planning grant was awarded, paying my expenses and salary for three

[*] To avoid confusion, we referred to Sam May of Berkeley as "Maybe" and Stacy May as "Maybenot."

months, and I set off on the first of my long train rides to California. I was greeted at the San Leandro depot by Dorothea's mother, whom I had not met before and, after a weekend in her home, established myself in the International House on the Berkeley campus. My mother-in-law had warned Dorothea against marrying "a mere child," but she and I soon became and remained good friends.

At Berkeley, I teamed up with Milton Chernin, the assistant director of the Bureau and a few years my senior. Together, we toiled through the summer to produce a document outlining a three-year study that would cost the Foundation the munificent total of $30,000 and would support three researchers, a statistical assistant, and a secretary for three years. The WPA workers, some fifty strong, who had been employed on the Bureau's previous statistical studies would also be attached to the project, but paid from federal funds.

The trip was not all labor. I had my first view of the Sierra Nevada at Yosemite on Memorial Day weekend, and on another weekend drove down to Stanford University with Dorothea's mother, finding a sleepy rural campus that looked like a medieval Italian monastery basking in the summer heat and sun. As we admired the frescoes on the facade of the chapel, a string quartet practiced baroque music in the background.

Dorothea ended my several months' exile by joining me for a couple of weeks to attend her sister's wedding; then she and I drove down the coast along the recently opened highway to Big Sur, which she had visited a couple of years previously while the road was being built. She took me to Slade's Creek, near which the much publicized Esalen Institute of encounter group fame was later built, and along a trail to the edge of the ocean cliff. Affixed to the face of the cliff just below a gushing spring of steaming hot water, we found a platform on which had been installed two old-fashioned enameled bathtubs and a large wooden barrel into which water could be diverted and cooled. Mixing the cold water with the water directly from the spring, we took our baths, looking out on the Pacific Ocean from the hundred-foot height. Precursors of California hot tubs.

When the Berkeley proposal was complete, I returned to Chicago to work for ICMA and, for a brief period, for Public Administration Service, a nonprofit consulting service for municipalities, spun off from ICMA and directed by Donald Stone. Early in 1939, the Rockefeller Foundation made a grant for the three-year Berkeley project, to begin in the autumn, and I was invited to come as director of the studies (with the rank of research assistant, as I did not yet possess a doctorate). Deciding to accept the in-

vitation took no more than ten minutes. Again it was obvious which branch of the maze I should follow.

On one of our last weekends in Chicago, Dorothea and I visited the Waukegan Dunes. Before boarding the North Shore train, we bought a Sunday newspaper. The headlines told us that Hitler and Stalin had signed a mutual nonaggression pact. Whatever lingering hopes any of us still had for a popular front against Nazism dissolved at that moment.

Chapter 6
Managing Research:
Berkeley

Dorothea and I set out for Berkeley at the end of the summer of 1939, intending to make the trip a bit of a holiday. The Burlington Railroad from Chicago to Denver connected with the Denver and Rio Grande Western. From Denver the train, nosing south to Pueblo, Colorado, pierced the Front Range through the Royal Gorge, then plodded steadily northward again up the valley of the Arkansas, with the Front Range red in reflected sunlight to the east and the shadowed, snow-capped Sawatch Range to the west. For five hours the locomotives throbbed rhythmically, lifting their heavy load 1,000 feet upward each hour, just the vertical pace of a mountain hiker on a steep trail. One by one, Mounts Princeton, Yale, and Harvard passed in review (three almost identical pyramids, but the latter a few hundred feet higher than its rivals). Gradually the valley narrowed.

Around sunset, the altered click-clack of the wheels told you that you had crossed Tennessee Pass and the Continental Divide, and were now descending the valley of the Eagle River, a tributary of the Colorado. In another hour, the train would halt at Glenwood Springs, where you could disembark and spend a couple of days at the old resort hotel, enjoying the mountains and the sulfurous hot springs.

From Glenwood Springs, the D&RGW brought you next to Salt Lake City, where the Western Pacific picked up the Pullman cars for the rest of the trip. If you chose the right train, you would sleep through most of the Nevada desert and enter California through the Feather River Canyon the next morning. It was a wonderful trip, full of gorgeous mountain scenery, the long half-day pull up the Arkansas valley being especially dramatic, and the transcontinental Pullman providing much of the ambiance of a sea voyage, but without storms or seasickness. Perhaps you can still do something like that on Amtrak; I haven't checked the schedules recently.

Dorothea was returning to her native California soil, I was making my second visit there. We reached Glenwood Springs on the evening of August 30, and spent the next day on a mountain trail above the hotel. That night we turned in early. Our room was on the first floor, facing a large interior courtyard of the hotel. At 2:00 A.M. I was awakened by a radio blasting from the court. I recognized the loud rasping voice instantly, and felt the hair bristle on my neck. Adolf Hitler was announcing that German armies had entered Poland.

The war had little immediate effect on our professional lives, almost no effect at all for two years. But except for the lull of the Phony War,* those years were full of terrifying news.

The Bureau of Public Administration

When my mind was not turned to the events in Europe, my three years at Berkeley as director of Administrative Measurement Studies were as exciting and illuminating as the previous three years had been. I learned to manage an organization of considerable size (not only my own staff of five, but the fifty WPA workers who were attached to our project and, while we were studying the State Relief Administration, several hundred people in that organization as well). Somehow, my social self was ablemost of the timeto overcome the introverted self. I learned how to delegate. I even learned how to fire an unsatisfactory employee.

I was able to delegate much of the direct management task. The WPA group had a supervisor, a cheerful and able young Mormon graduate student, whose name I have not been able to recover. Bill Divine, who had just finished his studies in public administration at Pomona, took chief responsibility for the large project in the State Relief Administration in Los Angeles; he lived there and I visited every couple of weeks. Fred Sharp supervised most of the extensive field work for our studies of land use in the San Francisco Bay area. Ronald Shephard saw to it that we used appropriate statistical techniques and theories in all our work. Hence, my administrative duties were largely limited to supervising these immediate assistants and a secretary, planning and budgeting for the project, and hiring staff replacements. I learned early that (in principle, at least) it gets easier, not harder, to administer as you move upward in an organization.

* After the destruction of Poland in September 1939, there was little Nazi military action in Europe until the assault on Denmark and Norway in April 1940. Hence, "Phony War" for the interlude.

Nominally, Sam May, director of the Bureau of Public Administration, was my boss and in charge of the project. But he paid little attention to it, being busy learning to ice skate at the new rink in Berkeley and courting a young woman who soon became his second wife (he had been a widower). Often forgetting that he was there, I made the hiring and firing decisions that properly were his, and had complete responsibility for the plans and budget. Once or twice he expressed irritation at my failure to consult him, but he never reversed my decisions.

He paid more attention to some other Bureau functions, especially the advisory service it provided to the state legislature. I pitched in a couple of times on legislative studies, writing a report on water problems in the Central Valley, then as now a major set of issues in California politics, and on reapportionment of the state legislature. Once I helped draft the annual report to the legislature of UC Berkeley President Robert Gordon Sproul. He told us to be sure to devote plenty of space to medicine, agriculture, and engineering; the rest didn't matter much.

My colleague of the previous summer, Milton Chernin, a senior member of the Bureau staff and fully twenty-nine years of age, with his Ph.D. behind him, was the older brother who instructed me on the novel problems of managing a large project, and who served as front man when the demands of extroversion were too much for me. He participated actively in our study with the State Welfare Administration and advised on the others.

Chernin also served as a surrogate director for most of the other programs of the Bureau. At this time he had no tenure track position at Berkeley, and no strong prospects for one, but friends on the faculty who were aware of his talents were working on it. Before we left, he had become a faculty member in the School of Social Welfare, and subsequently dean, a position he held until his retirement from the faculty many years later. He also became a major force in Berkeley academic politics, and after his retirement served as president of the Faculty Club for a number of years. He died in 1989. When I visited the Berkeley campus early in 1990 to give the Hitchcock Lectures, I was delighted and moved to find his portrait beaming down on me, hanging over the fireplace of the Club. He had been a mainstay of the campus for fifty years.

Chernin was a small, rather homely man (Dorothea dissents, but that was my perception), graying and balding prematurely, with a big nose on an invariably cheerful face. He was full of jokes and stories, all of them with a point and many of them self-deprecating. He was extremely bright, oriented to social policy more than to science. He was a staunch liberal, and, as we shall see, was sometimes accused, without basis, of Communist tendencies and associations. Modest to a fault, more than generous in sharing

credit, he was loved deeply by his associates, hence always able to claim their loyalty. It was a joy to have him around, together with his attractive, bright, skeptical, streetwise wife, Gertrude, a social worker.

Victor Jones and his wife, Annie Mae, had come to Berkeley the previous year from Chicago, where they had been our good friends in the Political Science Department. Victor and I and several others shared an office for several years in the Bureau, very amicably, although we sometimes had to draw a chalk line on the floor to prevent territorial encroachments. Victor was a professional Southerner, with a great fondness for black-eyed peas. (They always looked to me like beans instead of peas.)

There were too many others at the Bureau to mention here. It was our social as well as our professional homean exuberant young group of political scientists who both worked together and partied together. At a party celebrating my success in the final oral Ph.D. exam at Chicago, they presented me with a copy of Bertrand Russell's *An Inquiry into Meaning and Truth,* in which I find all of their names inscribed to remind me of those times.

For the first two years, we lived in a tiny cottage on Virginia Street, north of the campus and high above it, with a gorgeous view of the Golden Gate. The living room floor sloped noticeably westward to a large French door, ten feet above the walk, making things a little precarious for people who drank too much during a party, but we never lost a guest in this way. After Dorothea became pregnant, we moved down the hill, bitterly regretting our loss of the view, but glad to be relieved of the steep climb home each evening.

The landlord of our aerie, the ancient Mr. Greeley, didn't maintain the premises very well. Between the bottom of the front door panel and the sill was a one-inch gap, allowing free entry to the rainwater that flowed across our porch during the winter rains, and free entry also to a friendly little garter snake. When we asked Mr. Greeley to replace the door, he replied, "I'd like to, but it fits so well." This statement was so outrageously contrafactual, and uttered so matter-of-factly, that we were silenced, despairing of communicating.

The Research Project

As the research project got under way, we did not take too seriously the details of the Rockefeller proposal, and especially the rather flaky data we had inherited from the local government study, but asked ourselves what kind of measurement studies would both be feasible and make substantial contributions to the field. We wanted to show how quantitative empirical

research could contribute to the understanding and solution of municipal problems. In the course of the three years, we completed three major studies, producing a monograph from each (Simon et al. 1941; Simon, Shephard, and Sharp 1943; Simon 1943), as well as a number of papers.

Here I had a different experience of collegiality than in my previous work with Ridley. Now I was the "boss," but my colleagues were my contemporaries, and we worked as equals, formal authority seldom showing itself. Bill Divine, Ronald Shephard, and Fred Sharp each had a rather distinct sphere of work, so that I tended to work with them one-on-one, rather than as a group. We found each other congenial, and our relations were warm.

The first study, a field experiment we carried out in the State Relief Administration, was something I would never have dared had I been experienced enough to understand what it entailed. It was, I think, the largest experiment that had ever been carried out in an organization up to that time, comparable in scope to the Hawthorne studies of worker attitudes and productivity, carried out in the Western Electric Company during the 1930s, and more systematically designed. Its purpose was to determine how large the case loads of social workers should be for the most effective operation of the agency. Bill Divine and I have told much of the story of the research elsewhere (Simon and Divine 1941), and in chapter 8 I will describe how it became embroiled in a heated California state political battle.

The SRA study generated a great mass of data. To process them, we made arrangements to use the equipment in the Los Angeles Service Bureau of IBM. There we encountered new machines with wired plugboards that were far more flexible and powerful than the old tabulator I had used in Chicago. This was my second experience with prehistoric computers, further whetting my appetite and curiosity.

The second study, an analysis of fire risks and losses, was a natural successor to the measurement research that Ridley and I had done. It involved a detailed analysis of land use maps for the Bay area, and a correlation of building construction and use with fire losses, so that fire loss experience could be compared among cities after appropriate allowance for the amounts and kinds of property at risk.

Only a few insurance companies had at that time enough statistical sophistication to appreciate the study, and to the best of my knowledge, it went largely unnoticed and unused. Nearly forty years later, I received an admiring letter from a fire insurance actuary, who assured me that the study had been a generation ahead of its time. That was comforting, but did not make the work seem less futile.

The third study, *Fiscal Aspects of Metropolitan Consolidation* (Simon 1943a), was a theoretical analysis of the incidence of urban property taxes

together with an examination of the actual pattern of municipal revenues and services in the San Francisco Metropolitan Area, the two analyses leading to conclusions about what economic effects the consolidation of local governments in the metropolitan area would have.

A paper drawn from the study was published in the *Quarterly Journal of Economics* (1943b), my first technical publication in that discipline, and later reprinted by the American Economic Association. It was a standard reference on property tax incidence for many years. The important lesson I learned from this analysis was that my conclusions depended at least as much on certain assumptions about boundary conditions as on the central assumptions of economic rationality that lie at the core of neoclassical theory.

By *boundary conditions* I mean the assumptions that have to be made about which indirect effects of a change in taxes the human actors would take into account in making their decisions and which they would ignore. Would they respond to the prospect that increasing the tax would lower the return on capital generally, or would that be neglected in their calculations? The answer to that question turned out to make all the difference with regard to who would ultimately pay the tax. The "action" lay in the boundary conditions, not in the assumption that people were trying to behave optimally. Recognizing this fact later provided me with an important clue for building the bridge between theories of human bounded rationality and economic theory.

Completing the Doctorate

Apart from what I learned in carrying out the research project, the period at Berkeley contributed in several other ways to my education. Through exposure to Ronald Shephard and Kenneth May, who were doctoral students of the mathematician and economist Griffith Evans and the mathematical statistician Jerzy Neyman, I received an education in economics and statistics that took me well beyond what I had learned at Chicago. Acknowledgments in my publications during the 1950s record what I learned from them about the method of comparative statics in economics and about the theory of statistical tests.

While we were at Berkeley, I also completed my University of Chicago Ph.D. in political science. By arrangement, I took a three-month leave of absence in 1940 to prepare for preliminary examinations in constitutional law, political theory, political parties and propaganda, and statistics, which I was permitted to write in Berkeley under the supervision of the Political

Science Department there. I was permitted to take a statistics exam in place of that in international relations usually required; the statistics exam was constructed by Oskar Lange of the Economics Department at Chicago.

Before mailing the exams back to Chicago, I had them copied as insurance against possible loss, and I still have those copies. On casual rereading they now look most impressive—I was able to cite hundreds of Supreme Court cases by title and date and to drop the names of numerous obscure political philosophers. On the other hand, while my answers on the political parties and propaganda exam had seemed brilliant and even original to me at the time, they seem less so now. Occasionally the questions I answered were rather different from the ones that were asked, still a common failing on students' examinations.

In answering one question on statistics, I ostentatiously provided two separate derivations of the *chi*-square distribution. (While taking my shower on the morning of that exam, it came to me with blinding and unaccountable certainty that there would be a question on *chi*-square, and I boned up on it before setting out for the exam room. On no other occasion have I had such loving attention from my guardian angel.)

Apparently I displayed enough erudition on the examinations to satisfy the department. Leonard White was kind enough to write me before Christmas that, while the committee had not yet met to make a decision, having seen the grades on the examinations, he had no doubts about what its decision would be. Shortly after the beginning of the new year, 1941, I was notified that I had passed. With the exams out of the way, I began to devote evenings and those weekends I could spare to drafting my thesis.

The document gradually expanded from an outline to a few paragraphs for each chapter, then to a full draft. My committee (Professors White, Pritchett, Ridley, and Perry from the Philosophy Department) responded with minimal criticisms, mainly because the first two were unwilling to claim that they knew what I was doing. The most searching critique I got was from Charner Marquis Perry, a specialist in ethics. The second draft was approved, and I was allowed to return to Chicago in May 1942 for my examination.

The exam was scheduled for early afternoon. I stopped to watch a match on the tennis courts at 58th Street, was caught in a heavy Chicago spring shower, and arrived at the examination room drenched. The committee (Herman Finer was present, but Lasswell, Gosnell, and Schuman were not) had been looking at my graduate transcript, and had discovered to their astonishment and even consternation that the only graduate course for which I had credit was in boxing (I had earned a B). I explained that I had relied on the Chicago policy that demanded successful performance only

on comprehensive exams; and besides, credit for a number of my graduate courses had been recorded on my undergraduate transcript (which they did not have before them). The explanation was accepted, how grudgingly I don't know.

My only other difficulty on the exam derived from my stubborn positivism. The examiners, especially devout Catholic Jerry Kerwin and British Labourite Herman Finer, found it difficult to believe that one could not *prove,* from self-evident premises, that Hitler was a bad man. And if one couldn't prove it, what right had one to believe it? I doubt whether they bought my positivist explanation that choice begins with faith in value premises, not with proof of their correctness. But at the end of the exam they conferred for only about fifteen minutes before calling me in to congratulate me.

What was the extent of my formal education when I had earned the Ph.D.? In addition to the broad general education that my high school and the University of Chicago had given me, I had excellent training in political science and a solid foundation in economics. I had made a modest beginning in mathematics, a basis for subsequent self-instruction. I had studied bits and dabs of science.

Clearly I had strong preparation for the teaching and research in administration, economics, and even operations research, that would largely occupy me for nearly fifteen years after I received my degree. Unless practice in self-education is such preparation, however, I had almost no background for the work in computer science, artificial intelligence, and cognitive psychology that would occupy me for most of the rest of my life. Of course these were new or radically changing fields when I entered them. Since my colleagues and I were active in creating these new disciplines, we had plenty of time for learning and no problem of catching up with our predecessors. Young researchers who moved into molecular biology in the 1940s can tell a similar story. Interdisciplinary adventure is easiest in new fields.

Writing *Administrative Behavior*

As I have mentioned, my dissertation later became *Administrative Behavior,* and it contains almost all the essential content of the book. I have also offered an explanation (or rationalization) of how it was possible for me to write such a document without extensive experience of administration, and I should like to expand on it.

The dissertation examined administration as a decision-making process, introducing that framework with the help of a maze metaphor: "A simplified

model of human decision making is provided by the behavior of a white rat when he is confronted, in the psychological laboratory, with a maze, one path of which leads to food." As several readers of the manuscript who provided me with comments prior to publication of the revised thesis objected strongly to the human/rat analogy, it disappeared, along with the maze metaphor, from the published version. But it is clear from this brief quotation and the pages that followed it in the dissertation that I viewed decision making very much in terms of making successive choices along a branching path.

Whether I had been preoccupied with mazes prior to this time, I cannot recall. They were prominent in the writings of the principal author upon whom I relied for my psychological facts, Edward Tolman of the University of California at Berkeley. As was true of most other behaviorist psychologists of that time, the subjects of his experiments on choice were usually rats, not people.

The dissertation contains the foundation and much of the superstructure of the theory of bounded rationality that has been my lodestar for nearly fifty years. The idea had its origins in the Milwaukee recreation study, was reinforced by what I had discovered about the boundary conditions of rationality in the California tax incidence study, and was not contradicted by any of the management or other human experiences I had had in my six working years, or in the years that preceded them.

In a previous chapter, I explained how, without any management experience, I wrote, at twenty-two, a textbook on municipal administration, to be used in a training course for city managers. I was thoroughly familiar with the literature of public administration and management, and simply culled and organized this information, with some sharpening of the logic and some translation into the specific terms and situations of municipal administration. Only intelligence and literary skill were required, not experience.

But how could I, at age twenty-five and with minimal management experience, have written *Administrative Behavior,* a book that challenged (more or less successfully and correctly, as it turned out) much of the received administrative theory of its day, and provided a new conceptual framework (decision making) for the analysis and description of organizational phenomena?

Part of the answer is that I had read Chester I. Barnard's *Functions of the Executive* with painstaking care shortly after its publication in 1938, and had led a discussion group with my colleagues in Berkeley in which we examined it closely. In *Administrative Behavior,* I acknowledged Barnard as the source of many of my central ideas regarding authority, the "zone

of indifference" or "acceptance," the equilibrium of inducements and contributions, and other basic topics. Although Barnard did not construct a systematic theory of decision making, much of his discussion was directed at the executive's decision-making processes. All of these intellectual debts can be traced through the footnotes of my book.

Barnard's contribution to my notion of organizational identification is more equivocal. The idea is certainly present in his book (see, for example, the distinction between organizational and personal decisions, pages 187 89, from which I quote at length on pages 203 4 of *Administrative Behavior*), but I had come to it much earlier, while working on the Milwaukee recreation study in 1935 (see *Administrative Behavior,* pages 211 12).

The other central idea in my book that appears in only muted form in Barnard's is bounded rationality. The closest parallel is Barnard's notion of opportunism and strategic factors, ideas that he derives from John R. Commons. Since I had also read Commons, the latter's *Institutional Economics* may have been a common source for these various conceptions of rationality that deviate from the economists' maximization of subjective expected utility.

Administrative Behavior also contained a provocative discussion of the "proverbs of administration." This attack on traditional principles of administration was derived almost purely from the logical structure and internal inconsistency of the principles themselves. No experience of organization was required to detect it, just a taste for rigor in reasoning.

Now the idea of bounded rationality, which appears to be the most novel and original component of the work, is not specifically an organizational concept. It applies as fully to individual decision making as to organizational decision making. By the age of twenty-five, I had already had ample experiences in life to understand the limits of the economists' framework of maximizing subjective expected utility as applied to actual human behavior. The scantiness of my experiences with organizations posed no particular limit to my development of an alternative approach to decision making.

Applying the ideas of bounded rationality to organizations could then be easily achieved with only a bookish knowledge of organizations. It was simply necessary to ask what the implications of bounded rationality were for the division of labor, for authority, for organizational identification, for coordination, and so on. Inference rather than empirical observation could, and did, guide this analysis.

That this kind of inference led to a realistic account of many organizational phenomena is the surprising outcome of the writing of *Administrative Behavior;* within the work I explain the underlying insight that led me to it:

Rationality, then, does not determine behavior. Within the area of rationality behavior is perfectly flexible and adaptable to abilities, goals, and knowledge. Instead, behavior is determined by the irrational and nonrational elements that bound the area of rationality. The area of rationality is the area of adaptability to these nonrational elements. Two persons, given the same possible alternatives, the same values, the same knowlege, can rationally reach only the same decision. Hence, administrative theory must be concerned with the limits of rationality, and the manner in which organization affects these limits for the person making a decision. The theory must determine . . . how institutionalized decisions can be made to conform to values developed within a broader organization structure. The theory must be a critique of the effect (judged from the point of view of the whole organization) of the organizational structure upon the decisions of its component parts and its individual members. [P. 241]

Around 1945, while I was teaching at Illinois Institute of Technology, I revised my thesis, circulated it for comment, revised it again, found an editor willing to risk it (Donald Porter Geddes at Macmillan), and published it in 1947. It was built around two interrelated ideas that have been at the core of my whole intellectual activity: (1) human beings are able to achieve only a very bounded rationality, and (2) as one consequence of their cognitive limitations, they are prone to identify with subgoals. I would not object to having my whole scientific output described as largely a glossa rather elaborate gloss, to be sureon the pages of *Administrative Behavior* where these ideas are first set forth (especially pages 39 41, 204 12, and 240 44).

One of the people to whom I circulated the manuscript was Chester Barnard, whom I had admired since his book appeared a decade earlier, but whom I had never met or even communicated with. He replied by sending me about fifteen pages of insightful comments. This emboldened me to ask him if he would write a foreword to the book, which he did.

The book created no sensation when it appeared, but it was widely and quite favorably reviewed in journals of public administration and business management. Chester Barnard's foreword undoubtedly contributed to its good reception. At the time, I was disappointed that none of the reviewers recognized it as the revolutionary document I firmly believed it to be, but in retrospect, I think I was treated generously. It sold a couple of thousand copies within a year or two after publication, gradually declined to about four hundred a year, and then, after the fifth year, began a steady increase in sales. It has remained in print since, never selling less than a few thousand copies each year.

America Enters the War

The years in Chicago and Berkeley were lived against the grim background of the Götterdämmerung being prepared and enacted in Europe. The Spanish Civil War, the rape of Austria and Czechoslovakia, the Hitler-Stalin pact, the invasion of Poland, the fall of France, the bombing of Britainall evoke in me vivid memories of anger and frustration.

In the spring of 1940, at a movie theater in Los Angeles, I watched with tears of rage the newsreels of divebombers destroying buildings and strafing farmers in the unprotected Norwegian countryside. I took comfort from the apparent invincibility of the French army and fortresses, until I reread the forecasts of Liddell Hart and J. F. C. Fuller in the *Encyclopedia Britannica* and knew that the Blitzkrieg would not be halted.

In the terrible summer of that year, I often studied on the Berkeley hillside overlooking the Bay and the Golden Gate, frequently interrupted by gloomy thoughts of the destruction of Europe, and perhaps the Free World. Hitler had touched my life directly, too, if only slightly: My father's sister, niece, and niece's husband escaped from Germany; Uncle Julius died either during a flight on foot over the Pyrenees or in a concentration camp.

We heard Winston Churchill's brave words of defiance, and hung on the news of the bombing of Britain. We were relieved when Ambassador Kennedy stated (perhaps with fingers crossed) his confidence in the survival of Great Britain, and thereby gave support to President Roosevelt's re-election campaign. I never doubted that Great Britain would repel the assault, but all the reasons for my confidence were (by historical hindsight) specious, the products of wishful thinking. I learned from this and later experiences during World War II that I am an incorrigible optimist, perhaps sustained by the even stauncher optimism of my wife.

At the beginning of July 1941, the Russian armies were reeling under the Nazi attack. In my office, I kept track of their movements with pins on a map. I could not believe that Stalin had been so stupid as to allow his main armies to be trapped in Poland and White Russia; I was much more optimistic than those around me, thinking the Germans had greatly exaggerated their claims of prisoners. I pointed to the huge distances in eastern, as compared with western, Europe and to the logistic difficulties the Germans would soon encounter.

A group of us drove up to the Russian River for the Fourth of July weekend holiday. I sat in the sun, weeding a strawberry patch (I could weed the plants even if I couldn't spot the berries because of my color blindness). From a nearby house, a radio was booming out the sententious words of

H. V. Kaltenborn, the oracle of the airways in those days, as he pronounced a death sentence on the Russian armies. Although I didn't believe him—despised him, in fact—it was depressing. I clung to my faith in logistics and in Stalin's rationality. (I turned out to be right, again mostly for the wrong reasons.)

Later in the summer of 1941, we joined our neighbors Dan and Lucille Arnon for a pack trip on the Muir Trail in the high Sierra. As we followed the San Joaquin River higher and higher toward its sources, then branched off to Evolution Valley, the mountains became our world. There was no Berkeley, no war; just the great peaks around us, and the stream rushing through the green valley, and the daily tasks of pitching camp, fishing, cooking, rounding up and packing the mules, Daisy and Ruby. We climbed the steep trail to Evolution Basin, huddled at the timberline under the sheer 1,000-foot cliff of the Darwin Range.

Just short of Muir Pass, we turned back and began to retrace our steps. The spell was broken only when we came to the gap overlooking Evolution Valley and could again see the foothills above the Central Valley. Then we remembered Berkeley and Europe. After being away for two weeks, we had no knowledge of whether our country was at war or at peace.

A little further down the trail we met a family just coming in: a husband and wife with two little girls. The younger girl proudly told us that, although two years earlier she had ridden a horse, this time she was walking all the way. We asked for the news. America was still at peace, they said. But the war came, of course, soon after.

On the morning of Sunday, December 7, I was lying on the floor of our small living room, reading the *New York Times* (I had not yet kicked the newspaper habit) and listening to a radio symphony program. Dorothea was in the next room. The music was interrupted for an announcement. As with millions of other Americans, that is how the news of Pearl Harbor broke in on our quiet Sunday. My first reaction was one of relief, a release from the tension of the long and anxious waiting. Now Hitler was doomed. My second reaction was, "The poor Japanese, they don't stand a chance."

As I had been an ardent interventionist, I felt I should enlist promptly for combat service. But because of my education and experience, I thought it not unreasonable to seek a commission. Easier said than done, because of my color blindness. But Professor Joseph Harris in Berkeley's Political Science Department was a good friend, and his brother was commanding general of the Ninth Service Command. With Joe's encouragement and letter of introduction to his brother, I sought a waiver for my color blindness for

service in the Coast Artillery (which had mostly become anti-aircraft artillery).

In a letter to Joe Harris on March 24, 1942, I wrote:

> My own plans for the coming year are more indefinite than ever. I have made preliminary inquiries at the Presidio where I was informed that it is unlikely that my color blindness would be waived for any of the combat Arms. The Navy rejected me on the same grounds last week. I have written to your brother, but there has not been time yet for a reply. I have not yet given up hope, but I have given some thought to what I should do if I can't get into the Services.
>
> I have decided, first of all, that I will not take a commission to do any purely paper-work in the Army or Navy, even if my defect is waived. I think I can be more useful as a civilian. If I do stay in civilian life, I believe that I can accomplish more as a teacher than as a minor functionary in some "war agency." Hence, I am keeping my eyes open for a teaching post. If any inquiries come your way, I hope you will keep me in mind.

After a bit of correspondence, the waiver was forthcoming from the Ninth Service Command. Soon I was asked to report for my medical exam, at Fort Baker, just north of the Golden Gate. I appeared, carrying the sheaf of correspondence relating to the waiver, which had become an inch thick with the endorsements of successive layers of the military bureaucracy. I was declared hale, a recent high-tech repair to one of my teeth was admired by the dental examiners, my color blindness, caught by the Ishahara test, was duly noted, and I was sent, still stark naked, to the commanding colonel. He went slowly through my file, page by page, from bottom to top. At last he raised his head and said, "Oh, that waiver is for limited service in the combat arms, and the damned Democrats have changed the rules again so that we cannot grant waivers for limited service."

My choice was between training for a commission in a noncombat branch with a desk job for the duration, or eventual conscription as a private. On those terms, I preferred to stay out until called. My "greetings," postponed because of my family responsibilities, by a temporary loss of papers between Chicago and Berkeley, and by a siege of mononucleosis that delayed a medical exam, finally arrived the week before Hiroshima, but were canceled when Japan surrendered and the rules about dependents were changed (by this time we had two children). I would be lying if I said I was sorry to have missed that war; perhaps just a little guilty.

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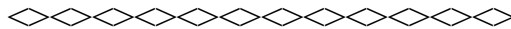
The three Berkeley years were a contrast in black and whitethe grim shadow of the war falling on the bright sunlight of my professional and social life. But it is the sunlight that dominates my memories of it: the exhilaration of directing the project research, the comradeship of Chernie, Victor, Bill, Fred, and Shep, and the others in the Bureau and the Political Science Department, the gradual emergence of the thesis out of my moonlighting labors, the economics and statistics I learned by association with Shep and Kenny May and, through them, with Griffith Evans and Jerzy Neyman. When I visit California now, it is easy to work up nostalgia for it. I merely have to walk up the steep hill of Virginia Street to our little cottage, which still stands there overlooking the Golden Gate, but has experienced upward mobility with the addition of a second story.

Chapter 7
Teaching at Illinois Tech

When it became increasingly probable, in the spring of 1942, that I was not immediately going to become a soldier, and when my degree was in hand, I began to think about how I would support myself and my family when the Berkeley project ended. Once again, I had no real decision to make. Victor Jones, whom I had known first as a graduate student at Chicago, then as a colleague at Berkeley, and who was now returning to Berkeley after a year at Illinois Institute of Technology in Chicago, recommended me for the position he vacated there.

There were two hurdles: When it still appeared that I was going into military service, Dean John Larkin at IIT had offered the job to another person. Fortunately, he turned it down. Second, Dean Larkin was a little skeptical of my qualificationsa long list of publications but no teaching experience. Would I be able and willing to teach large numbers of under-graduates? His doubts overcome, he offered me the job and I accepted. I was pleased at the prospect, for an engineering school seemed more likely to provide a congenial environment for a mathematical political scientist than most other universities. I was right, and have subsequently spent my entire teaching career in institutes of technology (at least until Carnegie Tech decided to change its name and become a university).

The move by train back to Chicago was not difficult. Kathie, three months old, fit snugly into a basket. Our household furnishings came by truck. There was a serious housing shortage at this time (which became even more severe when the war ended), but we moved into the apartment the Joneses had vacated, a second-floor Pullman with three bedrooms on 57th Street near Kenwood, in the University of Chicago neighborhood. (Milton Friedman and his family occupied an apartment in the same building for many

years.) The government-controlled wartime rent was about $900 per year, not bad on a $2,800 salary.

Life at IIT

Teaching loads at Illinois Tech during World War II ranged from fifteen to eighteen hours a week, summers included, but were reduced to more reasonable levels thereafter (twelve hours). The long teaching hours left little opportunity or energy for empirical research. To supplement a modest academic salary, I took up part-time work again with the International City Managers' Association, editing, writing, and participating in training activities. A good deal of time went, also, into becoming a skillful academic politicianeven to playing bridge or billiards nearly every noon to widen my acquaintance among the faculty. As a result, I was elected to a postwar planning committee, whose activities greatly increased my understanding of university administration and finance.

My initial preoccupations in my new job are reflected in part of a letter I wrote on January 3, 1943, to Grace Knoedler, who was helping put our measurement studies through the University Press in Berkeley:

The school situation here is as uncertain as I imagine it to be at California. The consensus seems to be that, since we are a technical school, our enrollment may not suffer badly. Whether I will teach political science next semester, mathematics, or chemistry, I don't yet know.

I have had a good deal of fun this semester, especially with a course in American Political Institutions and Ideas. We spent the first part of the semester reviewing American gov't, and the rest studying the formation of the Constitution. Not particularly scientific, but I've had a chance to read some of the writings of the founding fathers.

Teaching constitutional law to engineers is, as you might surmise, not the most exhilarating pursuit that can be imagined, but they seem to absorb at least a modicum of it. My other course, Administrative Aspects of Planning, hasn't worked out quite as satisfactorily as I had hoped, largely because there is a problem of working out a raison d'être with the faculty of the architecture department. They teach these people a very Utopian variety of city planning, and I have a choice between telling the students that, legally and administratively, such planning is not possible in this country at this time, or telling them how to do a less Utopian kind of planning. You can see that this might breed both confusion and discouragement in their minds, and I have yet to find the proper approach.

With respect to teaching methods, I am neither a wiser nor a happier man. First, 3 hours a week of lectures is too much talking time for a course

if the students do any independent work outside. Yet, if the class is large, it is hard to use that time for anything but lecturing; and if the students don't do any reading, it is impossible to conduct a discussion. Thus far, I have largely refused to cover the material of the readings in my lectures. The students who dislike to read, and they seem to be numerous enough, are pretty completely thrown off the track by this, and never get back on again.

How does one instill curiosity into people? Without it, this "educational" process is a continual struggle between a student who is trying to get by, and a teacher who is trying to catch him at it, and neither of them particularly profiting by it.

Well, I don't know the answer to all this, and for occupational reasons I am going to proceed cautiously, conservatively, and traditionally for the next year or two—but not indefinitely. Maybe the New Deal in subsidized higher education will bring to us students who have some real drive and interest.

I undoubtedly wrote this letter at a moment of end-of-term despondency or cynicism after one semester of teaching, for from the beginning, I have in fact enjoyed teaching, and have been successful at it, or at least popular with my students. Except for one occasion, which I will explain in chapter 18, evaluations from my students have always been near the top of the range. The problems I mentioned in the letter, with constitutional law and planning administration, were real enough, however, as I shall explain.

At IIT in 1942 the students I enjoyed most were the co-op engineering students, who normally alternated semesters in school and in a factory. They were a couple of years older than typical undergraduates (nearly as old as I when I started teaching there) and serious about their studies, especially when they thought them relevant to their future careers.

My biggest assignment, often two or three sections each semester, was to teach these engineers "Constitutional Law," a required course. John Larkin had found it difficult to get the engineers to take an American government course seriously, and hit on law—taught by the case method, with its discipline of logic and writing briefs—as an alternative route to the goal. Surprisingly, it worked, for the students found in it something solid to chew on and digest.

In 1942, however, because of the war, the students were required to carry overloads in order to graduate as quickly as possible; and since my course was marginal to their professional goals, they began to complain about the workload. In fact, some of their engineering professors had suggested that perhaps they could persuade me to ease up in this "inessential" course. I responded by sending a memorandum to the dean of engineering and the

engineering professors stating that, if each of them were willing to reduce his assignments by 25 percent, I would follow suit. I heard no more about the matter, and had the respect from then on of both students and faculty.

In fact, we got on so well that they invited me to give a talk at their senior banquet, an affair attended by the dean of engineering and the president. Randomly thumbing through the encyclopedia, I became fascinated by the article on lemmings, and decided that those obsessed creatures provided me with an excellent topicallowing me to deliver a sermon on leadership while exercising my skills as a ham and stand-up comedian. I outdid myself that evening. Perhaps the presence of the president alarmed me enough to get my adrenaline flowing. From then on I was on President Heald's list of stars, and my academic career at IIT went smoothly.

Those mature and demanding co-op students, and the even more mature and demanding GIs who appeared after the war, taught me a great deal about teaching. I learned that there is no use lecturing to a class unless the class is listening. And they will only listen if you are saying something that they think they can understand and that seems relevant. They will listen better if you talk LOUDLY. If you pace up and down, you can tell from their moving heads whether they are following you (like the crowd at a tennis match). You can also get feedback by keeping your eye on the prettiest girl in the class to see whether she is attentive. (Unfortunately, at that time engineering classes did not have female students.)

If the students are engineers, they will better understand the logic of a Supreme Court case if you can represent it on the blackboard as a wiring diagram for an electric circuit, the switches representing the yes-or-no choices of the court. (The maze again!) The wiring diagram for the case of *Marbury v. Madison,* in which the Supreme Court for the first time declared an act of Congress unconstitutional, being rather baroque, pleased the students no end. Teaching is not entertainment, but it is unlikely to be successful unless it is entertaining (the more respectable word would be *interesting*).

Coverage of subject matter is a snare and a delusion. You begin where the students are prepared to begin; and you carry them as far as you can without losing them. Whether that takes you to the end of the specified curriculum, half as far, or twice as far, is irrelevant. You talk from notes, and certainly do not read lecturesin fact, it is better if you do not write them out. Anything you cannot communicate without reading will be forgotten instantly, and probably is not suitable for a lecture anyway.

You prepare notes for more material than you can possibly cover so that you don't suffer from the beginning teacher's nightmare: What happens if I run out of material before the class ends? (If you do, which almost never happens, you dismiss the class. They will thank you for it.) There is zero

correlation between the number of hours you put in preparing formally for your classes and their success, provided that you have a coherent general outline of the curriculum and a thorough knowledge of the subject.

You start every class by giving students the opportunity (or better, the obligation) to ask questions about their reading, about previous sessions, or about anything. You take each question seriously, and answer it without making a jackass of the student who asked it (no matter how foolish the question). After a year of teaching constitutional law, I found I was able to write a twenty-page document to distribute to students that provided answers to 90 percent of the questions they asked. I was never sure whether that was a good thing, though, for it cut down the questioning.

Of course, students don't learn by being lectured at, anyway; they learn by thinking hard, solving problems, dissecting proofs. Requiring them to write briefs was the most important component of our teaching at IIT. *After* students have thought hard about a topic, a lecture can help them sort out and organize their thoughts. Enlightenments, like accidents, happen only to prepared minds. If students have thought about something, you can discuss it profitably in class; without the preparation, it is just a bull session.

You keep lectures on the high ground. The details of proofs are better gleaned from books. Above all, you feel no obligation to repeat the contents of the textbook, for that would simply confirm students in the habit of not reading it.

During subsequent years when I was a department head, I was occasionally visited by a delegation of students complaining about a faculty member. Without exception, I believe, the real core of the complaint was that the instructor showed disdain for students, or a punitive attitude toward them, or cynicism about teaching. Students are prepared to tolerate any other form of incompetence in an instructor, but not hostility. From my teaching at Illinois Tech, I learned these and other principles for being an effective and popular teacher, and I doubt that my teaching style has changed very much since those days.

Constitutional law was not my only challenging teaching assignment at Illinois Tech. The Institute had a distinguished architecture department, which at that time held its classes at the Chicago Art Institute. The chairman of the department was Mies von der Rohe, of Bauhaus fame; and Ludwig Hilbersheimer, also from the Bauhaus, was the professor of urban planning. The students were, almost to a person, staunch disciples of Mies and "Hilbs."

One of my tasks was to teach the senior architects a course in urban land economics, followed by one in city planning administration, for both of which my textbook writing at ICMA (one of the textbooks was on planning

administration) and my research on urban property taxes in Berkeley had prepared me well. However, *economics* was a dirty word to most of the architecture students, who desired above all to preserve their profession for the expression of noble artistic impulses and to protect it from the baneful influence of money-grubbing speculators.

The architecture being taught at IIT, adhering to the International style, was usually labeled "functionalist," but I soon learned that functionalism was quite different from a concern that a building serve its intended functions. What it meant to Mies was that a building should be "structurally honest," making evident to the eye of the beholder just what function each component was performing, what load each girder was bearing. If a mullion was decorative, carrying no load, it should end several feet above the ground so that it would not appear to support anything. Whether Mies's buildings "worked," other than visually, was to a great extent a matter of accident. He did not always remember, for example, that the windows of bathrooms should be frosted or that chemistry labs might need a gravitational flow of distilled water. Functionalism for Hilbersheimer had a different meaning. All his city plans started with the prevailing winds. The city was to be designed so that, on average, factory smoke would not blow into the residential areas. I am sure there were additional considerations, but a prominent feature of all his plans was the wind rose, a diagram showing the frequency and strength of winds.

In this setting, I felt less like a teacher than a missionaryone preaching not to tolerant pagans but to true believers of another faith: I was preaching the message of Islam to devout Christians. It was challenging and exciting.

I started out with Lewis Mumford's *The Culture of Cities,* which apotheosizes the medieval city. Now, according to Mumford, the medieval city was not planned (although individual buildings often were), but grew in an "organic" way, following some laws of nature he never quite elucidates. Its beauty is not a formal, man-made beauty, but a natural one. Following Mumford's argument, some of the brighter students came to see that not all order and design come from the mind of a planner. A city can grow, and so can beauty, out of the interaction of many natural and social forces. The students could not and would not deny that the medieval city, which had developed in this way, was a thing of beauty and a joy.

With the possibility established that a plan (or pattern) did not imply a planner, the students were ready to learn that markets and prices could be organizers, pattern formers. Of course I did not try to convince them (nor did I myself believe) that natural market forces could do the whole job of structuring the functional and beautiful city. When they had learned the lesson of markets, I took them on to the concept of externalities (for ex-

ample, noxious odors wafted from the stockyards to surrounding neighborhoods), those features of real economic situations that escape the market mechanism. This provided a framework for discussing the functions and administration of local planning agencies.

I don't know whether Mies and Hilbersheimer ever learned what heresy I was preaching, for I never confronted them directly, nor, I expect, did the students. (They were much too awed.) But, however much Mies might have been dismayed had he read my Gospel, I was wholly shocked to learn the content of his—that the architect was an artist, whose task is to build beautiful buildings (or cities) either in collaboration with or in spite of the client.

Any rights of the client to determine the amount of resources to be applied to the task, or the functionality of the final structure, were not included in Mies's view. On the contrary, the client was to be educated, persuaded—I won't say *duped*—to contribute the resources necessary to produce a great work of art, as defined by the architect. The client was an instrument, a means.

Mies used to love to tell the story of how he came to build his first modern structure, the Tugendat House. As a young man, he had designed some quite conventional, late Victorian, houses near Maastricht in the eastern part of the Netherlands. The wealthy Tugendat had seen them, and wanted one like them. Here Mies would pause in his story to draw on his cigar, and you were supposed to ask how Tugendat reacted when the architect arrived with his plans for an avant-garde glass and chrome structure. "Well," Mies would say, "at first he didn't like it at all. (Pause.) But then we smoked a couple of good cigars. (Pause.) And then we drank a couple of glasses of good Riesling. (Long pause.) And then he began to like it very much."

If a certain passion creeps into my voice as I recount this, it is because my subsequent encounters with architects have taught me that this attitude was not peculiar to Mies, but is widely shared through the profession. Architects are notoriously prone to design buidings that are bid in at 40 percent over the agreed budget. And they are notoriously inept with such prosaic details as air-conditioning, energy efficiency, waterproof roofs, and all manner of other things their clients and the inhabitants of their buildings consider important.

A society as affluent as ours can afford to provide painters with just about all the canvas and paint they can use, and let them paint what they want. But no society is affluent enough to provide its architects with all the steel and glass and concrete they need to save their artistic souls. Nor should the members of a democratic society be obliged to delegate to the architectural profession the decisions that determine the comfort and pleasantness of

their daily surroundings. Architects have every right to try to educate public taste, but not to dictate it, or to intimidate their lay clients, diffident and embarrassed by their ignorance of the arts.

Perhaps that is enough of a sermon on the moral shortcomings of the architectural profession. This is an autobiography, not a tract. But how can it be complete if the author/subject has not revealed his strongest moral feelings, even his prejudices?

Teaching the architects at Illinois Tech gave me an intensive education in architecture, but also one in painting. I usually arrived an hour or two early for classes at the Chicago Art Institute, acquiring a thorough familiarity with the marvelous collection of paintings in the galleries. I counted that as a major perk of my professorship at Illinois Tech.

I experienced special pleasure, too, when a couple of the students in my class entered a citywide city planning contest sponsored by one of the newspapers and, in competition with professionals and their own faculty, won first prize for their proposals for the organization and administration of planning in Chicago.

My teaching duties at Illinois Tech during the war went well beyond constitutional law and city planning. Much of the time we were teaching students in a naval officer training program, which required them to learn about geopolitics (sophistication in which was supposed to account for the German strategic success) and contract law. So I taught those subjects as well, learning more than my students and generally enjoying it. I also had stints of teaching elementary statistics, labor economics, engineering economics (we'd call it "operations research" today), American history, and I do not recall how many other miscellaneous subjects. As a product of the University of Chicago, I had (and still have) the opinion that I should be able to teach almost any undergraduate course. I was never called upon to make good in chemistry or mechanical engineering.

During these years I was mindful of my goal of bringing quantitative methods to the social sciences. I continued my mathematics and science education by studying textbooks and working the exercises. Occasionally, I sat in on courses taught by colleaguesEli Sternberg in theoretical mechanics and Karl Menger in topology. Both were excellent teachers, and Menger's course was especially dramatic because he built it around the history of the concept of dimension, a history in which he had played a significant role. After the war, I also audited one or two graduate courses on mathematical methods in physics at the University of Chicago.

Menger, Sternberg, and I, with the aim of providing IIT students with some additional intellectual stimulation (the atmosphere was a little too vocational for our tastes), initiated a seminar in philosophy of science, which

continued for about a year. The combination of that seminar and Eli's mechanics course stimulated my first writing in that subject, a paper on the axioms of Newtonian mechanics (Simon 1947b).

Specifically, I was bothered by the sloppy way in which the concept of mass was introduced in the standard textbooks in physics and mechanics, and set out to clean things up. When I arrived at what seemed to me a more rigorous definition, somewhat related to the ideas of the Austrian physicist Ernst Mach, I wrote it down, and with Eli's encouragement submitted it to *The Philosophical Magazine,* which, in spite of its name, is a highly respected physics journal that had published some papers on the subject. My paper was accepted with almost no revision.

Although the possibility of such a connection was not at all in my mind at the time, that paper later connected up with work I was doing at the Cowles Commission (see next section) on causality and identifiability, and the junction of these topics led, in turn, to some of my most significant work in the philosophy of sciencespecifically, work on the axiomatization of scientific theories and the status within them of theoretical (not directly observable) concepts. But these ideas emerged gradually over a long period of years, and forty years later I am still refining them.

Toward the end of my stay at IIT, I had a luncheon conversation with Karl Menger that I cannot forget. He had started his career, he said, with a deep interest in logic and the foundations of mathematics. The publication of Kurt Gödel's famous Impossibility Theorem (1931) struck him a blow from which he never recovered. If it was impossible, as Gödel had shown, to provide wholly rigorous foundations for mathematics, what was the meaning of mathematical certainty? Menger never again worked on the foundations of mathematics. Even thinking about the subject depressed him, and as he recounted this story, he gradually subsided into a gloomy silence that continued through the lunch.

The Cowles Commission

During my years at Illinois Tech, my family and I lived close to the campus of the University of Chicago and had many friends there. Toward the end of the war, at the suggestion of Bill Cooper, who had come back to teach in the undergraduate college at Chicago, I began to participate in the weekly seminars of the Cowles Commission for Research in Economics. The staff then included Jacob Marschak, Tjalling Koopmans, Oskar Lange, Kenneth Arrow, Larry Klein, Leo Hurwicz, Don Patinkin, Gerard Debreu, and a number of others. Franco Modigliani, then at the University of Illinois, often

came up from Urbana for the meetings, as did Andy Papandreou from Evanston, where he taught at Northwestern. George Stigler and Milton Friedman, associated with the university but not with Cowles, sometimes participated. There were also frequent visitors from abroad, including Ragnar Frisch and Trygve Haavelmo from Norway. The list, you will note, includes no less than nine future Nobelists.

A visitor's first impression of a Cowles seminar was that everyone was talking at once, each in a different language. The impression was not wholly incorrect. Maintaining order in this group of lively minds was no mean task, and when Franco (or another) got hold of the chalk, it was not easily wrested from him. But the accents may have been more a help than a hindrance to understanding. When several speakers tried to proceed simultaneously, by holding tight to the fact that you were trying to listen to, say, the Austrian accent, you could sometimes single it out from the Polish, Italian, Norwegian, Ukrainian, Greek, Dutch, or middle American. As impressive as the cacophony was the intellectual level of the discussion, and most impressive of all was the fact that everyone, in the midst of the sharpest disagreements, remained warm friends.

At the Cowles Commission I received my fourth education in economics. The first had been my high school reading and preparation for debates; the second, my Chicago training with Henry Simons and Henry Schultz; the third, my education at Berkeley from the statistician Jerzy Neyman and from the students of Neyman and Griffith Evansespecially Kenneth May and Ronald Shephard.

One of the Cowles discussion topics closely related to what I had learned from May and Shephard was Paul Samuelson's famous essay (1941) on comparative statics and dynamics, which proposed a promising new systematic approach to the prediction of shifts in the equilibria of economic systems. Another topic, which came to be known as the "identification problem," had been introduced to me by Henry Schultz and concerned the statistical ambiguities that arise when one tries to estimate both supply and demand relations from the same statistical data. Definitive work was done on the identification problem, under the leadership of Marschak and Koopmans, at the time I was active with the Cowles Commission. My only contribution to that project, and that only after I had moved to Pittsburgh, was to show that a formal concept of causal ordering among variables in a system could be constructed, and that the causal ordering was uniquely defined precisely when the system was fully identified.

The Cowles work on identifiability was closely tied to another venture in which I had no part at all: the construction of large econometric models of the national economy, models that grew rapidly in size as computers

became available for estimating their parameters. This was the project to which Larry Klein devoted his efforts.

A fourth topic, an outgrowth of Koopmans' wartime work on scheduling oil tankers, was "activity analysis" or, as it is better known today, linear programming. The first national conference on this subject was held in Chicago in 1949. Koopmans played the principal role in developing the economic implications of the subject, while George Dantzig originated the key computational technique, the simplex method. Again, I was only peripherally involved, using linear programming techniques to investigate the economic effects of technological change.

While I was participating in the Cowles seminars, I also came to understand something about macroeconomics. John Maynard Keynes's famous book, *The General Theory of Employment, Interest, and Money,y* wholly verbal and equationless, had mostly baffled me, but now John Hicks, and especially Franco Modigliani, published mathematical models of the Keynesian system that I could understand. I came to understand just enough of monetary theory (a specialty of Modigliani and of Don Patinkin) to know that I did not understand it at all, and to suspect that other economists didn't either. That suspicion lingers with me today.

My active participation in Cowles research, as distinct from simple attendance at the seminars, was the product of another accident. To clarify questions that arose in my mind while teaching an American history course at IIT, and making use of analytic techniques I had learned from May and Shephard, I wrote a theoretical paper on the economics of urban migration, which was published in the journal *Econometrica* (1947c). At just that time, Marschak and Sam Schurr were planning a major study of the economic aspects of atomic power, to determine whether the "free energy" that everyone was proclaiming was a reality, and what the consequences of atomic energy for productivity would be. On the basis of my migration paper, Marschak coopted me to write the chapters of the study on macroeconomic implications.

If you examine the Schurr-Marschak volume, you will see that we forecast only a modest economic role for atomic power, a conclusion that was neither very popular at the time nor given much credence. We received the usual Cassandra treatment. We had looked hard for the possibility that the new source of energy might have a major "trigger effect," but our economic data and reasoning told us it simply was not there. With forty years' hindsight, our predictions still look good.

My participation in the atomic energy study had an amusing by-product a few years later. When the General Atomics Corporation was formed in La Jolla, California, in the early 1950s, some top-flight physicists on the

staff decided they should bring in a consultant to educate them on the economics of the industry. The physicist Ed Creutz, a former Carnegie Tech colleague who had joined General Atomics, recommended me for the job, and I agreed to spend a week there. It was decided that I should give a seminar on the first day and then hold court in an office where people could come to me for consultation.

In the seminar, I said essentially that there would very likely be an industry here in thirty or forty years, but that the pioneers who built it up would probably lose a lot of money in the meantime. That message (although it turned out to be an awfully acccurate one) was not cheerfully received. I spent two days in my office, but no visitors came. I waited through Monday and through Tuesday. At noon on Wednesday, I went to the harbor, rented a small sailboat, and spent the rest of the week sailing on San Diego Bay. My advice could have saved them hundreds of millions of dollars, and I did not even ask for a cut of the savings.

Friends from the Cowles Years

Among the gifts I value most from my association with the Cowles Commission are the lifelong friends it brought. Among these good friends, both those who are now gone and those who are still alive, the two of whom I think most often are Jascha Marschak and Tjalling Koopmans. By any accounting of time spent together, my relations with them were not much closer than with a number of the others. Perhaps there is another reason for my feelings about these two: They were of ages to be to me a father (almost) and an older brother, respectively.

But I prefer to think that my feelings about Jascha and Tjalling have something to do with their remarkable qualities. They were Europeans. Like my father, they pursued their professions with intense seriousness, but without neglecting other parts of life. They were intellectually curious about all things. The health of the body politic was important to them. Their quiet humor floated above a deep pool of serious concern for the human condition. There was a great gentleness in both of them.

In the early 1950s, when I was on a faculty recruiting trip from Pittsburgh, I had dinner with Marschak one evening in the Quadrangle Club at the University of Chicago. The conversation turned to the selection of faculty. As he had assembled a spectacular group of stars in the Cowles Commission, I asked him what qualities he looked for in selecting staff. "Oh," said he, "I pick people with good eyes." I stared at him. Good eyeswhat could he mean? I told him he was joking, but he insisted: He looked at their eyes.

And then I began thinking of the clear dark Armenian eyes of Arrow, the cool blue Frisian eyes of Koopmans, and the sharp black Roman eyes of Modigliani. It was certainly true that they all had remarkable eyes. Ever since, I think I have included that among my own selection criteria; intelligence shines through the eyes.

Much later, just a year or two before his death, I visited Jascha in his Los Angeles home, in the hills on Tiger Tail Lane. We took a long walk together, slowly because he was already frail. We engaged in our usual debate, he expressing his permanent faith in human optimizing rationality and I defending a bounded rationality point of view. We were revisiting old territory, yet he expressed no impatience at my intransigence and listened thoughtfully to my arguments. Debate with him was always like that: thoughtful, neither wavering nor stubborn, considerate. In fact our views were very close, for he was regarded by the profession as almost as much a renegade as I, but he never lost his belief that the limits of rationality must find their place in a broader frame of optimization. Best of all, his intelligence was as keen and flexible as it had been when we first met, thirty years earlier.

On a third occasion, midway between these two others, I behaved badly. On a visit to New Haven, Connecticut, where the Cowles Commission had moved, I was invited to dinner at the Marschaks. During the cocktail hour, to stir up the conversation a bit, I proposed the following hypothetical situation. Suppose that computers have developed to the point where they can be raised just like children and can acquire human culture, the only difference between computer children and flesh-and-blood children being that the former are less susceptible than the latter to physical and mental disease. A referendum is to be held to decide whether future generations are to be computer children or flesh-and-blood children. How would you vote?

The room became very quiet and very cold. Jascha's wife, Marianne, the warmest and most cordial of human beings, was obviously angry. I thought I was going to be turned away without dinner. Jokes that challenge the basic values of humanity are not funny, especially if they skirt too close to the human values rejected by Nazism, with its medical experiments and gross disrespect for human life. I did not intend my question as a challenge to values but as an opportunity to explicate them. But I had obviously touched a deep and serious nerve. I hastened to back-pedal, and I did get my dinner.

Moral concerns were also an important part of my relations with the Koopmans. My last conversation with Tjalling was heartbreaking. He had just had a serious stroke, and had only partially recovered his memory and speech. We sat in his home in New Haven through an afternoon and con-

versed about many matters. He never lost his patience or his calm while groping for thoughts and words. I could vividly imagine myself in the same position, lashing out at the world around me, blaming it for my disability. But calm fortitude was of his essence. It would not have been a conversation with Tjalling if he had behaved in any other way.

Several years before that last conversation, we had decided that he and I with our wives would go off somewhere for a few days to talk about the conditions for peace in the world. It was Tjalling's suggestion, not because he was unrealistically sanguine that he or we could change the world, but because he thought it was everyone's responsiblity to have carefully thought out views on such problems and to contribute toward their solution, even if that contribution could be only an epsilonor perhaps just an expression of good faith. So we went to the Poconos, and spent a couple of days enjoying each other's friendship, walking in the mountains, and looking for conceptual paths that led away from the Cold War.

I am afraid that I have made the Marschaks and the Koopmans sound like solemn peoplethey were not. They were warm human beings with a talent for friendship, and my memories of afternoons and evenings with them and our mutual friends are memories of funthe serious kind of fun that only committed intellectuals who take the world seriously can have, but also the unself-conscious kind of fun of a congenial company.

My professional meeting ground with Tjalling was economics and econometrics. And on that ground we found the most profound difference between us. While we were both committed to "hardening" the social sciences with the help of mathematics, mathematics meant something entirely different to Tjalling than it did to me. I discovered thismuch to my amazementat a dinner at our house when the Social Science Research Council (see page 172) held a conference in Pittsburgh, on Expectations and Uncertainty. The year was 1953. The encounter so startled me that I remember exactly where I was standing during the conversation: facing the living room fireplace, while Tjalling had his back to it.

For me, mathematics has always been a language of thought. I don't know precisely what I mean by that (and explicating the meaning is today one of my important research goals), but I can try to explain. When I am working on a problem, I am sure that I do not usually think in words, but in terms of a more abstract representation that is perhaps partially pictorial or diagrammatic and partially symbolic. Mathematicsthis sort of nonverbal thinkingis my language of discovery. It is the tool I use to arrive at new ideas. This kind of mathematics is relatively unrigorous, loose, heuristic. Solutions reached with its heir have to be checked for correctness. It

is physicists' mathematics or engineers' mathematics rather than mathematicians' mathematics.

For Tjalling Koopmans, it appeared, mathematics was a language of proof. It was a safeguard to guarantee that conclusions were correct, that they could be derived rigorously. Rigor was essential. (I have heard the same views, in even more extreme form, expressed by Gerard Debreu; and Kenneth Arrow seems mainly to share them.) I could never persuade Tjalling that ideas have to be arrived at before their correctness can be guaranteed, and that the logic of discovery is quite different from the logic of verification. I am sorry that he did not live to read and comment upon my recent work on the logic of scientific discovery. Perhaps we could have built a bridge across what seemed a great gulf that separated our attitudes toward mathematics. It is his view, of course, that prevails in economics today, and to my mind it is a great pity for economics and the world that it does.

Beginnings of Decision-making Research

The association with the Cowles Commission did not diminish my preoccupation with decision making, but turned part of my activity on that topic to new directions and brought me into the thick of the dramatic intellectual developments that took place in the social sciences just after World War II. The excitement of the time can be conveyedor re-evoked for those of us who lived through itby listing the labels for constellations of ideas that were born then: operations research and management science, the theory of games, information theory, feedback theory, servomechanisms, control theory (these and others collected under the banner of cybernetics), statistical decision theory, and the stored-program digital computer.

The ideas were all closely intertwined, with decision making at their core, and they quickly generated a scientific culturean interlocking network of scientists with a real sense of community, which was almost independent of the special area in which each worked, and which ignored the diversity of their backgrounds and training. They came from physics, statistics, economics, biology, mathematics, engineering, philosophy, and even a few from psychology and political science. (In chapter 12 I will give a fuller account of the *zeitgeist* of this period, in discussing the historical origins of artificial intelligence and cognitive simulation.)

My dual participation in the engineering culture of Illinois Tech and in the econometric culture of the Cowles Commission gave me early access to this world. I learned of the theory of games before John von Neumann and

Oskar Morgenstern's book, *The Theory of Games and Economic Behavior* (1944), was published, then spent most of my 1944 Christmas vacation (days and some nights) reading it. I wrote what I think was the very first review it received.

When the wartime security wraps were removed from computers, my earlier experiences at ICMA and in California with IBM plugboards made it easy to see their immense potential. Statistical decision theory had already formed part of my graduate training, and I had published a paper on the topic during the Berkeley period. The operations research techniques formed a natural continuity with my administrative measurement research. Through the neuroscientist Gerhard von Bonin, who lived in our apartment building, I met the legendary psychophysiologist and systems theorist Warren McCulloch and, through the Cowles Commission, the great mathematician John von Neumann.

Return to Engineering

After Dorothea and I returned to Chicago in 1942, we saw my parents fairly frequently. Milwaukee was only two hours away, and there were first one (1942), then two (1944), then three (1946) grandchildren. During these years my relation with my father grew closer than ever, as we were able to share many of our professional interests. I was very pleased when he invited me to his beloved Professional Men's Club, a weekly luncheon club representing the whole range of professions, to talk about the atomic energy study, and on another occasion to talk to the Milwaukee Engineers' Society.

Shortly before my father died, in November 1948, I made a discovery that moved me deeply. It had never occurred to either my brother or me to follow his profession of engineering. The reasons are obscure. Had we been immunized against this inheritance by his own abandonment of the Rhineland vineyard? I don't know. It gradually dawned on me that the path I was following in my professional maze was returning me to the paternal calling, and not only because I had chosen to teach at an engineering school. As a designer of control gear, my father had been a significant contributor to the development of feedback devices. Now I was beginning to think of feedback theory as a tool for modeling the dynamic behavior of economic systems and organizations.

In one of the last letters he wrote to me, my father sent me some references I had requested on servomechanisms. Soon, I was using servo theory (now usually called control theory) in papers on inventory control and production planning, and was able to make contributions to the theory. Twenty years

later, I took great pleasure in printing in *The Sciences of the Artificial* (Simon 1981) a drawing of a servomechanism my father had patented in 1919, just three years after my birth.

And in the 1980s, when I was elected an honorary member of the Institute of Electrical and Electronic Engineers, and subsequently received the Harold Pender Award from the Moore School of Electrical Engineering at the University of Pennsylvania, I decided that I had been a closet engineer since the beginning of my career.

Administration Again

After four years without administrative chores, in 1946 I accepted the chairmanship of the department of political and social science at Illinois Tech, the beginning of about twenty-five years of departmental and deanly administrative duties. But *accepted* is not strictly accurate. I told John Larkin that I wanted the job, and he finally overcame his reluctance to appoint someone so young and unknown. Don Smithburg and Victor Thompson joined the department, and together we laid plans for our textbook, *Public Administration,* which appeared in 1950, and for a professional Master's program in public administration at IIT. Since we did not have time to get off the ground a program of empirical research in organizations, I was able to return to empirical and theoretical studies of administration only after I moved to Carnegie Tech in 1949.

Fair and Unfair Competition

If we are to account for my path through the maze during my professional years, we have to descend from a level of high principle to some more worldly concerns. However important my liberal values have been to me as a human being, we have seen that they had only a tangential impact, and a minor one, on my professional career.

When I arrived at Illinois Tech in 1942, I was still quite uncertain about my long-range career plans. As the war began to wind to a close, I had to think about whether I would remain at IIT or look for other opportunities. I did not look very actively, because I enjoyed my job and my associates and believed that Illinois Tech, under the leadership of President Henry Heald, had good prospects for the future. My salary was adequate—by 1946 I was earning $4,600 as an associate professor—and we lived com-

fortably in our apartment near the University of Chicago, with our three children.

When the University of Chicago approached me, I was happy to discuss possibilities with them, but I was not tempted by the offer of an assistant professorship, involving a reduction in rank. With the publication of *Administrative Behavior* the next year, IIT promoted me to a full professorship at a salary of $6,000. We felt almost wealthy. Then the University of Illinois, which was expanding its graduate school, offered me a professorship in public administration at a still higher salary.

I was tempted until I visited the campus at Urbana and learned that the Department of Political Science was wholly uninformed about these new plans of the graduate school. I was not looking for that kind of academic political fight, and it was well that I was not, because the whole graduate school initiative collapsed a year later under a major attack from the Illinois legislature. During my negotiations about the position, however, President Heald again agreed to a substantial salary raise.

This was the last time in my career that I sought out a job opportunity, even semi-actively. If people approached me, as they did on a number of occasions after I moved to Carnegie Tech, I listened and deliberated, but my answer was always in the negative. After I had made and communicated my decision, I let the university know I had had an offer, but I never bargained about my salary.

This did not amount to either naïveté or charity on my part. I knew that my talents were highly marketable and that my university would have to keep my salary at the market level if it did not want to see me wooed away. Perhaps sharp bargaining could have inched my salary a little higher, but the time spent in bargaining would not have yielded much of a return. So at a very small price in dollars, I have earned brownie points for not haggling, and avoided lots of stress.

Two other considerations have been of much greater importance than money for the path my career took: my attitudes toward competition, and the criteria that, at choice points, directed me down one path rather than another. This is perhaps a good place to review the rules of play, as I conceived them. The record makes clear that I have been, and am, a competitive person, and in addition to the intrinsic satisfactions of academic work, I have never been insensitive to the implicit competition with others that pursuit of a career entails.

A highly competitive person has a hard row to hoe. There is no satisfaction in winning a competition unless it is a stiff and fair one. *Stiff* is easy to define; it is stiff if one's own realistic assessment of one's abilities make the odds longthe longer the odds, the greater satisfaction on winning. *Fair*

is harder to define, for if one wins a contest against long odds, there must be a reason. The odds weren't really long; they only appeared to be so. Isn't it unfair to appear to be an underdog when one really isn't? Let's start with some obvious distinctions: A professional gambler needs to win in order to earn his living. Fairness is not his concern. He tries to be unfair in various ways: Keeping cards up his sleeve is one way that the rest of us universally deplore; the morality of concealing his skill to attract dupes is hardly less questionable. *Fairness* means at least an honest deal (no hidden cards) and no intentional concealment of one's abilities.

How do these criteria apply to the life of science? I advise my graduate students to pick a research problem that is important (so that it will matter if it is solved), but one for which they have a secret weapon that gives some prospect of success. Why a secret weapon? Because if the problem is important, other researchers as intelligent as my students will be trying to solve it; my students are likely to come in first only by having access to some knowledge or research methods the others do not have.

For example, in tackling the problem of understanding human thinking, which will be the topic of chapters 13 and 14, the secret weapon that my research partners, Al Newell, Cliff Shaw, and I had was access to a digital computer, and an ideaderived from contact with computersthat it could be used as a general processor of symbols. The computer and the idea were not available to Gestalt psychologists who otherwise might have written the first programs for heuristic search. We were very pleased in the spring of 1956 when we realized that we had won this competition, that we were the first to explicate the symbolic processes that enable people to think and solve problems. But hadn't we been unfair to take advantage of our private knowledge and our private access to computers? What merit was there in winning such a one-sided contest?

One can see from this example that "fairness" in science is a rather strange, even arbitrary, concept. It isn't unfair to be smarter than other people (but be sure you aren't deceiving yourself!). It isn't unfair to work harder than they do. It isn't unfair to happen to know relevant things that they don't know. It isn't even unfair to happen to have the most powerful piece of equipment in the world.

Nevertheless, in the contests we design for ourselves, we always have in mind some implicit criteria of fairness, and our victory is spoiled if the criteria are violated. As a boy, I used my intelligence to win in academic competition, but somehow felt superior to those who reached the same scores with less intelligence but by dint of more effort. Hence, when I was not valedictorian in my high school class, but graduated third (and shortly thereafter was also third in the University of Chicago Freshman Week ex-

aminations), I was not bothered by my ranking, for I knew that I had worked much less hard than my victorious competitors. They were overachievers!

In my subsequent career, I have certainly had no aversion to being a workaholic, and have not enjoyed my successes less for having worked for them.

In the high school environment, where bookworms were not suffered gladly, it was not "fair" to win by studying harder. In the world of science, with no holds barred, overachievement was the normal route to success. Nonetheless, I have probably never quite gotten over the "Look, Ma, no hands" syndrome. Success is especially pleasant when it is effortlessbut it seldom is. To save appearances, one simply redefines work as fun (which, unaccountably, it usually becomes).

But then, how about the long oddsthe special pleasures of an underdog victory? In reviewing the record, I observe that I have always been pretty careful in setting the odds, and have usually behaved like an honest professional gambler, if that is not a contradiction in terms, taking my advantages where I could find them, never eschewing a (legitimate) secret weapon that I found at hand. In giving up thoughts of a possible political career, I felt that being a nonveteran and a Jew was too much of a good thing as far as underdogging was concerned.

In one respect, however, I have quite consciously played the underdog. I have never believed that I had to be at Harvard or Stanford or M.I.T. to win the academic game. While I was a student at the University of Chicago, the university still played Big Ten football, although it rarely won a game. When one member of the team, Jay Berwanger, nevertheless made All-American during a season in which every game was lost, there was no doubt that his achievement had special luster. It was a personal achievement that owed nothing to the organization he belonged to.

Although I don't believe I consciously thought of it in this way, that was my ideal: to win without conspicuous social support, whether from family or university. Then it would be certain that I had won "fairly," and not just by using the hidden, or not so hidden, weapon of a superior environment.

I was exceedingly reluctant to leave Illinois Tech even when tempted by schools of greater reputation, and when I finally did leave in 1949, I felt a little disloyal in abandoning the challenge of helping raise IIT in the ranks of academe.

Fairness is a tricky concept. Evidently it is not unfair to win the raffle of the geneswith respect to either intelligence or industriousness. It is not unfair to have the experiences or to be at the places that provide one with a secret weapon. It is unfair to inherit merit from the prestige of one's family

or organization. Put this way, the distinctions seem highly arbitrary, and I am not prepared to defend them. I simply report the rules of the game that guided my career and conditioned my feelings of success in competition. As it turned out, they provided me with an enjoyable and winnable game.

Gravitating toward the Sun

Speaking of competition puts one in a Darwinian frame of mind. How does a career, and especially the choices made along its course, look from a Darwinian point of view? What is a Darwinian maze?

Fitness is the central concept in modern Darwinian genetics. It is measured simply by the rate at which an organism reproduces itself. If two organisms compete for occupancy of the same ecological niche, relative fitness determines which will survive. Even small differences in fitness can lead to enormous differences in reproductive success over only a few generations.

Replace competition between two populations by choice between two alternatives, and replace fitness by any measure associated with the outcomes of the choices. Then if one is confronted repeatedly with choices of some kind (for example, whether to take a second helping), slight differences in the probability of selecting one alternative or another can soon produce substantial differences in outcome (body weight). All of us are familiar with this particular example.

The win or loss of a chess game may be decided in the same incremental way. Especially when good players are nearly equal in skill, the game is seldom lost by a single bad move. Rather, the winning player secures a cumulative advantage by exploiting successive small weaknesses that are observed in the opponent's choices of moves. We can express the better player's advantage by a probability, slightly greater than 0.5, that in each pair of moves that player's will be the stronger.

For most of us—those of us who have not won million-dollar lotteries, or suffered sudden crippling accidents—life is much like the chess game. We make hundreds of choices among the alternative paths that lie before us and, as the result of those choices, find ourselves pursuing particular, perhaps highly specialized, careers, married to particular spouses, and living in particular towns. Even if we point to a single event as the "cause" of one of these outcomes, closer scrutiny of the path we have trod would reveal prefatory or preparatory events and choices that made the occurrence of the critical event possible.

This kind of biased random walk describes very well my own choice of occupation. The bias, though perhaps slight, gave it direction. I expect that

I was always ambitious to contribute to the solution of central problems in science, but I started out my career with little knowledge of the geography of science or my location in it. During my period of study at the University of Chicago, I did tend to home in on the Big Questions: I read the Great Books assiduously, I found Whitehead and Russell, I dug into the work of Walter Pitts and Warren McCulloch (1943) on the application of Boolean logic to nerve networks and Claude Shannon on switching circuits (1938) as soon as I learned of it.

Mechanical calculators and IBM punched-card machines fascinated me, and I became knowledgeable about them and used them. I tried to understand special relativity theory, and studied differential geometry as a preliminary to tackling the General Theory. I read von Neumann and Morgenstern's *The Theory of Games and Economic Behavior* (1944) within weeks of its publication, and stayed up all night to finish W. Ross Ashby's *Design for a Brain* (1952). My nose was clearly sensitive to where the action was, and all my choices were slightly biased toward the Main Chance. The University of Chicago had done an excellent job of developing my scientific tastes.

But my actual research career started in an academic backwater: public administration. However important that field was and is to public affairs, it attracted few scholars with a real understanding of what research is all about, or of how to construct theoretical foundations for an applied field. Viewed by the norms of science, many of the books published in public administration (and management generally) are positively embarrassing. For these and other reasons, this field was nearly invisible to mainstream social scientists. Even if a researcher made a contribution with potential beyond administration, it was unlikely that it would be noticed by anyone outside the field.

My case was even worse. I spent my first three working years (1936 39) largely on very practical tasks at the International City Managers' Association. Mary, my first college sweetheart who once visited me in Chicago long after our affair had ended, expressed amazement that I should be devoting my life to such trivia. I replied that it was a job, and a pretty interesting one at thatand that sooner or later I would probably finish my degree and go into academic life. But I certainly had no plan for the transition.

How, then, did I find myself within a few years placed strategically in the social sciences, to be noticed by and to influence virtually all of them? The nose helped a lot (as it did Tolman's rats in their mazes). When I did sense something exciting and fundamental, I sniffed my way toward it and became involved in it almost without plan or forethought. Earlier, I men-

tioned the weekly salon that Dorothea and I had established before we left Chicago in 1939, and which gave us strong connections with the early protocyberneticists whose work exuded excitement and significance.

Similarly, at Berkeley I gained contact with the renowned statistician Jerzy Neyman and with students of the economist Griffith Evans. (I never met Evans personally. Many years later, I addressed an inquiry to him, and received a postcard reply: "I am over eighty years of age and trying to complete some of my own research, so I am afraid I do not have time to answer your question." A good sense of priorities.)

I was neither a name-dropper nor a celebrity seeker. I generally had little contact with the Great Men, but absorbed their influence indirectly through my contemporaries who were their students. I was much too timid, usually, to approach the Great Men themselves. I have recounted already the one real out-of-class contact I had with Carnap, and I met Rashevsky outside class only many years later, at the party at the Marschaks I have mentioned. I do recall that, with some other students, I once accompanied Bertrand Russell to his apartment while he was visiting Chicago, and I later had contact on a number of occasions with von Neumann. But my list of celebrities collected while I was a young man is not long.

This reticence has persisted. I have seldom been able to cultivate an acquaintance with someone, however important and useful it might be, unless we were brought together by some working or social relation without initiative on my part. Even today, when I have numerous routes of access to power, I exploit them very littleat least by comparison with others whom I observe around me. I do not know whether to classify this as a virtue or a vice. It certainly is not a considered policy, but a necessity for comfortable living.

No doubt it has something to do with vanity, too. I prefer being asked to asking, whether it be a matter of jobs, research grants, professional talks, or anything else. A strong distaste for the prospect of being rebuffed has probably made me considerably more platonic in my relations with women than I might otherwise have been. So perhaps mine is a rather useful vanity masquerading as decency.

But to return to the subject of gravitation, my first important movement from the recesses of outer space toward the sun was my association with the Cowles Commission, which was at the very center of the new postwar developments in mathematical economics and econometrics. Moreover, Cowles had close ties with the RAND Corporation (an acronym for Research and National Development), the original Think Tank, located in Santa Monica, and largely funded by the Air Force. RAND was well keyed into the early developments in cybernetics and computing.

There is a Russian folktale about the peasant from a distant village who encounters a friend on Red Square, just outside the Kremlin in Moscow, and asks, "Why, Ivan Ivanovich, what are you doing here?" "Oh," he replies, "I came to see and to be seen." For centrality to the postwar quantitative social sciences, the Cowles Commission and the RAND Corporation were definitely the places to see and to be seen. My presence in these places made *Administrative Behavior* visible not merely to scholars in the discipline of public administration but to others, as well, who could sense how crucial decision processes are to explaining human rationality. Thus *Administrative Behavior* did not languish in its provincial homeland, but was noticed by economists and decision theorists.

It was also noticed by sociologists and the new community of behavioral scientists christened and nourished by the Ford Foundation's program. My visibility to that community was probably due to the salience of the Chicago Political Science Department. *Administrative Behavior* became one of the type specimens of Chicago political behavioralism, for both its friends and its enemies. Among the first to take note of it were the Chicago sociologist Edward Shils, who mentioned it in his pamphlet *The Present State of American Sociology* in 1948, and Bernard Berelson, the Ford Foundation's behavioral science honcho, who sought my advice (along with that of many others) on the Foundation's program plans.

When I moved to Pittsburgh in 1949, I retained my close ties with the Cowles Commission and with RAND, and by 1952 had become a RAND consultant, spending several summers at its offices in Santa Monica. As I will discuss, I also became an adviser to Berelson on the proposed Center for Advanced Study in the Behavioral Sciences.

The Ford connection soon led to participation in the affairs of the Social Science Research Council, and later the National Research Council. So, however peripheral research on public management and on organizations was to the central themes of the social and behavioral sciences, from about age thirty I was as visible as a young man could wish. From then on there was no question that my work, if worth noticing, would be noticed. Wandering in the forums of the Cowles Commission, RAND, the Ford Foundation, the Social Science Research Council, and the National Research Council, I would see and be seen.

Chapter 8
A Matter of Loyalty

On April 3, 1948, the U.S. Congress approved the Economic Cooperation Act (ECA), implementing the Marshall Plan for bringing about the economic recovery of Europe, then in a desperate condition and threatened with the prospect of Communist revolutions. Some four months later, the Economic Cooperation Administration was a going concern with a viable program. I had the good fortune to have a grandstand seat at these events.

Don Stone, whom I had known and briefly worked for at the Public Administration Clearing House, had since 1941 been director of the Administrative Management Division of the U.S. Bureau of the Budget. He had several times offered me positions in his division, but I had declined, preferring an academic career. I did, however, frequently serve as a consultant to the Bureau.

Don was selected by Paul Hoffman, a former automobile manufacturer who had been appointed by President Truman as administrator of the ECA, as his staff man for organization. Don invited me and a few others to come to Washington for some months to help organize the agency. I took several extended trips to Washington in the spring of 1948 and spent the entire summer there, first holding a position as consultant, then as director of the Management Engineering Branch of ECA.

As I have told the story of how ECA came into being in an article that is reprinted as chapter 16 of the third edition of *Administrative Behavior,* here I will limit myself to some of its personal aspects.

When we began, there was no agency, just a few desks and telephones, and a telephone directory that grew rapidlyfrom 15 names on April 13 to 741 names on July 26. Classical organization theory would have called

for us to draw up an organization chart, with divisions and sections and branches, and a manual describing the functions of each. There was no time for that. Instead, we focused on drawing up a mimeographed document, *Basic Principles of ECA Organization,* that described the mission of ECA, selecting and emphasizing one particular point of view of the many that were competing for hegemony. There were at least six important alternative approaches to conceptualizing ECA, and we blended those that seemed best to us while playing down the others.

Our policy document never received any official approvalthat would have taken months and much compromisebut it was widely circulated, giving each new person who entered the organization a specific picture of what it was about. That picture emphasized negotiating with a unified Europe through the Paris office (rather than bilaterally, with the individual European countries), and placed the balance of payments in the center of agency planning and budgeting. This conception of the program served as both a weapon and a motive for the competitors in the power struggle that went on in this, as in every, burgeoning organization. Units that fit the conception could use it to claim a larger place in the program, and the administrators of those units were led to see the broadening of their functions as essential means for implementing the ECA program.

The Management Engineering Branch had one other arrow in its quiver. Before the Personnel Division could fill a position with a permanent employee, we had to provide a formal job description. By setting our priorities properly, we made it easy for units that fit our conception of how the ECA should operate to hire employees, and very difficult for the others. We used that power discreetly but vigorously.

I do not want to exaggerate my influence, or the influence of the Organization and Management Division in general, over the shape that ECA and its programs took. In the long run, in this as in many other situations, it was mainly the nature of the task to be accomplished and the pressure of the task requirements on the organization that shaped the agency. But perhaps we helped to start it in a good direction and to speed it on its way. Whether or not we were influential, it was a most educational experience for me.

By 1948, Communists and supposed Communists were being discovered under every rugbeginning with William Remington and followed soon after by Alger Hiss. Any graduate of the University of Chicago, with its reputation of tolerance for campus radicals, was guaranteed a full field investigation before he could obtain a security clearance. The ECA Security Office found me a highly questionable character, and gave me my clearance with great reluctance. To explain how this came about, I must give a fuller

account of my history as a liberal activist, and my history as an investigatee of security agencies. The story begins before the period we are now considering, and continues long after, but it will be more easily understood if it is told in one piece.

Liberalism, Depression Style

At what age I became a civil libertarian is not recorded. While yet in grammar school, I had a letter published in the *Milwaukee Journal* in defense of atheism. I was indignant that my father forbade my doing such a thing again. After all, I had signed my own name, and the fact that some people, misled by the common address, had confused "Herbert" with "Arthur" was no fault of mine. Nonetheless, I did abstain from writing further letters of this kind.

At school and church, I was a nonconformist in certain minor ways, and learned to bear the embarrassment that nonconformity brings with it. By the time I reached college, I was more or less a Socialist, and became aware that socialism and communism were often considered indistinguishable, and that communism (and supposed communism) was very little tolerated by society, especially as represented by the Chicago police. One of my dormitory fellows spent a night in jail because he attended a meeting that was raided. Whether it was literally a party meeting or just some sort of fellow-traveling protest I don't remember. I conversed with Communists on the campus, and argued with them, but never had an impulse to join them. I was (and am) a New Deal Democrat, probably imprinted by Franklin Delano Roosevelt's inaugural address.

When Bill Cooper and I and some others (including Dorothea) formed a Progressive Club on campus in the autumn of 1937, we were noticed by the monthly campus magazine, *Pulse,* which reported our activity in such an atrocious imitation of TIMESTYLE that I cannot resist quoting from the article:

> Testimony to the progressives' characters and purposes can be found in no better source than their potent faculty members, all active, who exuberated in delight at discovery of students who thought by themselves, for themselves. Faculty men were pleased to allot time from already crowded hours to furtherance of cherished ideals which had at long last found untinged company in a campus organization. Understandably proud therefore was President William J. Cooper in announcing traditionally hard-to-get faculty mentors Charles

Merriam, Paul Douglas, Jerome Kerwin, Malcolm Sharp, Edward Levi, as charter members, smilingly adding that there were more to come.

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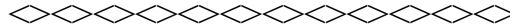
Nearest approach to pinko viewpoint is found in advocation "(5) Such legislation as will protect the consumer, aid the underprivileged, promote economic stability, provide more adequate support for education, operate for a more equitable distribution of wealth, and reduce the dominance of vested interests over the economic life of the nation."

More significant than their pros are the Progressives' consopposition to: any form of fascism or communism; undermining of the democratic process; use of coercion and violence; sabotage; instigation of class warfare; fomentation of animosity with view to revolutionary upheaval.

While they haven't yet been pestered by Communists, whose platform calls for active participation in all "progressive parties," an ejection provision in their constitution will care for such as these. Meanwhile they concentrated on action.

The Progressive Club focused most of its attention on local government (Bill Cooper, Dorothea, and I were all working in that domain), which in Chicago at that time was maximally corrupt. Dorothea and I also joined a newly formed organization, the Hyde Park Independent Voters, to support a reform, antimachine candidate for alderman of the 5th Ward. We canvassed our precinct from door to door, were generally received genially and even warmlyand earned a total of fifteen votes for our candidate in the election. We had to conclude that we were not very effective canvassers, but we look back on the experience with fondness, for the Hyde Park Independent Voters later became the Independent Voters of Illinois (IVI), which ultimately merged into Americans for Democratic Action, of which we are therefore prenatal members.

Like most student organizations, the Progressive Club never grew to any size, and most items on its agenda never progressed beyond the grandiose statements in its constitution. But the club did not represent my total activity as a liberal at that time. While I was working for Clarence Ridley at the City Managers' Association, some of the employees in the Public Administration Clearing House (PACH), of which this was one unit, decided to form a union. I was generally sympathetic to unions, but had never thought of myself, an intellectual and white-collar worker, as a prospective union member. But when I learned that Louis Brownlow, director of PACH, was greatly alarmed by the notion of a union and was actively campaigning against it, I immediately decided that if a union were formed I would surely join it. The union never materialized, so it was a tempest in a teapot. I mention it to illustrate my touchiness on matters of civil liberties.

During these last two years in Chicago, Dorothea and I dined very economically in an eating cooperative, the Ellis Co-op, organized on the principles developed by the Rochdale cooperatives in nineteenth-century England, hence explicitly nonpolitical. The diet was adequate, and Mrs. Polacheck cooked expertly, but by reason of budget constraints rutabagas were ubiquitous on the menu. Once every semester, she splurged by preparing a dinner of delicious blintzes.

If the Co-op was nonpolitical, most of its members were not. In particular, there was a large contingent of Troskyites, who propounded most extraordinary theories about how the war in Spain should be conducted by the Republicans ("cooperation at the front, but no cooperation at the rear" was the basic Troskyite axiom). These same Troskyites constantly tried to persuade the Co-op to adopt political resolutions about Spain and other matters. In general, we outvoted them, but it was nip and tuck.

Meanwhile, as already recorded, my close friends Leo Shields and Winston Ashley and their Trotskyite-Aristotelian-Catholic associates had taken over the Beta Theta Pi fraternity. And to flesh out our popular front alliances with radicals, Dorothea and I attended one or more discussion meetings that were obviously organized by members of a Communist cell.

Dorothea was for a time a member of the American Student Union (ASU), and I also attended some ASU meetings. The ASU was never a Communist organization but was heavily infiltrated by Communist students, sometimes controlled by them, and definitely regarded by the FBI as a Communist-front organization. In loyalty investigations you get black marks for talking with Communists, but no brownie points for arguing with them or contesting their control of an organization.

During this period, the only morning newspaper in Chicago was the *Chicago Tribune,* which was incapable of distinguishing news from opinion. When a new left-wing newspaper, the *Midwest Daily Record,* appeared, we subscribed immediately. It was soon obvious that it was essentially a Midwestern edition of the Communist Party's *Daily Worker*. We retained our subscription, however, relying on our political sophistication to interpret and discount its news stories, and much preferring it to the thick stream of bile poured out by the *Tribune*. It was not until many years later that I learned one uses one's time best by reading no newspaper at all.

Dorothea and I were intensely political animals at this time, deeply concerned about events in Europe (first the Spanish War, then the aggressions of Hitler in central Europe). By many definitions of "fellow traveler," but not ours, we were fellow travelers, not accepting either Stalinism or Trotskyism, significantly more concerned about the dangers from the Right than the dangers from the Left, but quite aware of both dangers.

We believed that a French-Russian-English military alliance was essential to the safety of democracy in Europe, and migrated from our natural pacifism to a strong interventionist position for America. When in doubt, we could determine our policy by looking at the *Chicago Tribune* and opting for the opposite position.

The situation in Europe was becoming steadily more grim as Hitler stepped up his demands for the incorporation into the Reich of Memel, Danzig, Austria, and parts of Czechoslovakia. In April 1938 there was a great blizzard in Chicago. Dorothea and I struggled through the drifts to our respective offices, she to the Political Science Department and I to ICMA. At noon, a caterer was persuaded to bring some food into our building so that we would not have to brave the elements, and a group of us gathered together in the commons room at the Public Administration Clearing House, where ICMA was located. When someone turned on the radio, we were assaulted by the high-pitched tones of an enraged Hitler denouncing the president of Czechoslovakia. *"Benes, der Lüger!"* ("Benes, the liar!") I remember the phrase, and the hate in Hitler's voice as he uttered it.

It was too much for me. I put on my overcoat and trudged the half-mile through the snowdrifts to the lake shore at the foot of 57th Street. It had stopped snowing, but the strong northeast wind was driving blasts of spray from the half-frozen lake against the great blocks of ice that had piled up along the beach. The battle of wind with ice seemed to echo the battle of wills that was going on in Europe. I stood watching the struggle for a quarter of an hour, my face to the wind, then turned and walked back through the snow feeling somewhat calmer.

Even though it was entirely clear that fascism was the enemy, defining one's political position was not easy during this period. The Stalin-Hitler pact in the summer of 1939 completed our disenchantment with the Stalinists. The political trials in the Soviet Union during the 1930s had already caused us great concern, but we were unsure what they meant.

As late as our stay in Berkeley, from 1939 to 1942, I spent hours in the library stacks reading transcripts of the trials, unable to comprehend them. Why did the victims confess so abjectly? What were they really guilty of? The answer came to me only with the publication of Arthur Koestler's *Darkness at Noon* (1941). I picked his book up one afternoon and did not put it down until I reached the last page at dawn. Then I understood.

"Subversion" in California

Our associates at Berkeley, primarily the staff of the Bureau of Public Administration and the Political Science Department, were nearer the middle of the road than were most of our Chicago friends. Milton Chernin, my closest associate, was active in liberal politics in California and suspected by the authorities of Communist connections if not party membership. In fact, he was a liberal Democrat, and if he had ever been further to the left, I never had evidence of it.* "Liberal Democrat" was the label that fit most of us.

During the three years at Berkeley, my little research group carried out three major projects that have already been described, one of them a large-scale field experiment in the California State Relief Administration. The study was authorized by the state office of SRA, where Chernin had earlier been director of research. In the two field offices in Los Angeles that were turned over to us, we set up a careful experimental design, which was to govern the operations for three months.

At the same time, a great political donnybrook was going on at the state level, so that we more than once arrived at one of our district offices on a Monday morning to find that the director had been fired. We would then proceed down to the state headquarters, also in Los Angeles, pound on the desk, and demand (and get) the director's reinstatement.

The political situation was so confused that no one knew what authority we had, and those momentarily in charge assumed we would not be so peremptory if we had none. Hence, they always backed down, and our experiment ran through its rocky course to completion. One of our allies in the state office, a former assistant to Chernin, also subsequently turned up on the FBI lists as a suspected Communist. Meanwhile, on more than one occasion, I had written reference letters for him and for Chernin, as I certainly ought to have done.

After a time, we discovered in one of our two experimental districts a very active Communist cell, complete with a mimeograph machine. The members were good case workers and nice people, who later got into deep trouble and ultimately into prison as a result of perjury or contempt con-

* Chernin did like to tell the story of the Battle of El Cerrito Hill, in which General David Prescott Barrows, sometime chairman of the Berkeley Political Science Department, and later president of the university, surrounded the hill of that name, near the Bay, with police and National Guardsmen, and led a charge up it at dawn, having been advised that strikers were gathered there to launch some kind of revolutionary action. At the top they found two hoboes, lighting a fire to cook their meager breakfast. I don't know whether this event actually occured,but Chernin's delighted telling of it on many occasions could not have endeared him to the authorities.

victions (I don't remember which) for their testimony before a Red-hunting committee of the California legislature. Our only political entanglement with them was to attend a lively party they gave just when the agency was going into final collapse toward the end of our study. They all expected shortly to be unemployed.

So much for radicalism in the Los Angeles offices of the SRA. Back in Berkeley, the statistician on my project was leaving for another job, and I had to hire a replacement. (Actually, my boss, Sam May, was supposed to hire the replacement, but it never occurred to me to bring him into the act until I had made the decision and needed a signature.)

The mathematician Griffith Evans recommended two of his doctoral students for the job: Kenneth May, who was Sam May's son, an excellent mathematical economist; and Ronald Shephard, who was more of a statistician, and worked closely with Jerzy Neyman. After conversations with both, I chose Shephard, mainly because I thought it slightly awkward to hire the boss's son. Both were probably overqualified for the job.

Shephard was hardly on the payroll when Kenneth May burst into the newspaper headlines by publicly announcing that he was a member of the Communist party. The special piquancy of this development was that his father had recently become head of California's civilian defense agency. Sam essentially disowned his son, although they were reconciled some years later. When war broke out, Kenny volunteered and served in the ski troops in Italy, where he was commissioned a captain in the field.

This did not prevent the radicalism issue from shadowing his academic career after the war, and I always felt that his scientific productivity was greatly diminished by the energy he had to spend to defend himself politically. I had learned a great deal of economics from him and Shephard, and that personal contact added to my distress at his political difficulties.

Ronald Shephard turned out to be a fine member of my staff and, like Kenny, a lifelong friend. He was what one would call an original, always having an unusual way of looking at thingsparticularly at the irrationality of social conventions. After the war, when housing was difficult to find, he went around to university campuses, stating that he would accept any job that brought a house with it. Purdue University provided the house and he went there.

I never learned much about Shep's politics (he was what one would call a "personal anarchist," a label not predictive of political allegiances), but when we left Berkeley, he presented me with his personal copy of Karl Marx's *Capital*, all three volumes, which he said he was discarding. I put them conspicuously on my living room shelf, first in Chicago and then in

Pittsburgh, with the mental resolve that if it ever became politically necessary for me to remove them, I would at once migrate to Australia or New Zealand.

My Security File

That ends the recital of my contacts with radicalism in Chicago and Berkeley. Many years later, when, pricked by curiosity, I used the Freedom of Information Act to obtain the papers relating to my security clearancesall 550 pagesI was able to learn how much of my private life during this period was known, and how much unknown, to the FBI and Air Force Intelligence. Any of you who belong to the Depression or postwar generations will have recognized that there are at least a dozen items in this autobiography that could have been used to deny security clearance under the standard (and dangerous) rules of the game. The actual intelligence harvest, as recorded in my dossier, was quite random and very incomplete.

The items that later got me in trouble (I will tell the story shortly) were my subscription to the *Midwest Daily Record* and my friendships with Milton Chernin and his SRA associate, both of whom had used me as a reference. The newspaper had been noted by our Chicago landlady, who evidently inspected our trash barrels and reported it to the FBI when interviewed a decade later, in 1948.

My wife's ASU involvement was dimly suspected (it had been reported to the FBI by a dean of students at the University of Chicago) but never nailed down. The FBI certainly had no conception that we spent our main effort in that organization struggling against a Communist takeover; that was far too sophisticated for their world picture.

Among the things that never found their way into the FBI record were the Ellis Co-op; our attendance at one or two Communist cell meetings; my numerous Catholic-Trotskyite friends; the Communist cell in the SRA district office of our California experiment; Kenny May; and my volumes of *Capital*. Not a very good batting average for the FBI agents. Since, in the political climate of the 1940s and 1950s, any one of those items would likely have sunk me, I don't regret the inefficiency of the investigators. What I do regret, and resent, was the existence of an inquisitorial system that could build subversion and crime out of wholly legitimate activities and associations.

What also did not get into the record was the Progressive Club at the University of Chicago, with its active policy of excluding Communists from

membership. Once one understands the inquisitorial mentality, this is not at all surprising. Positive evidence of loyalty has no important place in the FBI record. Thirty interviews with friends, acquaintances, and associates who state with certainty that one is not a Communist do not weigh in the balance against a single interview that suggests that one may be. The negative item is recorded on the summary sheet, the positive items are not. One could have an interesting intellectual discussion of the Bayesian probability model that lies implicit in this inference process. Its practical effects are wholly corrosive of democratic freedoms.

Liberalism, the Postwar Period

By 1946, I had advanced to the chairmanship of the Department of Political and Social Science at Illinois Tech. The number of members of the department was not much more than the number of words in its title.

I continued to be sensitive to civil liberties issues. Much to my surprise, for I have absolutely no recollection of it, I recently recovered from my files a long memorandum I mailed in 1946 to Arthur Macmahon, then president of the American Political Science Association, recommending that the association set up a committee to address academic freedom in the universities. I reproduce here the first paragraphs of my memorandum to illustrate my concerns, and my views on academic liberties at that time.

PROPOSAL FOR A COMMITTEE ON
ACADEMIC FREEDOM OF THE AMERICAN
POLITICAL SCIENCE ASSOCIATION

There are a number of indications that the current anti-Communist enthusiasm is leading to a full-fledged attack on "leftist" tendencies among college textbooks and teachers in the social sciences. As in the past, this attack will not confine itself to individuals or books that are demonstrably communistic, but will broaden itself to include any person or thing that can be labeled "liberal." The members of the American Political Science Association, by virtue of their profession, should possess expert judgments as to the proper limits of academic freedom in the political realm, and as to the proper definition of "subversive." For this reason it is both suitable and highly necessary that the Association be in a position to take prompt action in defense of academic freedom should the anti-red campaign develop to dangerous proportions. It is proposed that a committee of the

Association be created with power to investigate instances of alleged subversive speech or writing, and to publicize its findings in each case.

MacMahon took my proposal quite seriously, and presented it on more than one occasion to the board of the association. He also consulted with the director of the American Association of University Professors, who after long silence made a bumbling and ambiguous reply that suggested that these issues were on AAUP turf and intrusions were not welcome. In the end, after more than a year of correspondence and deliberation, calm heads prevailed and nothing was done. The William Remington and Alger Hiss affairs, early events in the postwar Red hunts, were still a year or two in the future, and evidently did not cast a sufficiently dark shadow before them. I accepted the decision (I had to) as undoubtedly the product of more wisdom and experience than a young man possessed.

During the period of this correspondence, a small cloud crossed over my own sky. A letter was sent to Henry Heald, president of IIT, complaining that one of the members of my department had used a speech of the president of the National Association of Manufacturers in a deprecatory way (perhaps as an illustration of interest group propaganda). When Henry Heald interposed a courageous and appropriate protective shield, nothing came of the matter.

The 1948 U.S. presidential election also gave me some moments of anxiety. Among the members of my department, two were precinct captains for Henry Wallace's candidacy (supported by the Communists, among others), two for the ADA (backing Harry Truman), one for the "regular" Democratic machine, also backing Truman, and none for Dewey. If that had come to the attention of the *Chicago Tribune,* there would have been trouble for IIt and for me.

Perhaps I am craven in jittering about such matters, but perhaps jitters don't compromise your principles as long as they don't make you prudent. I certainly did nothing to deter my colleagues from performing their civic duties as they saw them, nor did I hint my concern to them. And of course nothing happened except for Harry Truman's marvelous victory.

At IIT, I taught an evening public administration class for public managers in federal (and a few state and local) positions. One of my students was a supervisor in the Chicago post office, Henry McGee, some years later to become the first black postmaster of Chicago. We became good friends, as did our families, and he convinced me that I should join the NAACP. On the FBI copy of my security application, there is a neat penciled check

opposite this item on my list of affiliations. Why would a white man join the NAACP in 1946? In the security interviews I have had, only in 1948 was that question actually asked.

Although IIt was located nearly in the center of Chicago's South Side "Black Belt," nearly all its black employees in the 1940s were janitors. The library had courageously, and over much opposition, hired one black clerical employee. When our department got money for a full-time secretary, my colleagues and I decided that the appointee would be black. Dean Larkin, although he was a cautious man, agreed to the action, and we took some steps to prepare other IIt employees to accept a black associate.

The next step was to find suitable candidates, which proved very hard. Since there were almost no secretarial jobs for blacks in Chicago, the Chicago high schools did not train them in secretarial skills, and black high school graduates did not find it profitable to pay tuition to private secretarial schools. After much search, we located Julia Jones, whose spelling was intuitive, whose grammar was not standard, whose vocabulary was limited, but who was obviously a bright young woman with good social skills, and willing both to learn what she needed to learn and to deal with whatever problems might arise in her lily-white surroundings.

Julia was a fine secretary during my remaining years at IIT, and my successor's secretary thereafter. Later she wrote me to express her gratitude for my patience while she was learning her trade. Her letter warmed but also embarrassed me, for I had done only what I ought to have done. Today it is hard to recall the atmosphere of those times (fortunately) and to remember how difficult it was to observe elementary moral principles.

Housing provided other tests of our social sensibilities. We had always lived in rented quarters, but as our family grew and we seemed firmly established at IIT, the urge came to buy a house. In 1947, the U.S. Supreme Court declared that racial covenants limiting sales of real property to Caucasians were unconstitutional, thereby freeing up most of Chicago's South Side, which had been bound by those covenants. We then felt we could buy a house without violating our principles, and acquired one in 1948 on 50th Street, a mile north of the University of Chicago campus.

A year later, to our surprise, we were on our way to Pittsburgh and had to sell our Chicago house. Meanwhile, the boundary of the Black Belt had moved up to 49th Street and was continuing to move. Should we show the house to black as well as white families? The answer may seem obvious in the light of our politics and values, but it was not. If we had been planning to stay in the neighborhood there would have been no question, because we could have participated in the solutions of any problems that might arise, and because if there were any financial costs to having black neighbors (it

was generally believed that real estate values would fall), we would share the cost.

But because we were moving away, our white neighbors, a number of whom had become our good friends, might accuse us of copping out or even of being "block-busters," interested only in money. An issue becomes a genuine moral issue when you feel it in the pit of your stomach. We felt the conflict between our loyalty to our good neighbors and our loyalty to our principles of human equality.

Before we put the house on the market, we discussed the matter with several neighbors whom we thought shared our liberal views. They took the high road, agreeing that liberalism did not mean much if it wavered the moment it touched the pocketbook. We then ignored the few complaints we heard from elsewhere on the block and offered the house through both black and white realty companies. In the end, it was sold to the little Episcopal church on the corner as its manse. Today, and quite independently of our tiny transaction, that neighborhood remains stable, racially mixed, and middle class. Our friends the Rothschilds lived there for many years and sent us news of it each Christmas.

There is one other instance I recall in Chicago that gave me a dubious association with communism. David Hawkins of the University of Colorado published a paper in 1948 containing a theorem that seemed too strong to me. Examining it closely, I soon found a counterexample which I sent to Hawkins. As we began to correspond about it, I also found the (weaker) correct theorem, and we agreed to write a joint paper making the correction and discussing the new theorem, which had interest in its own right. Our paper appeared in *Econometrica* in 1949. Some years later Hawkins appeared in Washington to testify (with dignity) before a congressional committee as an ex-Communist. I had co-authored a paper with a Communist whom I had never metan excellent example of the potential for guilt by association!

Aside from noticing my NAACP membership, the FBI did not record my several expressions of liberalism during the IIT years. Their absence from the record could be attributed either to enlightenment or inefficiency.

Loyalty Challenged

In 1948, when Congress passed the legislation for the Marshall Plan and set up the Economic Cooperation Administration, part of the political bargain, to secure the right-wing (anti-Soviet) votes that provided half the

support for the bill, was a provision for especially strict loyalty screening of ECA employees.

I accepted Don Stone's invitation to join the ECA and was required, even though my post was temporary, to go through a security clearance process. It soon appeared that there were difficulties, and I was called in by the security officers to explain the dubious items in my record—principally the subscription to the *Midwest Daily Record*. Had I really read that paper and, if so, why? "Yes," I answered, "I did. A political scientist reads a great many things in order to keep informed."

That did not go down well, especially since I did not offer further explanations or elaborations. The FBI record shows that, when one of my inquisitors on that occasion was interviewed fifteen years later by an FBI investigator during a subsequent clearance process, he explained: "If he had simply denied subscribing to it, we would have believed him. But his explanation was suspicious."

The ECA security people now did not want to certify my loyalty, a certification the law required for my continued employment. Fortunately, Don Stone had no doubts about me, nor did Ty Woods, an assistant director of ECA. After they went courageously to bat for me, I received the certificate of loyalty (suitable for framing, as the saying goes). But I am quite certain that a compromise was reached. The certificate was issued when the Security Division learned that I was only a temporary consultant who would soon go back to his university.

One nugget I later found among my security files, when I obtained them through the Freedom of Information Act, was a card bearing my name from the index file of the U.S. Civil Service Commission, dated 1948 and marked "questionable loyalty." This label was removed only in 1963. I could not resist writing to the Civil Service Commission to ask how "questionable loyalty" was defined in the law, and was told (without a hint of humor) that it meant only that the loyalty in question had not yet been determined definitively. So the mark of Cain was on me, unbeknownst to me, from 1948 to 1963.

I say "unbeknownst," but I was not entirely unsuspecting. Starting in 1948, I was several times asked by federal agencies to serve as a consultant, and then found that, after I had responded positively, there was no follow-up. The "certificate of loyalty" from ECA clearly attested to my loyalty only within very narrow boundaries of space and time, and the record of the investigation had not been lost.

The next episode of the story took place in Santa Monica, California, at the RAND Corporation. Financed mainly by the air force, RAND engaged in a large number of classified studies relating to air force strategy and

national security. Some basic research was also supported, and each summer a sort of free-floating crap game was organized that attracted an elite group of academics from all over the country to come to Santa Monica and think and swim. Partly they thought about specific air force problems, partly about the theory of decision making and especially game theory.

Through my connections with the Cowles Commission, and Cowles's with RAND, I was a RAND consultant once removed. Merrill Flood, a department head at RAND who had been a pioneer in applying management science techniques to municipal operations, knew me and my work and invited me in 1952 to come to RAND as a summer consultant. That, however, required security clearance.

In my reply to Merrill, I mentioned the difficulties I had had at ECA, and said that I would be willing to go through a security clearance only if RAND would not retreat at the first obstacle but would push the matter seriously. He agreed, I filled out the voluminous forms, and the clearance (at the lowest, "Secret," level) went through, apparently without problems. At least none were brought to my attention.

From the spring of 1952, I was a frequent consultant at RAND, particularly in connection with the Systems Research Laboratory which was created that year, and then, after 1955, with the Computer Science Department. I spent the entire 1960 61 year on leave of absence at RAND. In September of 1960 a new air force decree came down requiring anyone employed in the RAND Santa Monica building to have a "Top Secret" clearance. Accordingly, a new set of forms was filled out (it wasn't so bada secretary could copy most of the information from the previous set), and the FBI went about its full field investigation.

Months passed without any word. Then I was asked to report for an interview at an air force base in south Los Angeles. The base appeared to be almost deserted; I was directed to a totally isolated small building almost at its center, and in this intimidating setting two air force intelligence officers interrogated me. Nothing new emerged from the "interview," except that my father had evidently sent some money to a Russian-American friendship organization during or after the war.

My questioners were particularly interested in the University of Chicago period, a quarter-century in the past, and when I mentioned (in the transcript of the interview, which they edited, I think the verb was "admitted") that I had had contacts with Communists and with Catholics who also thought they were Trotskyites, they asked me to supply names. Then I did something for which I have always felt ashamed, although it did no harm. I mentioned my old friends Leo Shields (dead on Omaha Beach) and Winston Ashley (safe in his Dominican college). Harm or not, it was a violation of principle.

From my security records I later learned that air force intelligence actually tried to locate Winston, without success.

On my return to Santa Monica, I suggested to Dorothea that we go for a walk (I was not at all sure that the house was not bugged) and I let off steam for half an hour. Walking has always been a good way for me to calm down. I had covered many miles of the streets of Washington, too, during the summer at ECA after my interrogation.

In September 1961 I returned to Pittsburgh without having heard a "yea" or "nay" on my RAND security clearance. At that point I addressed a personal letter to Eugene Zuckert, secretary of the army, whom I had come to know when we both served on the board of directors of the Nuclear Science and Engineering Corporation, stating that "if you could without impropriety express an interest in my application being acted upon within finite time, I should appreciate it." I emphasized that I was not asking for special treatment, just for getting the papers moved off whatever bureaucratic desk they were on.

Within a few days of mailing that letter, I was informed that the clearance had come through, but it came so soon that I do not think the letter could have had anything to do with it; it was simply a coincidence. Perhaps there was a rule that all such cases had to be acted on within a year. The total elapsed time from the submission of my application to the approval of the clearance was a year and a week.

The RAND incident represented my last serious encounter with security problems. Of course when I was nominated to President Johnson's Science Advisory Committee, I had to be cleared at the executive office of the president at Top Secret level, but either because of my clearance in 1961 or because the rules are different at the White House, there was no delay. Since that time I have received Top Secret clearances from time to time in connection with my activities in the National Academy of Sciences. Again, no trouble. But when my security records were later sent me, I noticed that in all of these cases the summary sheets still reported the derogatory information that had been gathered in 1948.

The Loyalty of Intellectuals

In recounting this long tale, I have not discussed the important question: Am I in fact loyal, and reliably so? Or should the government be concerned that I might commit subversive acts or violate security? That is not an easy question. Security specialists have, with good warrant, a general suspicion

of intellectuals. Intuitively, they know that intellectuals seek to be loyal to abstractions like "truth," "virtue," or "freedom," rather than to a national state or its flag. As I write these lines, just a month has passed since young Chinese intellectuals died near Tiananmen while defending such abstractions. What does it mean when an intellectual swears loyalty to his country? Under what conditions can that oath be trusted?

We have some empirical evidence on these matters. First, most not all of the security breaches that have been detected in this country since World War II were motivated by greed or blackmail, not by ideology. In exceptional cases, there was, indeed, loyalty to a "higher" goal, often thought by the miscreant not to be inimical to the interests of the United States. I would cite Alger Hiss, as nearly as we can understand his case, and the uncontrite Oliver North as examples, and there were no doubt others.

But in most cases the intellectual, and even the ideologue (it is a little difficult to think of North as an intellectual) is not a good subversive, certainly not a good mole. Normally, intellectuals want to trumpet their ideology and values, not hide them, to lead the revolution, not spy for it.

Thoroughly disciplined members of the Communist party, especially immediately after the war when official attitudes toward Russia were returning from friendly to hostile, are the principal contemporary exceptions. If these had been the main target of our security efforts, it should not have been as difficult as it proved to be to separate them from other varieties of liberals. But perhaps separation was not the goal of the security agencies. We can still remember President Nixon's "enemies list."

At the beginning of this account, I mentioned that I have been and am a New Deal Democrat. The reason is depressingly simple, and has little to do with the wisdom or unwisdom of specific policies of either political party. Among the fundamental problems in every society, two stand out. People have to be motivated to contribute to the society, to produce. At the same time, they have to be protected if they are unable to take care of themselves adequately. You can think of it as the balance between incentives and distributive justice. Too much concern with the latter may weaken the former, and vice versa.

Using this simple-minded dichotomy, you can classify people (roughly) into two groups by their answers to the following question: Is it more important that (*a*) all chiselers be detected and removed from the welfare lists, or (*b*) no sparrow should fall from Heaven unseen and uncared for? If the answer is (*a*), the respondent is a Republican; if (*b*), a Democrat. Either answer is rationally defensible. I just happen to prefer the second one.

The introduction of this autobiography promised you mazes without minotaurs. Perhaps that was a little optimistic, because the maze of loyalty and national security that we have just been through did house a minotaur. Fortunately, and by not too wide a margin, I escaped being its victim. At the same time, I surely did not slay it, nor has it ceased to claim other victims. It remains a dangerous beast at large in a democracy.

Chapter 9
Building a Business School:
The Graduate School of Industrial Administration

Bill Cooper joined the Economics Department at Carnegie Institute of Technology in Pittsburgh in 1947. At his invitation, I visited Carnegie a year later to give a seminar for the economists. Pittsburgh had been for me, as for everyone, the Smoky City, the city where the streetlights had to burn at noon to pierce the sulfurous smog. I had seen Pittsburgh only from an overnight Pullman, which stopped there around midnight on my trips between Chicago and Washington.

Wakened by the shunting and switching of the cars, I would peer out the window of my berth as the train slowly maneuvered through the steep-walled valley of the Monongahela, the hillsides reflecting the lurid red glare of the open hearths, the coke ovens, and the blast furnaces of the great steel mills that lined the valley. Intermittently, a Bessemer converter would send up a great flaming flare, turning the scene almost daylight-bright. With the smoke and flame, the blackness lit by the red fires, it was a preview of hell.

I had a curious first impression of the Carnegie campus, too. I arrived there by cab through a snow-covered Schenley Park on a bright winter morning, catching a glimpse of Henry Hornbostel's stately Palladian buildings, then sitting almost outdoors, it seemed, in Bill's many-windowed office, surrounded by snow-carpeted lawns. I lectured to the economists on disguised unemployment in agriculture in "backward" economies, a topic I had explored in the course of my studies of the economic effects of atomic energy. The economists detected a bit of a foreign accent, but were polite.

Pittsburgh was a far more pleasant city than my midnight experiences had led me to expect. I learned of the Pittsburgh Renaissance which was just then ridding the city of most of its major sources of smog and pollution: houses heated by coal (replaced by natural gas), steam locomotives (replaced by diesels), and Bessemer converters (replaced by open hearths). Techno-

logical changes had conspired at this time to make all of these polluters uneconomical, and civic action had introduced strong and successful regulations to clean the air. (Was inventionthe new technologythe mother of necessity here?)

Sometime in 1948, soon after my first visit to Pittsburgh, the Carnegie Institute of Technology received a gift of $5 million in endowment and $1 million for a building for a new Graduate School of Industrial Administration (GSIA) that would provide business education for students with undergraduate degrees in science and engineering. The donor was William Larimer Mellon, who had founded the Gulf Oil Company.* From his industrial experience, he had concluded that modern high-tech firms needed top executives who both were skilled in management and understood science and technology. The provost of Carnegie Tech, Elliott Dunlap Smith, had described to Mellon the newly revised undergraduate industrial management curriculum as a rough prototype for a program that would attain these goals. Mr. Mellon was impressed, and the gift followed.

At Bill's suggestion, I was asked to come to Carnegie again to discuss the plans for the new school with Provost Smith and Lee Bach, chairman of the Economics Department. An invitation to join the faculty as professor of administration and chairman of the Department of Industrial Management soon followed.

I was not eager to leave Illinois Tech, for I had confidence that with Don Smithburg and Victor Thompson we could build a strong public administration program there. We were also writing our textbook in public administration (published in 1950). I had sunk deep roots into IIT. I was finally convinced, however, that the financial resources of GSIA could launch, much sooner than at IIT, the program of empirical research in organization that seemed the logical sequel to *Administrative Behavior*. My visits to Pittsburgh showed me that it was a livable city, no dirtier than Chicago, perhaps cleaner.

Having made the decision, on a trip to Pittsburgh in April 1949, I took a long walk early one morning through much of the north part of Squirrel Hill. Just before this visit, I had drawn on a map of Pittsburgh a circle of one mile radius around the Carnegie Tech campus, for I was resolved to walk to work instead of commuting, and had checked the census tract data to discover which portions of this area were inhabited by college-educated,

* William Larimer Mellon was a grandson of the banker Thomas Mellon, who had established the Mellon fortune at the time of the Civil War. He was a nephew of Andrew Mellon and a cousin of Richard King Mellon, who in turn was the central figure in the Mellon barony when I reached Pittsburgh and the partner, with Mayor Dave Lawrence, in bringing about the Pittsburgh Renaissance.

middle-class families. I looked in these portions for a house we could afford.

In the last lap of my walk, I climbed the steep slope behind the campus to the Schenley Park Golf Course, and walked east a half-mile on a street called Northumberland. The houses were noticeably nicer than the house we had just bought in Chicago. From the corner of Northumberland and Inverness, I looked at the lawns and flowers a bit enviously, glad that I would soon be able to bring the children to a brighter and greener neighborhood.

That was more than forty years ago. We still live in the house on Northumberland Street that we bought in the summer of 1949, just a mile from the Carnegie campus, from which I have walked back and forth each day, gradually erasing the memories of commuting on Chicago streetcars. I estimate that I have walked nearly 20,000 miles on that one mile of Northumberland Street, enough to carry me around the world if I don't stick too close to the equator (but only, as a friend has pointed out, if I can walk on water).

After 10,000 trips along the same path, one's surroundings should become invisible. They don't. On the first half of the morning walk straight west from home, I proceed down the tree-lined street of affluent homes, noting new construction, for-sale signs, and other marks of change. From April to early autumn, a succession of flowers, shrubs, and trees display their colors. I hear a song sparrow or a mourning dove; occasionally in autumn I catch a glimpse of a flicker's white rump and red-gold wings. I'd like to say that I spend the time of those morning walks thinking deep thoughts. I rarely do. Thoughts are easily interrupted by any new sight or sound, the chain broken and unpatchable.

At the halfway mark, after crossing Forbes Street, there is a short climb to gain the ridge at the golf course, where, looking southwest far across the valley, I see clusters of houses marching up the hills that form the steep south bank of the Monongahela River, nearly three miles away. The masses of white and pastel-colored houses reflecting the morning light are sharply outlined against the dark green hills, giving them a three-dimensional solidity.

In foreground and middle distance below me lie the meadows and woods of Schenley Park, the contours of the last line of treetops in the park composing a complex counterpoint with the gently undulating ridge on the horizon. As I proceed along my path, the contour continually shifts against the ridge, forming at each glance a new composition. Sometimes I am reminded of the Rhine below Weisbaden, of Gardanne on its hill hear Aixen-Provence, of Siena, viewed from a distant ridgeall different, but each with this sense of human settlement crowding up onto the protective hills,

crowned each by its church or fortress. Mumford's medieval city, repeated even in New World Pittsburgh.

On some mornings, of course, the far bank of the river is hidden in mists or smog (much less of the latter since the steel mills disappeared). In winter, the buildings on the slope are dark against the snow, a negative of the summer scene. But ten thousand different views present me with new pleasures for every walk. Then the rapid descent down to the campus.

Going home in the evening is another matter. The walk starts with the steep hundred-foot climb to the golf course. These days, even walking slowly, I reach the top winded. (Why wasn't it possible to arrange the uphill walk for morning and the downhill for evening? Alas, the East End of Pittsburgh isn't laid out that way.)

Sometimes, looking back, I am rewarded by a spectacular sunset, but, with the light behind them, the houses on the distant hills do not announce themselves with the same sharp contrast as in the morning. The south bank loses its solidity, its third dimension, and becomes a flat backdrop to the park landscape. At the end of the golf course, I turn my back to the view and trudge home.

Getting Started

Life during the early years of GSIA was a three-ring circus. Lee Bach, who was appointed dean of the new school, Bill Cooper, and I played leading roles in developing its faculty and curriculum. Provost Smith, who had an industrial and academic background in personnel administration, was also very active. We had almost a clean slate, but what had previously been written on it is worth mentioning.

First, we inherited the undergraduate industrial management (read "industrial engineering") curriculum, which Bach, Cooper, and Smith had revamped, and which in fact provided an excellent template for our new graduate program. Second, Lee Bach had come to Carnegie in 1947, at age thirty, with the promise that he could start a small doctoral program in economics, and had hired several economists on the strength of that promise. The first of these two inheritances gave us a good start, the second, as I shall recount, caused complications.

Almost none of the founding fathers of GSIA (except Provost Smith) had extensive backgrounds in management or business education. We were social scientists who had discovered in one way or another that organizational and business environments provide a fertile source of basic research ideas,

and who therefore did not regard *basic* and *applied* as antithetical terms. Accurately or not, we perceived American business education at that time as a wasteland of vocationalism that needed to be transformed into science-based professionalism, as medicine and engineering had been transformed a generation or two earlier.

We were most fortunate in that we took on this task at that particular moment in history. World War II had spawned something called "operational analysis" or "operations research," the use of quantitative tools for managerial problem solving and decision making. Just after the war, a number of people were seeking to transfer these tools to peacetime industrial applications, and new tools (such as linear programming) were being discovered.

At about the same time, the behavioral sciences were flourishing and were being brought to bear on issues in organization and management. (The activities of the Political Science Department at Chicago, Barnard's *The Functions of the Executive,* and my *Administrative Behavior* were examples of these trends.) Publication of the extensive field studies carried out in the Hawthorne Works of the Western Electric Company by F. J. Roethlisberger and W. J. Dickson (1939) had begun before the war, and our field experiment in the California State Relief Administration was completed in 1941.

The postwar flowering of management science and of the behavioral approach to organization theory provided the substance of applied science we needed. The quantitative undergraduate training of our students made it possible to put that science into the curriculum. Having worked out a Master's curriculum appropriate to these goals, we developed two major research areas: organizational behavior and quantitative management science. I assumed leadership in the former, in collaboration with Harold Guetzkow, who joined a year later. Bill Cooper took the principal initiative in the quantitative area, but I also participated heavily in that, heading one of his research teams. So during this period I was at once organization theorist, management scientist, and business school administrator the three rings of my circus.

We got off to a good start, keeping very busy for several years recruiting faculty, launching classes of about fifteen students each, and getting the research programs under way. My unaided memory insists that everything went fantastically well, but my files remind me that our ultimate success was not achieved without pain. The crisis arrived in the spring and summer of 1951. It had two interrelated foci, Bill Cooper and the economics faculty.

Although he was born in Birmingham, Alabama, in 1914, I cannot think of Bill as a Southerner. He grew up on the west side of Chicago, full of

plans and projects. From previous chapters, it is already evident that some of his projects have had major impacts on my own life. You will recall that in 1937 he had persuaded a girl named Dorothea Pye to accept me as a partner on a double date. Not long after that, he and I formed the Progressive Club at the University of Chicago as the instrument for our political activities. I have mentioned that a B in boxing was the only grade on my graduate transcript at Chicago, but I did not disclose that Bill Cooper had been my instructor in that class.

Nearly ten years later, Bill had suggested to me that I might like to attend with him the seminars of the Cowles Commission, almost converting me into a full-time economist. Some four years after that, he had persuaded me to leave my position at IIT and join the new venture in business education just starting at Carnegie Tech. Bill Cooper can be very persuasive, with the persuasiveness of the true entrepreneur. Without his persuasion, my life would have been very different from the one I am recounting herewhether better or worse I cannot say, but different. It would not have traversed the same branches of the maze.

It was the economist Joseph Schumpeter, I believe, who defined an entrepreneur as someone who risks someone else's money. Put less pejoratively, an entrepreneur is a broker who brings about marriages between ideas and resources. He dreams imaginative dreams and convinces others that those dreams are attainable, persuades them to place their bets on him.

Classically, entrepreneurs were supposed to belong to the world of business. But brokerage of ideas and resources is not confined to business; it is at least as much at home in academe. In a typical major American research university, one-third to one-half of the annual expenditures are funded by the entrepreneurial efforts of faculty members who write their dreams of undiscovered truths in research proposals addressed persuasively to foundations and government agencies.

For the academic entrepreneur, the stakes are even higher than dollars because the resources at risk are human careers. An academic entrepreneur publishes a paper arguing that a particular domain of knowledge is a gold vein of secrets, thereby attracting a swarm of prospectors. He urges some colleagues into a joint venture of exploration. He persuades a graduate student to direct his life into a particularly alluring line of inquiry. Entrepreneurship in science is a Roman gamble: the winnings are more often glory than riches, and the losses, lifelong futility. Bill Cooper has been a highly successful academic entrepreneur, whose successes have paid off not only for himself but for those who have invested in him.

Bill was a Depression child, who, to make his way in the world, started

up the ladder of Golden Gloves boxing. His mental abilities were recognized by the accountant Eric Kohler, who found him caddying on a golf course and put him back on the track of education. Admitted to the University of Chicago without completing high school, Bill arrived on the campus in 1934. I have no recollection of how we met there, but we became friends soon after he came, and a couple of years later were engaged in the joint political venture that has already been described.

Bill came to Carnegie Institute of Technology in 1947, recommended to Lee Bach by someone at Chicago. He had meanwhile spent some years after graduation from college as assistant to the controller of the Tennessee Valley Authority, as a graduate student at Columbia University, as an economist in the Bureau of the Budget, and, just after World War II, as an assistant professor at the University of Chicago. He arrived in Pittsburgh in time to become one of the entrepreneurs who attracted the six million GSIA dollars to Carnegie.

Bill Cooper was not only an entrepreneur but a revolutionary. His imagination and his indifference to convention were critical ingredients in the successful GSIA effort. That indifference also caused a temporary personal problem for Bill. At Columbia University, where he was writing a doctoral dissertation, his offbeat approach to accounting and economics greatly perplexed some members of his thesis committee. For several years his colleagues at Carnegie urged him to make such compromises and clarifications in the thesis document as would satisfy the committee, for we did not want the lack of a union card to endanger his academic progress.

But Bill's entrepreneurial luck (or talent) held out here too. After a short time, his contributions to economics and management science became so well recognized that the question of a degree never came up at promotion time. Most people, by then, just supposed he had one; and in any event, it did not matter a whit whether or not he had. When in 1970 Ohio State University awarded Bill an honorary doctorate, I am sure he felt pleased, as all people do in such circumstances; but I am sure also that in the intervening years he did not feel that he lacked credentials for the job he was doing.

Bill's example probably accounted for the flexibility that the GSIA faculty later sometimes exhibited in deciding what constituted a thesis in industrial administration, awarding the degree, for example, to Allen Newell and to others for research in artificial intelligence. Bill Cooper was never one to think slots should not be as flexible as people.

As soon as the new management science techniques appeared on the horizon, and especially linear programming, Bill understood their potential

and undertook to master them, to push them forward and apply them to important practical problems. He also saw their key significance for the curriculum. Our first class of Master's students in 1950 was exposed to linear programming in a seminar that Bill and I taught jointly. He soon found in the Mathematics Department exactly the right partner, Abe Charnes, for pushing research that used this tool. After forty years of partnership, the team of Charnes and Cooper continues today to demonstrate the power and flexibility of the linear programming (LP) formalism, and the range of its real-world applications.

The contributions of Charnes and Cooper being matters of public record, they do not have to be recounted here. I will just mention how their entrepreneurial skills were tested by their first great successthe introduction of linear programming into the oil industry for blending gasoline in refineries. Prudent businessmen, like prudent educators, always follow Alexander Pope's precept: "Be not the first by whom the new are tried; nor yet the last to lay the old aside." The problem of the entrepreneur is to persuade someone to go first; the others will follow readily enough.

In the case of the refinery problem, Gulf Oil's Philadelphia refinery agreed to provide Bill and Abe with access and data; models were tested; and results were obtained (on paper) showing the superiority of the new model over traditional rule-of-thumb decision methods. But no amount of persuasion convinced the company that they should be "the first by whom the new are tried." So Bill and Abe could publish only their on-paper results in a scholarly article.

But that did the trick. The article came to the attention of ESSO, who assumed that surely Gulf was already using LP and that ESSO was lagging behind its competitors. Work was soon under way at the ESSO Research Laboratories, and a genuine application became a reality, with everyone congratulating himself on not taking the first, big risk.

Bill Cooper also played a central role in creating the Institute of Management Sciences. When Carnegie Tech received an endowment for a School of Urban and Public Affairs, Bill became its first dean. More surprisingly, he later held, for several years, an appointment at the Harvard Business School, which we had always regarded as The Competition. He evidently thought (and no doubt correctly) that Harvard had not yet wholly digested the message of management science and needed reform in that direction.

Still later, Bill rejoined his former colleagues Abe Charnes and George Kozmetsky at the University of Texas Business School. I will leave Bill's story at that point, and return to my account of events in 1951, having noted that Bill's entrepreneurial activity continues as vigorously as ever today.

Stormy Weather

Of the three members of the triumvirateLee, Bill, and Ithat assumed the leadership of GSIA, Bill was the radical, the least constrained by the established conventions of business education or by the realities of organizations. (At the same time, of the three, he had had the most extensive organizational experience.) His influence prevented Lee and me from conceding too much to the pressures of the outside business and academic worlds. His many innovative ideas also included some that seemed pretty wild to us. When there was disagreement among us, it often ended with a 2-to-1 vote, with Bill in the minority. (Lee did not take his deanship lightly, and did not settle things by formal majorities. He discussed and listened and was most unarbitrary, but in the end, he made the decision.)

Bill also brought into his research team several researchers who seemed to Lee and me to live in a very abstract world having little contact with real management problems. Space constraints displaced this little group to another building, 500 feet from the rest of usan enormous social distance, as anyone who is familiar with organizations and the importance of proximity for social communication will recognize.

Relations between Bill and me were soon badly strained, as he and his companions became more and more frustrated by the 2-to-1 votes. The situation worsened when one of Bill's close associates decided to leave because we would not meet all the terms he had set for remaining. Since, wearing one of my three hats, I was part of Bill's project, he had also injected himself into my continuing consulting relation with the Cowles Commission, which he viewed as a conflict of interest or of loyalty. Finally, he dragged his feet on providing support for the empirical work I wanted to begin in my scheduling project. We had all the ingredients for a good feud.

Meanwhile, the economists were becoming unhappy, believing they were being pressured (and perhaps they were) to move their research in directions relevant to a school of business. We didn't think of it as "pressure" but as opportunity to participate in research for which we had found funding: a study of the use of accounting data by factory managers in their decision making; a study of the roles of forecasting and feedback in scheduling factories with uncertain demands for their products.

Here were settings in which we could compare the ways in which decisions were actually made in business firms with the ways that economic theory and textbooks said they were made. These were not settings, however, to which economists were accustomed or in which they felt comfortable. There were few precedents in the profession for studying the decisions of individual firms at first-hand. Nevertheless, an economist, Gordon Tyndal, participated

in the accounting study, and three economists, Charlie Holt, Franco Modigliani, and Jack Muth, in the study of production planning; and both studies produced results of considerable value.

But there was another difficulty with the economistsperhaps the real reason they felt coerced and threatened. I had not for some years (certainly not since my association with the Cowles Commission) kept secret my skepticism about mainstream neoclassical economics. I was prepared to preach the heresies of bounded rationality to economists, from the gospel of *Administrative Behavior,* chapter 5, in season and out.

At first I did not recognize that when you are in a position of authority you cannot debate freely with people in your organization without some of them believing that they may endanger their careers if they disagree with you too vigorously. Perhaps they are rightI like to think not in my case, but self-deception is easy. People who agree with you are apt to seem a little more intelligent than those who don't. Power does corrupt.

But behaving differently as administrator than as colleague by pulling my intellectual punches did not (and does not) appeal to me as a way of life. In my previous administrative posts, in Berkeley and at IIT, my colleagues had not taken any guff from me. Perhaps I had been too young and inexperienced to be intimidating to them. And in my previous associations, we were almost all political scientists; I didn't come from an alien discipline.

GSIA, with its plurality of economists, was different. I began to acquire a distaste for the intermingling of academic with management roles that undoubtedly contributed to my later decisions gradually to retreat from administration. I would rather see my ideas gain acceptance by their merit than by administrative fiat; but at the same time I cannot be neutral about the directions taken by the organizations to which I belong. The conflict is eased when I do not have line authority. Then I have to persuade.

At any rate, I heckled the GSIA economists about their ridiculous assumptions of human omniscience, and they increasingly viewed me as the main obstacle to building "real" economics in the school. That was the other half of the crisis, which became acute when the two dissident factions, Bill Cooper's research group and the economists, sought to unite.

Early in July 1951, I had a long evening's conversation with Bill at a bar in Shadyside called The Fox Cafe, in which tempers were kept but all of the dirty linen was hung out for inspection. Bill described me as intimidating the faculty who disagreed with me, and proposed that I give up my formal authority as chairman of the I.M. Department. He even proposed an "autonomous" research project in administration, with its own $100,000 budget as compensation!

I listened for a long time to Bill's description of my shortcomings, then allowed as how I didn't think I would resign my chairmanship and counterattacked by discussing my problems working within Bill's research group and what was needed to solve them. The conversation ended inconclusively but in what I would describe as a tentatively friendly mood. The next morning I recorded the conversation in a four-page, single-spaced aide-mémoire, which I carefully filed in case there were later disagreements about what had transpired.

After I had confirmed with a few other faculty that morale was truly low, I directed a memorandum to Lee. As I reread it now, I find the tone of the document in some ways curious. I nearly always refer to our colleagues as "staff" instead of "faculty." Some of the references to the culling of the faculty that Lee and I had inherited have a rather cold-blooded sound. There is a straining for objectivity, and perhaps no more defensiveness than one would expect under the circumstances. The document seeks to conceal rather than convey the anger and anxiety I was feeling. The mood is one of diagnosis and treatment rather than blame.

In my memorandum, I told Lee that I had verified the generally low level of morale. The economists were ambivalent "about the special character of the school as a school of *industrial* administration," and were "desperately intent on retaining their professional roles as economists." They felt that they had no representation in administrative councils and that he, Lee, had ceased to look at things as an economist. There was much misinformation about the plans and intentions of the leadership, and in the griping, the faculty sometimes referred to themselves as "vassals."

The faculty in industrial administration had been urged to associate themselves with the economists in their opposition to current policies. While feeling themselves excluded from policy making, the faculty also complained about the long, inconclusive staff meetings, where they did not think they had a real influence on the decisions that were reached.

Having detailed the problems, I went on to list some possible actions: A committee composed solely of economists could be created to guide the graduate program in economics. A chairman could be appointed to take charge of this program, although there was a danger that he would either be "captured" by the economists or rejected as not loyally representing them. The troika of Bach, Cooper, and myself should be expanded to bring in other economists (balanced by faculty from industrial management). Lee should avoid transacting business conspicuously with me at lunch. I should probably participate more actively in Bill's research group, avoiding "methodological discussions about the need for economists becoming psycholo-

gists." More concern should be shown for the morale of faculty who would probably not be retained indefinitely. Additional administrative assistance should be provided for Lee.

I concluded the memorandum with the ringing declaration that "as I look over the situation . . . I see nothing that calls in question the basic directions the School's development has been following. . . . I see no real proposal for an alternative direction, but a rather high concentration of the stresses that commonly go with rapid organizational change." I then expressed optimism that we could rebuild morale and "agreement as to the School's function and focus" and "allay the fear of the economists that they are being made the victims of an empire-building attempt by the I.M. group."

The memorandum is a good example of my approach to an administrative crisis. Reading it after passions have had nearly forty years to cool, I am struck by how much it draws on theory from *Administrative Behavior,* and particularly the theory of organizational identification and loyalty, as the basis for its diagnosis and recommendations. Evidently I believed what I had written about organizations well enough to apply it to my own administrative problems.

Much of my later article on the organization of a business school (1967a) is already present in my analysis of this crisis. The crisis was ended, as most crises are, by gradual exhaustion, by applying a few Band-aids (some of them suggested in my memorandum), by the departure of some of our more alienated colleagues, and by the completion of the new GSIA building, which allowed us to reunite our faculty in one compact group.

The building, which was supposed to cost $1 million, was designed by an architect, with the usual recklessness of that profession, to cost $1.6 million. Since we were not about to dip into endowment, we reduced cost nearly to budget by postponing the installation of air-conditioning and eliminating the elevator (the building is only three stories in height). A few years later, needing more space, we installed an office for an associate dean in the elevator shaft.

To puncture as much as possible the disciplinary boundaries, we distributed the faculty more or less randomly throughout the building, so that there would not be compact pockets of organization theorists, economists, or finance specialists. Of course they gradually found ways of reuniting themselves, but we were able at least to delay their segregation into disciplines.

The problems that created the crisis did not wholly go away; they were built into the fabric of the GSIA mission. One problem was the fascination of abstract mathematical techniques, which sometimes emphasized the

mathematics more than the management applications. A second problem was the partial mismatch between the "pure science" values economists acquire from their discipline and the interest in real-world applications that characterizes a business school. A third, related problem was the near incompatibility of the behavioral theories of economic decision making that some of us were developing with the neoclassical theories espoused by most of the economists.

Keeping the balance of the scientific and the professional, of the economic and the behavioral, was an arduous job. Only the complete dedication and strong leadership of Lee Bach held the venture on course. In my paper on business schools, I wrote that "organizing a professional school . . . is very much like mixing oil with water: . . . And the task is not finished when the goal has been achieved. Left to themselves, the oil and water will separate again. So also will the disciplines and the professions" (Simon 1967a, p. 16). By hard work, we managed to keep GSIA pretty well emulsified, at least until the 1960s.

In my essay from which the preceding quotation comes, I explain why these problems are endemic to professional educationin medicine, engineering, and business. This law of nature, learned during my first two years at GSIA, continued to influence the development of the school.

The great thing about such controversy as we had in GSIA was that it was more about principles, issues, and policies than about personal or organizational advantage. I see Mr. Freud smiling, but he is wrong, as he should have known from his own controversies. Only people who believe deeply and almost fanatically in a dreamas many of us in GSIA didcan struggle so hard without inner doubt and conflict, and without losing, in the presence of frequent disagreement on particulars, a deep sense of common purpose and mutual respect.

Nor do I want to make conflict seem to play a greater role in GSIA than it did; but the story of a marriage that pretends there were no spats is always a little dull. More important than internal agreement and disagreement was the position of GSIA vis-à-vis the world. Evolutionists have discussed the advantages and disadvantages, for progress, of an isolated island community. GSIA was such a community, open and hospitable to alien ideas blown in from the sea, but protected from the need to defend the tender mutants it had bred against constant confrontation with all the established mainland species. Its success speaks for the island as a locus for innovation.

The island metaphor was only one part of GSIA's external posture. We also had David and Goliath much in mind. For several of uscertainly for Bill Cooper and methere was no greater pleasure than being the underdog,

unless it was the pleasure of being the winning underdog. And we often portrayed in such terms our struggles against the Goliaths of traditional education, conventional business practice, and classical economics.

More Colleagues

I have observed that the spectacular growth of GSIA owed much to the leadership style of Lee Bach, who served as dean from 1949 until 1961, and whose strength of conviction and character kept the enterprise on course. It owed a great deal, also, to Provost Elliott Dunlap Smith, who played a very active role in the early development of the school, and an educational role throughout Carnegie Tech that was exceptionally important for my own development.

Business schools (GSIA included) are sometimes accused of not teaching management and leadership. I expect that generally they do not, mainly because they don't know what to teach under those headings or how to teach it. Possibly, just possibly, we could start with biography, with the styles and behaviors of outstanding managers. It isn't very analytic, but maybe it could do some good.

Lee Bach serves as an example. His talents are very different from the entrepreneurial talents of Bill Cooper, yet he managed with immense success the venture capital that W. L. Mellon had entrusted to him. When I try to describe his style, it always seems too simple, too obvious. It's like saying of a tennis ace, "He always hit the ball squarely and with force, placing it precisely where he aimed." If you can do that, you can be a great tennis player. But is the advice worth teaching? What do you do with that information? I will try to describe Lee Bach's methods, but with no conviction that what I say will make great managers or entrepreneurs.

Lee Bach persuaded us, by example, to set the goals of GSIA sky-high, stretching the limits of the possible. And as aspirations were attained, goals moved higher. Second, Lee always found a way to reconcile technique (we were loaded with that) with common sense (sometimes in short supply). Third, Lee was always more interested in getting the job done, and done well, than in placing blame when something went awry along the way. High aspirations, common sense, and responsibility for getting results. It seems simple enough.

I have traveled a good deal in Japan, mostly for enjoyment but also from curiosity about Japanese methods of management. I have learned the Japanese language (badly); I have read widely. Over even a longer period, I have had at least four close Japanese friends. I have learned about the *ringi*

system of decentralized and consultative decision making. But I have also met some of the founders, like Matsushita, of the Japanese electronics industry. With all the talk of "decentralization" and "participation," these executives are tough cookies, men of decision and action. Perhapsprobablythey listen, but I doubt that they lead from behind.

The moral I have drawn from these journeys abroad and my studies is that a strong rudder and motivation on top are not antithetical to openness to ideas from below, from abovefrom everywhere. Management does not have to be weak to be "participative." All it requires is a manager who is strong enough in his inner convictions not to feel obliged to defend himself from ideas that come from without.

Lee Bach was also (and is) a tough cookie. He led from strength, but he was never afraid of ideas, even the ideas of others. He was not afraid because he had confidence in his ability to separate sense from malarky; he was never bowled over (to use his favorite phrase) by technological virtuosity and fireworks.

Lee spent a year in law school before he opted for graduate study in economics. Possibly as a result of this experience, he is a formidable cross-examiner, as anyone who has been questioned by him at lunch or in the classroom will testify. When the cross-examination ends, the proposal under consideration has been dissected thoroughly, and its muscles, blood vessels, and bones are all clearly visible on the dissecting table. And the scalpel was ordinary English, no mathematical equations or technical jargon.

Lee has a sense of legitimacy and hierarchy that today may seem a little old-fashioned (undemocratic?), especially in a typical academic environment. But Carnegie Tech was not a typical academic environment. It had come out of the tradition of technical universities, very different from the tradition of liberal arts colleges, much closer to the organizational tradition of business and industry. In that tradition, Lee never let you forget that the dean was responsible for the final decisions in his school, or that he was dean.

On the other hand, I rarely felt that Lee pulled rank on me or others, because decisions were made after full discussion, and consensus was reached more often than not. Lee was enormously patient in conducting lengthy Quaker meetings of the faculty, which usually reached agreement and rarely took a formal vote. Sometimes he did arrive at a meeting with a knowledge of what the outcome must be, but generally he was open to advice and persuasion, and invested little of his self-esteem in trademarking ideas with his own name. If decisions had to be made within specified ground rules, because of Carnegie Tech policies or for other reasons, he was open and explicit in stating the constraints.

Lee was sometimes regarded as cold by those who did not know him well, a reputation that stemmed from his concern for formal organization. His greatest weakness was his inability to delegate detail to others, which left him with an immense workload. In a year when he was on leave of absence and I was acting dean, I had great difficulty convincing him that nothing terrible would happen if he simply forgot about GSIA for a while, and by budget time in the spring, although his leave had not ended, he was back nearly full-time. If I had really *wanted* to manage rather than spend my time on research, I might have been quite unhappy that year.

I did accomplish one thing as acting dean. It always seemed to me important that Lee, with his reputation for aloofness, should have a secretary who would present a gracious and smiling face to those who came to see him. That did not describe his secretary at the time I took over. I quickly made her sufficiently unhappy with my managerial style that she quit. Lee never said much about the departure of his secretary, but I think he forgave me when he found his public relations significantly improved with the new one I had hired.

When I returned from my own leave at RAND in 1961, Lee informed me that he was going to resign as dean. He had learned that he was suffering from an illness that would be aggravated by heavy administrative responsibilities, and he was unwilling to proceed at half steam. A few years later he moved to Stanford, where he did not have an official administrative position, but where his advice and counsel played an important role in the rapid progress of the Stanford Business School and in top policy making in the university as a whole.

I warned you that I would say little about Lee that would tell you how to be a good manager, and I have made good my warning. The principles of good management are simple, even trivial. They are not widely practiced for the same reason that Christianity is not widely practiced. It is not enough to know what the principles are; you must acquire deeply ingrained habits of carrying them out, in the face of all sorts of strong urges to stray onto more comfortable and pleasant paths, to respond without inhibition to provocations, and just to goof off. Lee had the self-discipline actually to apply the principles, to behave like a good manager and leader. Not many of us do.

Another person of great importance to GSIA, to Carnegie Tech, and to me was Elliott Dunlap Smith, the provost. Smith was always careful to point out that his name was spelled with two *l'*s and two *t'*s, thus distinguishing his line from the more prestigious New England Eliots. Still, and in spite of the additional fact that he was raised in Chicago, he never forgot, or allowed you to forget, his New England roots or his ties with Yale.

As an unchallengeable patrician, he permitted himself a full quota of those idiosyncrasies and crotchets that mark the gentry of England, both Old and New. Occasionally there wafted from him a hint of condescending anti-Semitism, but had I been Irish, I am sure I would have detected corresponding condescension. In my experience, it never translated into acts of partiality; he was simply aware that he was a Brahmin, and did not reject the sense of superior birth that came with that endowment. His wife, Marie, was as New England as he.

Graduated as a lawyer, Elliott Smith had spent much of his professional career as personnel manager for the Dennison Manufacturing Company, a leading manufacturer of office supplies. There he educated himself to be rather a good amateur psychologist (I am not sure that the word *amateur* is even fair here), concerned with applying psychology to industrial management. His 1928 book, titled *Psychology for Executives,* has an appendix listing "good books on psychology" that references the core psychological works of that timeJames, Woodworth, Watson, Koffka, Dewey, F. A. Allport, and many othersbut not Freud. The book is full of sound functionalist psychology and common sense.

Elliott Smith was to distinguish himself mainly not as a writer or a scholar but as a passionate teacher and trainer of teachers. His own teaching was largely in the domain of human relations, first in Yale's industrial administration course in its Engineering College, then in Carnegie Tech's undergraduate industrial management program, and finally in GSIA.

Perhaps his interest in human relations had its origins in his early exposure to John Dewey's experimental school in Chicago, which he attended as a boy. Perhaps this interest was encouraged further by the fact that his own human relations often left something to be desired, as I shall explain. Certainly the interest matured as a central professional concern in his work at Dennison.

His course, "Human Relations in Industry," was built around a combination of lectures, role-playing sessions, and pure Elliott Smith. He was one of the pioneers in the use of role playing as an educational device, and probably unique in hiring and coaching students of the Drama Department to play roles opposite the management students. The management students interviewed secretaries, applied for jobs (with Smith as prospective employer), presented consulting reports, and disciplined erring employees on the GSIA stage, in front of their fellow students and critiqued by Smith.

One of Smith's favorite performances, early in the semester, was to read aloud on the stage and comment upon the genuine application letters he had received from his students, who were required to apply in this way for the position of teaching assistant. No student whose letter was thus read

(anonymously, of course) and dissected ever forgot the vivid thumbnail sketch of his character that Smith provided. The whole procedure would never have passed a university review committee on human subjects, an institution that did not then exist.

The character analyses were all hunch and no science (but of course that is the way the real world works too). I think Smith believed his diagnoses, but that is not the point. What he demonstrated to the students, unforgettably, is how we reveal ourselves to others, intentionally or not, accurately or not, by our written and oral words.

As befitted one who followed James and Dewey and ignored Freud, Smith's course provided a highly rationalistic view of human relations based primarily on learning theory. His central aim was to show students how to manage their own learning, of human relations skills or any others, and to set their own goals for learning. Fundamentally, he was a teacher of skills, not of knowledgebelieving in the latter only as it contributed to the former. It was more important that the students understand a few basic psychological principles and *apply* them than that they be sophisticated in academic psychology.

Hence the course was built around a simple, but sound, catechism of learning theory combined with exercises for applying the catechism. Learning requires knowledge of results (reinforcement). The student learns from doing, and *only* from doing. (The teacher is relevant only in influencing the behavior of the student.) There were other principles relating to problem-solving skills and human relations skills, but these will give the flavor. The entire catechism was handed out to the students at the beginning of the semester on a couple of mimeographed sheets, expressed in a highly esoteric ideographic code that Smith had developedI guess as a mnemonic device. Woe to the student who did not learn to use this encrypted language accurately.

At Yale, Smith had met Robert Doherty, electrical engineer and dean of engineering, who, upon becoming president of Carnegie Tech, brought Smith there as provost. Hence, Elliott's full stage, ranging over all of Carnegie Tech, was much larger than the one on which he acted with his management students.

A tall, lean man of enormous energy and speed of stride and speech, Smith was always full of ideas, some new, some old, which he supported passionately as long as he held them. He was not a stubborn man, but, as with most of us, his changes in mind usually took place between conversations rather than during them. Placed in authority, his vigor and activism were often a source of terror to his subordinates. (Some referred to him privately as "El Toro.") When one had been overwhelmed by his stream of arguments

(it wasn't enough for him to order, he felt impelled to convince) and had agreed to do something that was wholly unreasonable, one would hope that Smith would forget what had been promised. And the hope was often (not always) realized.

Once when I was auditing his class (for a time he hoped I would under-study him, but ultimately let me go my own way), he made a particularly sweeping statement, then turned to me and asked, "Isn't that so, Professor Simon?" My father had carefully taught me, "Never sign in the presence of the salesman," and I had learned that valuable lesson well, to the point of reflexive response. Recovering from my momentary shock, I quickly replied, "On the whole, it seems reasonable." Smith turned to the class and, his voice dripping with sarcasm, said, "Professor Simon says on the whole it seems reasonable. *I* tell you *it's so*."

As head of the Industrial Management Department, I had occasion to confer with Provost Smith frequently. Finding myself often unable to meet his arguments in "real time," I soon devised a defensive stratagem: I always brought along another colleague when I was summoned to the provost's office. It was the task of the colleague to wave a distracting conversational red flag at Smith when he saw that I was at the point of agreeing to something I ought not accept. That made life simpler for all of us.

I have painted Elliott Smith as a character, one who was rather difficult to live with. But he was difficult mainly because he expected you to think deeply and well about education, to justify your views on first principles, and to carry them into well-designed practice. Moreover, the first principles on which he himself operated, however idiosyncratically expressed, were almost all quite sound. Hence he had an enormous and lasting positive impact on education at Carnegie Tech and ultimately, by imitation and borrowing, at many other institutions. Those of us who had any considerable contact with him regard him as a major influence on our own educational thoughts and ways, and find ourselves frequently quoting and plagiarizing him, nearly forty years later.

From observing Elliott Smith I learned that being a decent person is terribly important, but being a "nice guy" is not important at all. Elliott Smith never attained high office. He was passed over for the presidency of Carnegie Tech when Bob Doherty retired, and he never achieved the influence with Doherty's successor, Jake Warner (who placed more emphasis on research than teaching), that he had with Doherty. He retired with some feelings of bitterness and failurethe latter, I think, quite unjustified. He would probably have made a terrible chief executive for any organization, but he was much more influential, by virtue of ideas, not position, than are most chief executives. I suppose my own reluctance to go the managerial

route was partly influenced by my observation of Elliott Smith's career, and by comparison of my human relations skills with his.

The New Model Business School

Having weathered its first crisis, and holding to its social science emphasis, GSIA quickly gained national visibility as the new model for a business school. European universities, moving cautiously into business education for the first time, generally found the scientism of GSIA a more comfortable model than the unfamiliar case method of the Harvard Business School. Two national studies of business education (whose directors came from liberal arts backgrounds and were extremely skeptical of business education in general) picked GSIA as the example for other business schools to imitate (R. A. Gordon and J. E. Howell 1959; F. C. Pierson et al. 1959).

Within about five years, GSIA came to be regarded as one of the two or three best business schools in the nation. To avoid constraining our ability to innovate, we did not seek national accreditation until our reputation was so well established that the accrediting body could not put pressure on us to conform to conventional ideas.

The status of the school at the end of 1957 can be illustrated by the supremely arrogant letter I wrote to Chancellor Lawrence A. Kimpton of the University of Chicago on December 26, 1957, and his remarkably gracious reply of January 6:

Dear Chancellor Kimpton:

Certain passages in your recent message to alumni were reminiscent of the contempt for extra-Chicago academia that was so often exhibited in Bob Hutchins' utterances. The particular barb that stung most sharply, of course, was the reference to business schools. That the business schools need reform is no secret. But your comment about "even the best" suggests that Allen Wallis hasn't informed you very fully about what has been done over the past decade in the Graduate School here at Carnegie, more recently at MIT, UCLA, and to varying degrees at other institutions. In fact, even the Harvard Business School, whose conception of reform would differ considerably from ours, has been moving at least as rapidly as Chicago toward bringing itself into closer relation with fundamental work in the behavioral sciences and economics.

When you announce that the Business School at Chicago is henceforth going to stress quality, fundamental social science research, and a quantitative approach to business problems I guess I do not really expect you to append a footnote acknowledgment to Carnegie as the origin of this con-

ception. However, if you or Allen would like a concrete picture of what your school will look like when such goals are realized, we will be most pleased to welcome you to our campus. Sometimes, in our immodesty, we think that we have done for business education here what MIT did for engineeringif it was MIT that did it.

But if there is disappointment in the fact that a fine new invention has been anticipated, perhaps there is consolation in the fact that most of those responsible for the anticipation were products of the Chicago of Bob Hutchinsincluding Lee Bach, Bill Cooper, Jack Coleman, Harold Guetzkow, and me. Perhaps it was because we acquired at Chicago some of the same brashness that I complain of in Hutchins that we felt as free as we did to flout tradition.

With best wishes to you and Allen in bringing the Business School at Chicago into partnership with the effort to make this stem of university education as fundamental and vital as the best of the other professional schools, I am

Sincerely yours,
Herbert A. Simon
Professor of Administration

Dear Professor Simon:

I appreciate your letter of December 26, 1957. Occasionally I let my prose run away with me, and I certainly did on my statement on business schools. I have an enormous admiration for what you and your colleagues have done at Carnegie Institute of Technology. Since we have spent a lot of time in the last few years trying to hire you and Lee Bach, and a number of other people from your Business School, I think our actions prove how false my flippant words are.

I appreciate your good wishes on the development of our Business School. Allen is doing a superb job, and I should confess to you that I told Jake Warner [President of CIT] the other day if only our Business School could begin to rival yours in quality, I would feel that we had made a great success. Please forgive me.

Very sincerely yours,
Lawrence A. Kimpton

The Ford Foundation, seeking to improve business education, used its golden carrots to push and pull other business schools toward the road GSIA had pioneered. GSIA faculty staffed a number of summer schools that the Foundation financed for business school faculty who wanted to learn about the new methods. GSIA students regularly won the Foundation's

annual awards for the best business school dissertations and its graduate fellowships. The Foundation also funded some of our research (though never on the scale we thought we deserved for our services to it). Many of our doctoral alumni soon became deans of other business schools.

The Setting and the Culture

Since I never met Henry Hornbostel, the architect of the Carnegie Institute of Technology campus and first head of the Architecture Department, this tale is not about the man but about his buildings. But, having lived in and around his buildings for forty years, perhaps I also know the man. I do not know whether, in building the campus, beginning in 1904, he stayed within budgetfortunately the bills were paid generations before my time. What I do know is that he was a man of great imagination and that my gradual awareness of the beauty of the campus buildings has been a source of substantial pleasure to me during my sojourn here.

When I first saw the CIT buildings, they seemed quaint, with their Beaux Arts symmetry and decorative detail. In spite of my experiences with Mies von der Rohe's department at Illinois Tech, I was rather addicted to the Bauhaus and the International styles. Hornbostel was a disciple of the sixteenth-century Italian architect Andrea Palladio, or more accurately, of the fifteenth-century Leon Battista Alberti. The campus was laid out in a large open quadrangle, its long axis running east to west along the boundary with Schenley Park and having a distinct downward slope of about four degrees.

The east (upper) end was bounded by the Fine Arts Building, an Italian palazzo, Hornbostel's jewel, the floors inlaid with mosaics showing the floor plans of celebrated buildings, and the facade that faces the quad decorated with a series of empty niches intendeduntil the budget ran outto be filled with sculpture. The west (lower) end was bounded by Hammerschlag Hall, an engineering building that was also the power plant. The tall chimney was encased in a splendid cylindrical Italianate campanile, so that I always thought of the building, especially in winter when smoke and steam billowed from the campanile, as the Samovar. It is exquisitely proportioned, the facade, facing the quad, showing a strong resemblance to the facade of San'Andrea di Mantova, one of Alberti's masterpieces.

On the south, or Schenley Park, side of the quad are Baker and Porter Halls, the one joined to the other by a 500-foot central corridor. Since the axis of these buildings slopes, the buildings have to slope, so that the corridor has a grade of several degrees, making roller skating from east to west a great sport. You can get a good idea of the first-floor corridor, viewed from

the east portal, from Vincent van Gogh's well-known painting of the interior of his insane asylum, Saint-Paul Hospital at Saint-Rémy. Near the east entrance of Baker Hall is a beautiful, tiled circular staircase.

Of course the floors in Baker Hall don't slope (although you have to remember to step down when you enter my office from the adjoining secretary's office). Planarity is achieved by locating most of the space in wings that lie at right angles to the main corridor. The arched openings to these wings, as indeed all the doorways, are banded with thick strips of black iron, a constant reminder of the economic base for Pittsburgh and Andrew Carnegie. The exteriors of all the buildings and the interior corridors are lined with yellow brick. Originally, the roofs were of red tile, but we had to install a less costly material when they were replaced a few years ago.

There is much more I could say about these original campus buildings, but perhaps I have conveyed a sufficient idea of them. If my description sounds quirky, even irreverent, it conveys correctly the first impression the campus has on most people. Seeing the buildings constantly, living in them, one slowly learns that they are masterpieces. Perhaps the recent architectural fad that erected Palladian structures with their round-arched windows in every city in the land helps in the instruction. But I had discovered that they were masterpieces before then.

One of the University of Chicago Aristotelians, whose name, I think, was Nils Fuqua, had a habit, whenever some aesthetic heresy was uttered in his presence, of saying, "Hear more Mozart!" So I say, "See more Hornbostel!" You don't even need to come to Pittsburgh. He designed the Oakland, California, City Hall (which unfortunately is to be demolished after being damaged by the 1989 earthquake), and the architectural treatment for the Hell's Gate Bridge, the large arched railway bridge that is visible on your right as you cross the Triborough on your way to Manhattan from La Guardia Airport. Come to think of it, it was a picture of that bridge in an atlas I owned as a child that first entranced me with the beauty of Hornbostel's style.

My description of GSIA and its programs, and particularly my account of its early administrative crisis, may well astonisheven shockreaders whose experience of academe began during or after the troubles of the civil rights movement and the Vietnam War period, or whose academic lives have not brought them in contact with institutes of technology. In the United States, institutes of technology, including the engineering and agricultural schools of the land grant colleges, have had rather different traditions from the liberal arts colleges, and even from the nonprofessional divisions of research universities.

The liberal arts tradition (if not always the practice) is one of extreme

decentralization. In its limiting form, a faculty is a collection of individuals, each expert in his or her own field and each possessing an autonomy called academic freedom. To be sure, collective decisions have to be madenew faculty selected, students evaluated for admission, curricula formulated, and so onand most of them are made with broad faculty participation.

For administrative convenience, the faculty is organized in departments along disciplinary lines (that is, members of a department generally hold their degrees in the same discipline, have had very similar training), and the chairman of the department is *primus inter pares,* most often elected by the members of the department. While curricula are determined by group processes, as they must be, individual faculty members organize and conduct their classes as they see fit, with a minimum of coordination with other classes.

In universities and colleges where research is part of the role of faculty, the researcher chooses his or her own topics. Senior researchers in major research universities are often, and quite correctly, described as feudal barons, whose academic freedom consists in the right to run their baronies without much interference from higher authority. Usually they acquire that independence by their entrepreneurial activities in raising most of their own research funds, and they are delegated broad authority to manage their research teams.

Of course, what I have described are ideal types. Few colleges operate precisely in this fashion. Presidents in small colleges, and deans and department heads in larger ones, have more than one votesometimes the only votein personnel and salary decisions. In universities, large introductory courses are sometimes taught by teams, who have to coordinate their plans and efforts. My earlier description of the University of Chicago Department of Political Science under Charles Merriam makes clear that it was less than a pure democracy.

At the time I came to Carnegie, institutes of technology lived much closer to traditions that came out of industrial practice. The administrative hierarchy was clearly defined, and deans and department heads, usually appointed from above after a modicum of consultation with their subordinates, had real authority. It was not unthinkable for research areas to be defined by a department for its members, or for faculty members to be urged or persuaded to aim their efforts in particular directions.

Carnegie Tech, and GSIA within it, clearly belonged to the tradition of institutes of technology, not the liberal arts tradition. Those of us who had responsibility for leadership in GSIA had no doubts or qualms about our duty (right?) to define research and teaching goals for the school. The research memoranda of 1950 and 1952, to which I have referred, are examples

of the way we went about discharging that duty. The research directions we outlined were also much in our minds when we recruited faculty. We were looking for able teachers and researchers who would find these kinds of missions challenging; we were not expecting them to define their missions wholly independently.

This perception of the world was congenial enough to most of the faculty members recruited on the industrial management side of our roster. Our environment and assumptions were not noticeably different from those of other business and engineering schools. It was far less congenial to faculty members recruited from psychology and economics, disciplines that usually reside in the liberal arts colleges of their universities and that do not often gain access to the large-scale research funding that produces baronies. So part of what we were seeing, in the tension within GSIA, was a conflict between two academic cultures that had different definitions of the academic role and of academic freedom.

That Lee Bach, Bill Cooper, and I came out of the liberal arts culture (the same University of Chicago in all three cases, in fact) but accepted the premises of the other culture requires explanation I do not feel fully able to provide. The fact that we were the leaders no doubt made us more willing to define the leadership role broadly. The fact that we had substantial funds for research from both internal and external sources required us to manage the activities that used those funds.

As a consequence, neither teaching nor research were areas of full faculty autonomy. The organization had more than a little concern and involvement with the content of both. We frequently drew conclusions about the particular talents of young faculty members, and advised them to move in one direction rather than another. Merton Miller, for example, who has had a very distinguished career in the economics of finance, had no initial intention of going into that field. When we convinced him that the area was underpopulated and that his abilities might shine there even more brightly than elsewhere, he prepared himself with great effectiveness to move in that direction. Such advice is not unknown in the world of liberal arts colleges or research universities; it simply was more common in our environment.

I hope that a note of apology has not crept into my voice as I describe these administrative arrangements. They have always seemed to me good for a university, enabling it to pursue goals vigorously, to innovate, even to change the world in modest ways. In such a university, I find the roles of both follower and leader generally comfortable, although I must confess that I have had more experience with leading than with following.

Academe had changed in some fundamental ways since World War II. One change has been toward gradual grass-roots democratization even of

those schools that descended from the tradition of institutes of technology and professional schools. The democratization was greatly accelerated by the student Troubles of the late 1960s and early 1970s.

Today, in most universities of even modest distinction, a dean cannot be appointed without strong support from the faculty of the college, nor a department head without departmental consensus. Even in the case of the university president, the views of the facultyespecially if it unites behind a candidateare likely to be as important as the views of the Board of Trustees, at least important enough to impose a veto on an undesired candidate. The last time a president of Carnegie was appointed from above with minimal faculty participation was in 1965.

It is harder today to bring about real innovations in a university than it was before the democratization took place. I regret that, and think the price of organizational democracy has been high. Moreover, I do not believe that this kind of democracy within organizations has any connection with, or relevance for, democracy in the society at large. But that topic is beyond my purposes here.

Chapter 10
Research and Science Politics

Not all of my time after I came to Carnegie Mellon was devoted to organization and institution building. This chapter is devoted to a discussion of our research there and my professional activities outside the university.

Studying Organizations

During the first six years of my research at GSIA, I filled out, empirically and theoretically, the decision-making framework I had laid down in *Administrative Behavior*. My files yield a planning document that Harold Guetzkow and I wrote on February 28, 1952, probably the basis for a grant proposal, outlining a five-year plan for research in organizations. With *Administrative Behavior* as its theoretical starting point, it proposed field and laboratory research as well as theoretical studies, emphasizing the need to bring together empirical findings from many sources, not just our own work, in order to build theory.

The most interesting substantive recommendation in the document was that decision making in organizations should be related to learning theory: "Our work has led us to the conclusion that there is an intimate connection between organizational structure and the learning of frames of reference and roles by members of organizations." The source of the idea could have been a combination of Harold Guetzkow's previous psychological research on the topic of "set," or frame of reference, and my experience in helping organize the Economic Cooperation Administration a few years previously. The idea foreshadowed the critical importance of reference frameswe would now call it "problem representation" or the "frame problem"in problem solving and learning. Problem representation is still high on the

agenda of research in cognitive science today, thirty-nine years after the date of that memorandum.

Even before our planning memorandum was written, we had begun our first big empirical study, the "Controllership Study" (Simon et al. 1954). It took us into factories and sales offices to see what use was made by operators of blast furnaces and sales managers, as aids to their decision making, of the company accounting and cost accounting records and of the services of the accountants.

The Controllership Study was an adventure to me, both watching George Kozmetsky (whom we hired from the Harvard Business School as a young Ph.D., and who already had extraordinary facility in analyzing accounting records) extract information from our respondents, and trudging through the reddish brown dust of the National Works of U.S. Steel in McKeesport to learn how a steel mill was actually managed and how its decisions were made.

In the Controllership Study, as in the later empirical work on organizational decision making we did in GSIA, I worked with a great many colleagues, the nature of the collegial relation being a mixture of those I had experienced in Berkeley and at Illinois Tech. As at Berkeley, a number of my co-workers were young academicians who had joined our group with the understanding that they would be associated with such research projects. I was their colleague, but also their project leader. Of my co-authors during this period, eight fall in this category.

Four other co-authors were more senior faculty colleagues, my relations with them being similar to my relations with Don Smithburg and Vic Thompson, with whom I wrote the public administration book at IIT. In addition, during the first five years at GSIA, I published papers with eight students, only two of whom (Ed Feigenbaum and Allen Newell) were my own doctoral students. Following the general custom of that time in political science and economics, I did not generally co-author papers based on the doctoral dissertations of my students, and even in recent years have done so only half a dozen times, and at the student's invitation.

At Carnegie, the project team usually met weekly to report progress, assign tasks, and, most important, to discuss the research. In the project on organizational decision making, we recorded and circulated minutes of these meetings, and in all the projects members of the team prepared frequent working papers, submitting their ideas to criticism and discussion with their colleagues.

Outside the projects, my collaborations have usually involved a single person, occasionally as many as three or four. Again, we hold regular weekly meetings, as I do with my graduate students. With individuals, the meetings

are usually scheduled for an hour, with working groups, they may last several hours. Each member of the collaboration spends much more time in individual empirical or theoretical work between meetings than at the meetings.

Initial drafts of papers that are ultimately published may be written by any of the collaborators. Since I write easily and fluently, I probably do more than my share of the writing, and certainly of the final editing, of papers of which I am co-author. Names on publications are alphabetical, unless one or two members of the team are clearly principal authors.

Throughout the rest of my career, I have continued to work with many students and faculty colleaguesI have had more than eighty co-authors in allbut not usually within the framework of large empirical projects involving many people. The years at Berkeley and the first five years at Carnegie were my main experiences of large-scale empirical field work.

The Controllership Study was followed by a Ford-funded project that involved detailed case studies of specific situations in companies that gave us access to their decisions in the making. Both Dick Cyert and Jim March were attached to that project, and in that way began the collaboration that led to their pathbreaking book, *The Behavioral Theory of the Firm,* in 1963.

In the minutes of the working sessions in which Harold Guetzkow, Jim March, I, and others participated, our language for describing decision-making processes developed in an interesting way. We had long talked about "decision premises" and "islands" of such premises that could be communicated from one manager in an organization to another as inputs to decisions. When Harold brought to these sessions his earlier research on problem solving, we began more and more to see decision-making processes as essentially the same as problem-solving processes. My own economic theorizing was leading me in a similar direction. Hence, as early as 1951, the language of problem solving began to creep into the minutes of our working sessions.

Our growing interest in problem solving led us to restudy the writings of psychologists who had done research on that topic, especially Gestaltists such as Norman Maier, Max Wertheimer, Karl Duncker, and George Katona. This was a first tentative step on the road that led soon afterward to my collaboration with Al Newell and Cliff Shaw in building a computer simulation of heuristic problem solving.

Jim, Harold, and I also undertook to produce a "propositional inventory" of organization theory that Bernard Berelson of the Ford Foundation commissioned. Jim March and I co-authored, with Harold's help, a book called *Organizations* (1958), which represents our interpretation of that assignment. It was successful in systematizing organization theory, less successful in marshaling the empirical evidence to support its propositions.

In particular, though we were acutely sensitive to the need for using field research to test and extend our theories of organizational behavior, we did not know how to compare information from diverse case studies that used relatively informal and wholly unstandardized methods for gathering and analyzing data. Much research effort was subsequently aimed at that problem at Carnegie, but we cannot say that we learned to handle all, or most, of the methodological difficulties. Perhaps our foremost contribution has been to show how human thinking-aloud protocols can be used as objective data, especially in conjunction with computer simulations.

Berelson did not think our book contained much of a propositional inventory, but the theory that it does contain has aged well. Jim and I have talked a few times about revising it, but have agreed that it would be difficult to come together again from our diverging professional paths. (Perhaps each of us should issue an independent second edition and see which sells better!)

Harold left in 1957 for other, presumably greener pastures, and Jim followed in 1964. After 1956 I was considerably diverted by the new computer simulation work. In 1961, Dick Cyert became dean of GSIA, greatly curtailing his research time. Since our research on organizational decision making was nearly unique, at least in the United States, we were generally unsuccessful in recruiting replacements for the original quadrumvirate who had provided the leadership. They simply were not being produced by other Ph.D. programs.

By the early 1960s, the Golden Age of organization theory and the behavioral theory of the firm had ended at Carnegie Institute of Technology. As we shall see, GSIA came to be dominated by research on sophisticated mathematical techniques in operations research and economics and by neo-classical economic theory. The economists' aborted revolution of 1951 achieved a large measure of success in the 1960s.

The Mathematical Side

In my management science and econometric research, I retained my consulting ties and close contacts with the Cowles Commission and, through it, with the RAND Corporation, then enjoying tremendous success and visibility as a new way of enlisting research talent to help advance applied goals. Beginning in 1952, as I discussed earlier, I made frequent trips to RAND in Santa Monica and spent a number of summers and one full year (1960 61) there during the following decade.

At Carnegie Tech, my management science research, as distinguished from

my research on organizational behavior, was part of Bill Cooper's project, as explained in chapter 9. The plans for that project were laid out in a basic planning memorandum dated February 21, 1950, a year and a half prior to the crisis I have told about. Under the heading of "production technology," both the Cooper-Charnes linear programming work and the Holt-Modigliani-Muth-Simon* factory scheduling research were foreshadowed.

There was also a good deal of emphasis on the effect of financial data on decisions, an area of special interest to Bill Cooper. This led ultimately to such products as Charles Bonini's computer simulation of a business firm, which demonstrated how changes in the accounting system could trigger changes in operating decisions.

My own mathematical and econometric work at this time had a number of themes, both substantive and methodological. A good overview of it can be found in *Models of Man* (1957a), a collection of some of the papers I wrote during my first five years at Carnegie. Part I contains papers on causality, deriving from my work with the Cowles Commission on the identification problem. Part II undertakes to show the utility of making mathematical translations of several current theories of social interaction (the theories of George Homans and Leon Festinger). Part III proposes several models that explicate how Barnard's and my organization theory can be related to the economic theory of the firm. Part IV formalizes and explores the concept of bounded rationality. Four chapters deserve special mention for their influence on subsequent events in my life and in the economics profession.

Chapter 14 of *Models of Man,* "A Behavioral Model of Rational Choice" (first published as 1955a), mostly written in 1952 during my first RAND summer, represents my first major step toward formalizing the psychological theory of bounded rationality. Although the term *satisficing,* a key concept in my subsequent work, was not used in it, the satisficing conceptsearching for "good enough" actions rather than optimal onesis already present. Of all my writings on this topic, the "Behavioral Model" paper comes closest to the mathematical format with which economists are comfortable. Hence, economists who wish to refer to bounded rationality and satisficing most commonly choose this paper for citation.

What made the paper distinct from most contemporary economic writing was its explicit concern for the process of making decisions, for procedural and not just substantive rationality. Because of its concern with process,

* The economist Charles Holt came to GSIA from the University of Chicago and Franco Modigliani from the University of Illinois. John Muth was one of our own graduate students.

the paper also represents a first step toward computer simulation of human behavior. The manuscript contained an appendix, never published except as part of a RAND technical report, outlining how a chess-playing program for a computer could compensate for its bounded rationality by using selective search guided by heuristics (see note 4 of the published paper). This proposal for a heuristic chess program is an extension of ideas that Claude Shannon had published in 1950, but my emphasis was on human simulation rather than chess prowess.

What motivated this part of the work was a talk that John von Neumann gave at the 1952 RAND summer seminar, emphasizing the difficult problems that had to be solved in order to program a computer to play good chess. I thought von Neumann overestimated the difficulties substantially, and moreover I believed that I had some solutions for them which I proposed in the appendix to the RAND technical report. This chess discussion disappeared from the final manuscript of my paper. Perhaps referees thought it irrelevant or perhaps I excised it when Al Newell began to carry the chess ideas much farther as he developed his 1954 paper on the subject.

Chapter 15 of *Models of Man,* "Rational Choice and the Structure of the Environment" (first published as Simon 1956), was a companion piece to chapter 14. Adopting again a satisficing point of view, it provides a sort of Darwinian model of rationality. Bracketing *satisficing* with *Darwinian* may appear contradictory, for evolutionists sometimes talk about survival of the *fittest*. But in fact, natural selection only predicts that survivors will be *fit enough,* that is, fitter than their losing competitors; it postulates satisficing, not optimizing. The paper showed how relatively simple choice mechanisms could enable an organism, searching through its life's maze, to survive in an uncertain environment in which several incommensurable needs had to be met. It depicted a procedural rationality for organisms that was squarely based on satisficing rather than optimizing.

Chapter 15 had a curious by-productthe only short story I have ever written. Since no one has ever told me that it has any literary merit, it should probably be read as philosophy, not literature. It uses the metaphor of the maze to explore the relation between satisficing and basic human values, a free translation of chapter 15 from model to metaphor; I will recount it in the next chapter.

Chapter 11 of *Models of Man,* "A Formal Theory of the Employment Relation," was a harbinger of the New Institutional Economics that has been nurtured in recent years by Oliver Williamson and others. In it I attempted to cast some aspects of organization theory in the mold of neoclassical reasoning. In this sense it is reactionary, a throwback. In my 1977

Ely Lecture before the American Economic Association, I apologized for it thus:

> In my 1951 paper, I defined the characteristics of an employment contract. . . . My argument requires a theorem and fifteen numbered equations, and assumes that both employer and employee maximize their utilities. Actually, the underlying functional argument is very simple. . . . The rigorous economic argument, involving the idea of maximizing behavior by employer and employee, is readily translatable into a simple qualitative argument that an employment contract may be a functional ("reasonable") way of dealing with certain kinds of uncertainty.

Thus, most of my theory of the employment contract can be expressed without either equations or maximization. But in that qualitative form it would not have captured the attention of economists, who, in the "new institutional economics," continue to pour the new wine into the old bottle of neoclassical reasoning. At least we have some new wine.

Chapter 13, "Application of Servomechanism Theory to Production Control," started Charles Holt and me off on our operations research venture in scheduling a paint factory. More accurately, it led us to a new operations research tool for dealing with a broad class of dynamic planning problems under uncertainty, for which the paint factory provided an application.

By making strong approximating assumptions about costs, we were able to solve an exact maximization problem with little computation. That is to say, we satisficed by finding the optimal policy for a gross approximation to the real world. The dynamic programming project later was joined by Franco Modigliani, and by several graduate students, including John Muth. Its main direct product was the Holt, Modigliani, Muth, and Simon book, *Planning Production, Inventories, and Work Force* (1960). It also had an indirect productrational expectationswhich belongs to the next segment of the chronicle.

The Systems Research Laboratory

Apart from the consulting relation that took me to the RAND Corporation in 1952, another aspect of my association with RAND from 1952 to 1955 has a great bearing on what followed.

During the GSIA period up to 1952, while we were busy with our studies of decision making in organizations, four peopleJohn Kennedy, William

Biel, Robert Chapman, and Allen Newell (all in the Santa Monica part of the forest)conceived the grand, or grandiose, design of studying in the laboratory the behavior of an air defense organization. The laboratory (christened SRL, the Systems Research Laboratory) would simulate an entire air-defense early warning station, staffed with perhaps fifty men. The U.S. Air Force was to supply the budget and the airmen-subjects for the simulation.

When, in the planning stages of this imaginative project, the group came to me for advice, because of my previous experience in conducting the California SRA experiment and other organizational studies, I became a consultant; and, on my first visit to RAND early in 1952, I met Allen Newell. I had not known him previously, but I was familiar with a couple of mathematical documents he had written, in which he tried to formalize organization theory. I was only mildly impressed by the mathematics, which seemed to me to contain more definitions than theorems (always a bad sign for formal theories), but I was well disposed to anyone who had the disposition and skill for applying mathematics to these kinds of questions.

In our first five minutes of conversation, Al and I discovered our ideological affinity. We launched at once into an animated discussion, recognizing that though our vocabularies were different, we both viewed the human mind as a symbol-manipulating (my term) or information-processing (his term) system.

The SRL experiments provided the most microscopic data one could want on how radar operators and air controllers made their decisions. A vast body of such data was accumulated from a series of experiments conducted over three years, during which all of the communications between subjects were recorded. Yet Al and I suffered from continuing frustration in trying to write formal descriptions of the process. Somehow, we lacked the necessary language and technology to describe thinking people as information processors. As a result, we were never able to produce a good analysis of the phenomena we observed in the SRL experiments, and only one relatively innocuous paper was ever published about them.*

The frustration that Al and I experienced with the SRL experimental data had major consequences, the first of which is reported in chapter 12 of this volume. In simplest terms, it determined the rest of my life. It put me in a maze I have never escaped fromor wanted to.

* SRL was not all futility, however. If it made little substantive contribution to basic science, its technology was subsequently more fully automated with the help of computers, and its training program was spun off to the Systems Development Corporation, which for many years was responsible for training military personnel to operate the Dew Line air defense system and later systems.

The Politics of Social Science

My gravitation toward the sun soon led me into professional activities in political science, and in social science generally, outside the university. Since my involvement in science politics began while I was at Illinois Tech, I will return briefly to that scene, beginning the tale just at the end of World War II.

In view of my education as a political scientist, it is perhaps not surprising that I devote considerable space here to the politics of science. My activities in this domain divide into two parts, the first concerned with the social sciences, the second with the relations between the social and natural sciences. I tell the social sciences part of the story here, the social and natural sciences part in chapter 19, as it relates more closely to events that took place after my election to the National Academy of Sciences in 1967.

Political Science

I entered into the world of science politics as a political scientist—that is to say, into a world quite segregated from natural science ("real" science, some would call it). During my tenure at Illinois Tech, I was active in the American Political Science Association, as one of the Young Turks fomenting the behavioral revolution in that discipline whose origins were recounted in chapter 4. *Administrative Behavior* and the published articles that had preceded it gave me some standing among the insurgents, but we had little success in finding a credible senior to lead us (V. O. Key, who was our candidate, was a gradualist, more or less committed to behavioralism, but not to revolution).

All I now recall of this activity was meeting with other conspirators in my hotel room at the (then new) downtown Washington Hilton during an APSA convention; and defeating the re-election of the incumbent APSA secretary at a later APSA meeting in Chicago. Our goal was to make sure that research within the behavioral framework would be received sympathetically by the *American Political Science Review* and would have an appropriate place in the annual meetings of the association. The behavioralists gradually captured the association, but it was the passage of time and the retirement of the old guard rather than revolutionary activity that accomplished the change.

The Ford Foundation

About 1951 Bernard Berelson came to consult me about the prospective program of the new Behavioral Science Division of the Ford Foundation, including a very tentative plan for a training center. Arnold Thackray (1984), in a historical account of the Ford program, notes that I was early designated as a potential adviser, possibly because I was acquainted with Don Marquis of the University of Michigan, one of the framers of the program.

My first advice to Bernie, given at a meeting he had called in Chicago, was that the Behavioral Science money of the Foundation should be used for research grants, and not for such boondoggles as a training center. The most useful role for the program, I thought, was to fill the gap that had been left by the omission of the social sciences from the charter (and hence the funding) of the National Science Foundation.

The advice I offered at the Chicago meeting was followed up by a long letter of December 10 to Berelson. I argued that "the rest of the Ford Foundation program is so heavily oriented toward application that Program Five resources should be jealously guarded for fundamental work." I again expressed my opposition to the training center idea, opining that it would be much better to distribute the money among three or four institutions, to free a few senior faculty in each for research and working with talented advanced students. These schools should be required to cooperate so that promising graduate students could divide their doctoral work among several institutions, and the Foundation should "season the brew with a competitive fellowship scheme at the undergraduate and graduate levels."

Expressing a concern I had long had that students are seldom exposed to the social sciences before fixing on a career in natural science, I proposed making grants to permit a few institutions "to experiment with procedures for attracting to the social sciences at an early stage in their training a few men with outstanding research potential." I also commented on the Foundation's proposal to support interdisciplinary research:

> The problem is less one of bringing unlike social scientists together than one of bringing unlike social sciences together in one man. There has been failure after failure of interdisciplinary "teams" to integrate anything . . . except to the extent that individual team members became interdisciplinary. I would not give a dollar to assist a typical political scientist to collaborate with a typical economist unless each one of them gave me a sworn statement that he would study seriously and not in a dilettante's way the discipline of the other for at least a year.

After more than three pages of such criticism and comments, I expressed the

hope that, in spite of these points of disagreement, I conveyed to you in Chicago my enthusiasm for the general objectives and emphases you have pointed up in the program. . . . [I feel we have] the same basic convictions as to . . . the job . . . to be done: to provide the trained human talent that is needed for the progress of the behavioral sciences, to facilitate the cumulative and interactive development of theory and empirical research, and to build research progress around the core disciplines . . . represented by social psychology and sociology.

But the Ford Foundation was already more than half committed to creating a Center for Advanced Study in the Behavioral Sciences (CASBS), so I was soon a member of the committee that was helping Berelson to plan it. The idea of such a center had emerged from the fertile mind of Paul Lazarsfeld of Columbia University (a long-time associate of Berelson), who conceived of it as a place where young postdocs would come to sit at the feet of the masters and learn good methodology.

As this seemed to me (and others) a terrible idea, we gradually turned the plans around to eliminate the proposed bimodal distribution of "juniors" and "seniors," and substitute a cadre of scholars distributed over the whole range of age and seniority. The Center would be a place for research and writing, rather than for postdoctoral training, and it would recruit the best social scientists it could find, who would spend a year there, preferably at a time when they had completed a major piece of research and were in the reflective and writing-up stage.

Part of my reason for opposing the original plan were my doubts that the elders were the persons best equipped to teach good social science method, and that the postdoctoral "juniors" would want to continue to sit at their feet (I was about thirty-five at the time). Perhaps more important, I thought that there was no lack of social scientists who knew how to do good empirical and theoretical research. What was lacking was money to support it. If there was to be a Center at all, its resources should be devoted, so far as possible, to subsidizing research. This change in the plan for CASBS destroyed Paul Lazarsfeld's dream, and although we remained friends to the end of his life, he never quite forgave me my part in its destruction.

On another issue I was less persuasive. After several possible sites for the Center had been considered, the hill above the Stanford campus was selected—a beautiful spot in Lotus Land that I would not have chosen to enhance productivity. This had the unforeseen (or was it unforeseen?) con-

sequence that Stanford University was able in the 1950s and 1960s to assemble a formidable social science faculty from California-smitten veterans of the Center.

The main consequences for my personal career of this involvement with the Ford Foundation and CASBS was that I became visible throughout social science circles and acquired an enormous amount of information about the current research picture, especially the names and numbers of all the players. Another consequence was that, by virtue of my strong opposition to the Center, I always felt embarrassed at the idea of spending a year there, and never did. (There were other reasons for my reluctance, toonotably its lack of suitable computing facilities.)

The Social Science Research Council

During the 1950s I was invited to membership on a couple of committees of the Social Science Research Council (SSRC), an organization that had been created in the 1920s by Charles Merriam and others to encourage interdisciplinary research in social science. A Committee on Business Enterprise Research brought together economists interested in the business firm with sociologists and others interested in organizations. Some years later, a committee of three (Carl Hovland, George Miller, and myself) was set up to administer a small grant from the Ford Foundation for what we would now call cognitive science.

In 1958, I was invited to join the Council's Board of Directors, continuing to serve on it until 1971 (incuding five years as chairman). During most of that time, Pendleton Herring was president of the council. At the top of my agenda for SSRC I placed the support of mathematical training for social scientists and the erosion of boundaries between disciplines.

As the council had been pioneering since 1952 in activities aimed at raising the level of mathematical sophistication in social science, on this topic I simply supported what was already going on and encouraged the expansion of *mathematical* to include computer simulation. The council's committee on mathematical training for social scientists was later spun off as an independent entity, mostly funded by the National Science Foundation. This committee, over the years, was highly effective in diffusing mathematical skills, particularly in sociology and political science (mathematization was already proceeding apace in economics).

The encouragement of interdisciplinary activity had been a principal motivation for founding the Social Science Research Council. But at the meetings of the council, I was appalled at how often I heard such phrases as,

"as a historian, I . . . ," "as an economist, I . . . ," "s a sociologist, I . . . ," and so on. I challenged these phrases each time I heard them, but it was like trying to purge *ainnuh* ("Isn't it so?") from the lexicon of a native of Milwaukee.

My free-floating experiences at the University of Chicago, and my many interdisciplinary contacts at the Cowles Commission and RAND, had not prepared me for the fierce disciplinary loyalties I encountered in the heart of the social science establishment. I came to see that disciplines play the same role in academe as nations in the international system. Academicians typically live out their whole careers within the culture of a discipline, rarely shaking off the parochialism this isolated existence engenders. (Still later I learned from my encounters with economics that disciplines undertake imperialistic adventures with the same zest that nations do.)

As best I could, I encouraged those activities of the council that cut across boundaries, and questioned activities that seemed more appropriate for the professional associations of the disciplines. My efforts had, at most, modest success. In 1966, the year before I took over the chair of the council, there were ten committees that were genuinely interdisciplinary, but at least six whose activities lay within single disciplines. That ratio didn't change much over the years of my activity in the council.

But there was a bigger problem. The Ford Foundation had become deeply enamored of area studies, which produce experts in Russian or East Asian or Latin American or African affairs. Almost all of its grant money, and especially its fellowship money, was being diverted in this direction. Funds for the council's traditional fellowships for interdisciplinary training had almost dried up. By 1966, the council already had nine committees dedicated to area studies, not counting the fellowship selection committees for the training of area specialists.

But weren't these area studies just the sort of interdisciplinarianism I was looking for? Perhaps I judged them too harshly and from insufficient information, but I thought not. Too often, they seemed to aim at training disciplinary specialization *within* area specialization: experts on the Russian economy, the Chinese government, the Indonesian family. And because of the necessary emphasis on language skills, combined with a frequent attraction to current events, they seemed often to degenerate into high-grade journalism.

Arguments addressed to the Ford Foundation that this was not the way to go had no effect. Foot dragging had little more. ("Whose food I eat, his songs I sing.") At least, I thought, we should be making strong efforts to convert area studies into genuine social science. To do that, we should

establish committees for comparative research on important topics, drawing together the appropriate area specialists. That would impel them to conceptualize what they were doing at a higher theoretical level.

We never found the funding to do much of this, and I was never certain how much understanding and support I had from the council staff. When I made my periodic speech on this topic, heads nodded in agreement, but no visible action followed. I am sure they were more realistic than I about how movable our funders were.

As I look at the scene today, however, I see more comparative analysis among cultures, much of it sponsored by SSRC, and I am correspondingly more positive about the long-run effects of the area studies programs. I don't think I can claim any personal credit for these newer developments.

My service on the Board of the SSRC had little apparent effect on the direction of development of the social sciences, and probably was not highly productive. But it was educational to me, and extremely pleasant. In addition to its bimonthly meetings in New York, each autumn the council held a larger gathering at Skytop, a resort in the Poconos, where I had the opportunity to become good friends and engage in stimulating conversation with a great many leading social scientists.

In my activities with the Ford Foundation and with SSRC, I had my first experiences of seeking to influence large affairs, where it is never clear whether one's efforts have any result but where a result even of size epsilon could be important. (Epsilon times infinity can be a large number.)

Chapter 11
Mazes Without Minotaurs

In my 1956 paper, "Rational Choice and the Structure of the Environment," I wove around the metaphor of the maze a formal model of how an organism (a person?) could meet a multiplicity of needs and wants at a satisfactory level and survive without drawing upon superhuman powers of intelligence and computation. The model provided a practicable design for a creature of bounded rationality, as all we creatures are.

I was so pleased with the paper's account of rationality that a year later I found myself writing a short story, "The Apple," fashioned after it. The maze of my story, unlike the labyrinth of the Cretan myth, provides no heroics, no Theseus to seek out the fearsome Minotaur at its center and then escape by following the thread given him by Ariadne. Its central figure is not Theseus but Hugo, an ordinary man. The story describes Hugo's life, much like every human life, as a search through a maze. In doing so it strips the mathematical wrappings from the technical paper that provided its metaphor.

Some light is thrown on my preoccupation with mazes, and hence my urge to write "The Apple," by a conversation I had with the writer Jorge Luis Borges when I was in Buenos Aires in 1970.

A Conversation with Jorge Luis Borges

In December of 1970, Dorothea and I visited Argentina, where I was to give some lectures on management. In my correspondence about arrangements, I did something I have never done before nor sinceI asked for an audience with a celebrity. For a decade, I had admired the stories of Jorge Borges (I didn't then know his poetry), and had been struck by the role that

mazes played in them. I wanted to know why. I wrote to him (in English, since I knew he was fluent in it):

> My profession is that of social scientist, and I seek to understand human behavior by means of mathematical models (or, more recently, with simulation models programmed for computers). In 1956, I published an article which described life as a search through the corridors of a labyrinth, greatly branching and populated by a large number of goals to attain.
>
> A few years later I stumbled upon *Ficciones,** in particular the story "La Biblioteca, de Babel," to discover that you too conceive of life as a search through the labyrinth. I asked if ever there had occurred a comparable transmigration, from the inert body of a mathematical model to the live flesh of literature.

(I did not admit to Borges, then or later, that in 1956 I had also tried to manage a transmigration of the soul of my mathematical model into a short story. You will see the result of that attempt later in the chapter.)

I met Borges in his beautiful high-ceilinged baroque office in the Biblioteca Nacional. We had several hours of conversation (in English), of which I reproduce here only the portion relevant to labyrinths.

B_ORGES_: But I'd like to know why you are interested in having this conversation.

S_IMON_: I want to know how it was that the labyrinth entered into your field of vision, into your concepts, so that you incorporated it in your stories.

B_ORGES_: I remember having seen an engraving of the labyrinth in a French book when I was a boy. It was a circular building without doors but with many windows. I used to gaze at this engraving and think that if I brought a loupe close to it, it would reveal the Minotaur.

S_IMON_: Did you see it?

B_ORGES_: Actually, my eyesight was never good enough. Soon I discovered something of the complexity of life, as if it were a game. In this case I am not referring to chess. Perhaps I can express it with a poem:

I Have Become Too Old for Love

My love
  has made me old.

---

 * Ed Felgenbaum brought the book to my attention during the academic year 1960 61, when I was at RAND.

But never so old
as not to see
the vast night
that envelops us.
Something hid deep in love
and passion
still amazes me.

Here there is a play on words. In English, the word for "labyrinth" is *maze* and for "surprise," *amazement*. There is a clear semantic connotation as well.

This is the form in which I perceive life: a continual amazement; a continual bifurcation of the labyrinth.

Sɪᴍᴏɴ: What is the connection between the labyrinth of the Minotaur and your labyrinth, which calls for continual choice? Does the analogy go beyond the general concept?

Bᴏʀɢᴇs: When I write, I don't think in terms of teaching. I think that my stories, in some way, are given to me, and my task is to narrate them. I neither search for implicit connotations nor start out with abstract ideas; I am not one who plays with symbols. But if there is some transcendental explanation of one of my stories, it is not for me to discover it, that is the task of the critics and the readers.

I write for the tale itself, simply by interest in the characters and thoughts that perhaps will also interest others. The critics and scholars have attributed all sorts of intentions to me; that this or that story should evidence some specific political or religious ideologyeven a metaphysical one. Perhaps the intention is, in me, subconscious and not at a conscious level. Nor do I try to use it to this level.

I suppose this can be an illusion, but I believe that those sorts of things are proper to the explanatory writing of thinkers, and I am not a thinker, except in the measure that all men are.

Sɪᴍᴏɴ: Without doubt there are clear differences among the distinct labyrinths that appear in your works. Clearly in that of "The Library of Babel" you start from an abstraction.

Bᴏʀɢᴇs: Not true! I can tell you how this story spewed out. I worked in a small public library on the west side of Buenos Aires. I worked nine years in this library with a miserable wage, and the people who worked there were very disagreeable. They were ignorant people, stupid really. And this gave me nightmares.

One day I said to myself that my entire life was buried in this library. Why not invent a universe represented by an interminable library? A

library where one can find all the books that have been written. At the same time, I read something about permutations and combinations, and saw in this library possibilities little less than infinite. And this is an example of a story where I know the origin of this theme.

The concept of this library evokes in me my deepest, most intrinsic, pleasures. I felt truly happy when writing about it. And it was not merely an intellectual happiness. One *feels* this kind of bliss.

S̲ɪᴍᴏɴ: And why are you attracted so strongly to the idea of the Minotaur?

B̲ᴏʀɢᴇꜱ: It is curious. That idea does not attract me so much as another name attributed to this mythological being. I encountered the name of Asterión in a dictionary. It held connotations of a heavenly body or stars. It is an image that I always thought readers could enjoy.

S̲ɪᴍᴏɴ: I find definitely that the concept of the labyrinth has a unity, truly conceptual, in your writings, notwithstanding the very interesting differences in the specific hues that are given it by every story or narration.

B̲ᴏʀɢᴇꜱ: In truth, I believe that this unity arises because all of my stories that speak of the labyrinth respond to a particular state of my spirit that carries me precisely to this theme.

S̲ɪᴍᴏɴ: As to your ideas on combinatorial analysis, what were your sources?

B̲ᴏʀɢᴇꜱ: I read a very interesting book. It was Bertrand Russell's *Introduction to Mathematical Philosophy*. Then I was much interested by a book called *The World and the Individual* [Josiah Royce (1899)], which presented a very singular specimen of this theme. It presented the case of a map of England drawn on the very terrain of the island. And assume that the map itself is somewhere within the whole map. And within the first, the map of the map, and so on. That gives an idea of the infinite. From my father I inherited the taste for these forms of reasoning. He used to take me aside to converse or to ask me questions about my beliefs. On one occasion he took an orange and said to me, "In your opinion, is the flavor inside the orange?" I said, "Yes." Then he asked me, "Good, then you think the orange is continuously tasting itself?"

S̲ɪᴍᴏɴ: I would suppose that the resolution of these questions would lead you to a deep solipsism.

B̲ᴏʀɢᴇꜱ: In fact, my father didn't send me to the philosophical sources. He only presented me with concrete problems. Much later he showed me a history of philosophy where I encountered the origins of all of these questions. In the same way my father taught me to play chess, although actually I have always been a poor player, and he a very good one. Moreover, my father transmitted to me the taste for poetry. His bookshelves were filled with such authors as Keats, Shelley, and other poets.

And he also recited them from memory. Even today when I repeat verses of FitzGerald's *Omar Khayyam* and some others, my mother says that she seems to be listening to my father.

S_{IMON}: Someone told me that you read *Don Quixote* the first time in English.

B_{ORGES}: Yes, that's true.

S_{IMON}: That's curious, because I first read it in Spanish. When I encountered it in English, the humor of Quixote lost all its subtlety.

B_{ORGES}: That's true. The experience with translations is often like that.

Then Borges asked me about my work, and I began to talk about computers and the implications of a belief in the possibility of computer simulation of human thought for free will:

S_{IMON}: This is the form in which I conceive free will: It resides in the fact that *I* am that which acts when I take a given action. And the fact that something has caused this behavior in no manner makes me (the I who acts) unfree.

So when we reach a bifurcation in the road, of the labyrinth, "something" chooses which branch to take. And the reason for my researches, and the reason why labyrinths have fascinated me, has been my desire to observe people as they encounter bifurcations and try to understand why they take the road to the right or to the left.

B_{ORGES}: It seems to me that these sorts of things happen continually in my stories . . . but if I did not write these stories in specific terms, all would be artificial. That is to say, if I write these stories it is because I have to, or because I need them. Because if not, I could invent other stories, and these stories would have no meaning for me, or perhaps for the reader. Because the reader will feel that they are artificial literary exercises.

So Borges denied that there was an abstract model underlying "The Library of Babel" or "The Garden of Paths That Fork." He wrote stories; he did not instantiate models. He was a teller of tales.

A Short Story

In my one attempt at story writing, I did start with a model, in fact, the model of a maze that I had just described in my 1956 paper on "Rational Choice and the Structure of the Environment." No doubt that can be detected in the finished product. Nevertheless, I too had to write it. You may

take it as philosophy, as a story, or as an artificial literary exercise. For those of you who don't like equations, it will at least provide a relatively painless introduction to my theories of decision.

THE APPLE: A STORY OF A MAZE

There once was a man named Hugo who lived in a castle with innumerable rooms. Since the rooms were windowless, and since he had lived there since his birth, the castle was the only world he knew. His mother, who had died when he was very young, had told him of another world "outside," lighted by a single large lamp that was turned on and off at intervals of ten or twelve hours. She had not seen the outside world herself, but stories about it had been handed down from generation to generation. Hugo was never certain whether his ancestors had really lived in, or viewed, this world, or whether the stories had been invented in some remote time to entertain the children of the castle. At any rate, he knew of it only through his mother's tales.

The rooms of the castle were rectangular and very longit took Hugo almost ten minutes of brisk walking to go from end to end of one of them. The walls at each end of each room were pierced by four or five doors. These doors were provided with locks so that they could be opened from one side, but not the other. The doors on the west end of a room opened into the room, while those on the east end opened out into adjoining rooms. When Hugo entered a room and the door shut behind him, he could not again return on the path along which he had come, but could only go on through one of the eastern doors to other rooms beyond.

At one time, Hugo became curious as to whether the rooms might be arranged in cycles, so that he could return to a room by a circuitous path, if not directly. It was not easy to decide, for many of the rooms looked much alike. For a time, he dropped a few crumbs of bread in each room through which he passed, and watched for evidences of his return to one of these. He never saw any of the bread crumbs again, but he was not certain but what they had been eaten by the mice that lived in the castle with him.

After the death of his mother, Hugo lived alone in the castle. Perhaps it seems strange that he or his ancestors had not long since died of hunger in this isolated life. Most of the rooms were quite bare, containing only a chair or two and a sofa. These Hugo found comfortable enough when he wanted to rest in his wanderings or to sleep. But from time to time, he entered a room where he found, on a small table covered with a white linen cloth, food for a quite adequate and pleasant meal.

Those of us who are accustomed to a wide range of foods, gathered for

the pleasure of our table from the whole world, might not have been entirely satisfied with the fare. But for a person of simple tastesand Hugo had not developed elaborate onesthe fruits and green vegetables, the breads, and the smoked and dried meats that Hugo found in these occasional rooms provided an adequate and satisfying diet. Since Hugo knew no other world, it caused him no surprise that the arrangements of the castle provided for his weariness and hunger. He had never asked his mother who it was that placed the food on the tables.

The rooms stocked with food were not very numerous. Had his education in mathematics not been deficient, Hugo could have estimated their relative number. For the connecting doors between the rooms were of clear glass, and peering through one of them, Hugo could see through a series of five or six doors far beyond. It any of the rooms in this range of vision had dining tables in them, he could see them from where he stood.

When Hugo had not eaten for some time, and was hungry, he would stand, in turn, before each of the four of five exit doors, and peer through them to see if food was visible. If it was notas usually happenedhe would open one of the doors, walk rapidly through the next room and reinspect the situation from the new viewing points now available to him. Usually, within an hour or two of activity, he would finally see on his horizon a room with a table; whereupon he walked rapidly toward it, assured of his dinner within another hour. He had never been in real danger of starvation. Only once had he been forced to continue his explorations as long as four hours before a dining room became visible.

Since life in the castle was not very strenuous, and since the meals that were spread before Hugo from time to time were generous, he seldom took more than two meals a day. If, in the course of a stroll when he was not actually hungry, he came upon a dining room, he simply passed through it, seldom pausing to pick up even a snack. Sometimes he would search for a dining room before retiring, so as to be assured of a prompt breakfast when he awoke.

As a result of this generosity of natureor of the castle's arrangements, however these were brought aboutthe search for food occupied only a small part of Hugo's time. The rest he spent in sleep and in idle wandering. The walls of most rooms were lined with attractive murals. Fortunately for him, he found these pictures and his own thoughts sufficiently pleasant and of sufficient interest to guard him from boredom, and he had become so accustomed to his solitary life that he was not bothered by loneliness.

Hugo kept a simple diary. He discovered that his time was spent about as follows: sleep occupied eight or ten hours of each twenty-four; his search for a dining room, about three hours; eating his meals, two hours. The

remaining ten hours were devoted to idle wandering, to inspection of the castle's decorations, and to daydreaming in the comfortable chairs with which the rooms were provided.

In this existence, Hugo had little need for personal possessions, other than the clothes he wore. But his mother had given him a small knapsack that he carried with him, containing a comb, a razor and strop, and a few other useful articlesand a single book, the Bible. The Bible, which was the only book he had known or even seen, had been his primer under his mother's tutelage, and continued to provide him with an enjoyable and instructive activity, even though a large part of the "world outside" it talked about was almost meaningless to him.

You might suppose that the murals on the castle's walls would have helped him to understand this world outside, and to learn the meaning of such simple words as "tree." But the pictures were of little helpat least in any ordinary wayfor the designs the castle's muralists had painted on its walls were entirely abstract, and no object as prosaic as a treeor recognizable as such to an inhabitant of the outside worldever appeared in them.

The murals helped Hugo in another way, however. The long hours spent in examining them developed in him a considerable capacity for understanding and appreciating abstract relations, and it must be supposed that he read the creation myths and the parables of the Bible in much the same waythe concrete objects taking on for him an abstract symbolic meaning. That is to say, his way of understanding the Bible was just the reverse of the way in which it was written. The authors of these stories had found in them a means for conveying to humble people in terms of their daily experiences profound truths about the meaning of the world. Hugo, deprived of these experiences, but experienced in abstraction, could usually translate the stories directly back to the propositions they sought to communicate.

I do not mean to imply that Hugo completely understood all that he read. The story of the Garden of Eden was particularly puzzling to him. What attraction did the Tree of Knowledge possess that led Eve to such wanton recklessnessto risk her Edenic existence for an apple? If he did not know what a tree was, he was familiar enough with apples, for he had often found these on the linen-covered dining tables, and his mother had taught him their name. Hugo found apples pleasant enough in taste, but no more so than the many other things that were provided for his hunger. Perhaps in this case, the very fact of his actual experience interfered with his powers of abstraction and made this particular story more difficult to understand than the others. He did, in time, learn the answer, but experience and not abstraction led him to it.

On the afternoon of a winter dayas judged, of course, by the events

and calendars of the outside world—Hugo, who had been relaxing in an armchair, felt the initial stirrings of hunger. In his accustomed way, he arose, walked to the east end of the room, and peered through the glass in search of a table. Seeing none, he opened the second door, walked through the next room, and repeated his surveillance. This time he saw, five rooms beyond the fourth door, the table for which he was searching. In less than an hour he had arrived in the dining room ready to enjoy the meal that was waiting for him there.

But on this occasion, Hugo did something he never had done before. Before sitting down to his meal, he scanned the table to see what kind of bread had been provided. He saw in the middle of the table, surrounded with sausages and cheese, a freshly-baked half loaf of dark rye bread. And as this met his eye, there came unaccountably to his nostrils—or more likely to his brain, since his nostrils could have had nothing to do with it—the odor of French bread baked with white flour, and accompanying this imagined odor, he felt a faint distaste for the meal before him.

If Hugo, at this critical moment in his life, had stopped short and pondered, the vague movements of his imagination might have quieted themselves, and his life could have gone on as before. But Hugo, though he had spent much of his life in reflection, had never before had occasion to deliberate deeply about a course of practical conduct, and he did not deliberate now. Without pausing further, he turned from the table, walked around it, and marched on quickly to the next set of doors.

No table was visible through the glass. He pulled open one of the doors, and resumed his rapid walk. At the end of the third room he saw again, through an exit door, a distant table. He peered hard at it to see if he could identify the food that lay on it, but the distance was too great. He walked—almost ran—toward it, and was delighted to find on entering the last room that a loaf of white French bread was included in the collection of items spread before him. He ate his dinner with great gusto, and soon afterward fell asleep.

Hugo's subsequent development—or discovery—of his tastes and preferences was a very gradual matter, and for a time caused him no serious inconvenience. Although not every table was provided with French bread or with ripe olives (he soon began to develop a taste for ripe olives), a great number of them were. Besides, he did not insist on eating these delicacies at every meal. To be sure, the amount of time he spent daily in the search for food increased, but this meant merely that he could substitute a more serious purpose for some of his idle wanderings, which perhaps even increased slightly his pleasure in life.

But several major happenings foretold a more difficult future. On one

occasion, Hugo passed by four tables in turn, because the food did not please him, and then, famished with hunger, hurried on for three hours more until he found a fifthwhich was no different from the other four except that his hunger was now greater. For several days after this experience, Hugo was less particular in his diet.

At about the same time, Hugo discovered that his preferences were now extending also to the pictures on the castle's walls. Twice, he found himself turning away from a door after a brief inspection of the room beyond, because the colors or designs of the murals did not please him. A few weeks later, he saw a distant dining room at a time when he was moderately hungry, but formed a dislike for the decorations of the rooms through which he must pass to reach it. In the second room he turned aside and peered through the other glass doorsthose not leading to the tableto see whether there might be another meal prepared for him that could be reached through more pleasant surroundings. He was disappointed, and proceeded on his original path, but on later occasions he turned aside often with nothing more than a hope that his new path would provide him with a meal.

Now Hugo's diary took on a very different appearance than before. First of all, almost all of his waking time was now occupied in the search for his preferred foods, a search that was further impeded by his distaste for certain rooms. Second, his diary now included more than an enumeration of the paths he took. It was punctuated with the feelings of pleasure and annoyance, of hunger and satiety, that accompanied him on his journeys. If he could have added these feelings together, he could have abbreviated the diary to a simple quarter-hourly log of the level of his satisfaction. This level was certainly now subject to violent fluctuations, and these fluctuations, in turn, sharpened his awareness of it.

Hugo felt himself helpless to blunt these sharpening prongs of perception whose prick he was now beginning to feel. Perhaps it is reading too much into his thoughts to say that "he felt himself helpless. "More probably, the idea did not even occur to him that his tastes and preferences might be matters within his controland who is to say whether in fact they were?

But if Hugo did nothing to curb his desires, he did begin to consider seriously how he was to satisfy them. He began a search for clues that would tell him, when he looked through a series of doors and saw a distant table, what kind of food he would find on it. He developed a theory that rooms decorated in green were more likely to lead to white bread than other rooms, while the color blue was a significant sign that he was approaching some ripe olives.

Hugo even began to keep simple records to test his predictions. He also

developed a sort of profit-and-loss statement that told him how much time he was spending searching for foodand with what resultand how much his tastes in decoration were costing him in the efficiency of his search. (In spite of the propitiousness of green and blue for good meals, he really preferred the cheerfulness of red and yellow.)

To a certain extent, these scientific studies were successful, and served to reduce temporarily the increasing pressures on his time. But the trend revealed by the profit-and-loss statements was not reassuring. Each month, the time devoted to finding the best meals increased, and he could not persuade himself that his satisfaction was increasing correspondingly.

As Hugo became gradually more perceptive about his surroundings, and more reflective in his choices, he began also to observe himselfsomething that he had almost never done in the past. He found that his tastes in decoration were slowly changing, so that he actually began to prefer the green and blue colors that his experience had taught him were most likely to lead him to particularly desired foods. He even thought he detected a reverse effect: that his aversion for highly symmetrical murals, which seemed always to be present in rooms stocked with caviar, was spoiling his taste for that delicacy. But this sentiment was so weak that it might have been merely a construct of his imagination.

This gradual adaptation of his eyes to his stomach served somewhat to quiet Hugo's anxiety, for he realized that it made his task easier. In retrospect, he wondered whether his initial preferences for certain kinds of murals had not developed unconsciously from eating particularly delicious meals in rooms similarly decorated.

Hugo's researches, and the gradual reconciliation of his conflicting tastes, could only have postponed, not prevented, disaster if the growth of his demands had continued. At the time of which we are speaking, he had reached a truly deplorable state. As soon as he awoke each day, he seated himself at the table he had discovered before retiring. But however delicious the meal he had provided himselfeven if the eggs were boiled to just the proper firmness, and the bread toasted to an even brownhe was unable to enjoy his meal without distraction. He would open his notebook on the table and proceed to calculate frantically what his objectives should be for the day. How recently had he eaten caviar? Was this a good day to search for peach pie, or, since he had eaten rather well the previous day, should he hold out for fresh strawberries, which were always difficult to find?

Having worked out a tentative menu, he would consult his notebook to see what his past experiences had been as to the time required to find these particular foods. He would often discover that he could not possibly expect to locate the foods he had listed in less than ten or fifteen hours of explo-

ration. On occasions when he was especially keenly driven by search for pleasure, he planned menus that he could not hope to realize unless he were willing to forgo meals for a week. Then he would cross off his list the items, or combinations of items, most difficult to find, but only with a keen feeling of disappointmenteven a dull anger at the niggardliness of the castle's arrangements.

Again before he retired, Hugo always opened his notebook and recorded carefully the results of his day's labor. He made careful notes of new clues he had observed that might help him in his future explorations, and he checked the day's experiences against the hypotheses he had already formed. Finally, he made a score-card of the day's success, assigning 10 or 15 points each to the foods he particularly liked (and a bonus of 5 points if he had not eaten them recently), and angrily marking down a large negative score if hunger had forced him to stop before a table that was not particularly appetizing. He compared the day's score with those he had made during the previous week or month.

A period of two or three months followed during which Hugo became almost wild with frustration and rage. His daily scores were actually declining. Fewer and fewer of the relatively abundant items of his diet seemed to him to deserve a high point rating, and negative scores began to appear more and more frequently. The goals he had set himself forced him to walk distances of twenty or thirty miles each day. Although he often found himself exhausted at the end of his travels, his sleep refreshed him very little, for it was disturbed by nightmarish visions of impossible feasts that disappeared before his eyes at the very moment when he picked up knife and fork to enjoy them. He began to lose weight, and because he now begrudged the time required to care for his appearance, his haggardness was further emphasized by a stubbly beard and unkempt hair.

Midway one day that had been particularly unsuccessful, Hugo, almost at the point of physical collapse, stumbled into an armchair in the room through which he was walking, and fell into a light sleep. This time, unaccountably, he was troubled by no dreams of food. But a clear picture came to him of an earlier daysome two years pastwhen he had been sitting, awake, in a similar chair. Perhaps some resemblance between the sharply angular murals of the room he was in and the designs of that earlier room had brought the memory back to him. Whatever the reason, his recollection was extremely vivid. He even recaptured in this dream the warm feeling of comfort and the pleasant play of his thoughts that had been present on that previous occasion. Nothing of any consequence happened in the dream, but it filled Hugo with a feeling of well-being he had not experienced for many months. An observer would have noticed that the furrows on his

forehead, half hidden by scraggly hair, gradually smoothed themselves as he dozed, and that the nervous jerks of his limbs disappeared in a complete relaxation. He slept for nine or ten hours.

When Hugo awoke, the dream was still clear in his mind. For a few moments, indeed, his present worries did not return to him. He remained seated in the chair admiring the designs on the wall oppositebold, plunging lines of deep orange and sienna, their advance checked by sharp purple angles. Then his eye was caught by the white page of his notebook, lying at his feet where it had slipped from his sleeping fingers. A pain struck deep within him as though a bolt had been hurled from the orange and purple pattern of the wall. Sorrow, equally deep inside him, followed pain, and broke forth in two sobs that echoed down the hall.

For the next few days, Hugo had no heart for the frantic pursuit in which he had been engaged. His life returned very much to its earlier pattern. He rested, and he wandered idly. He accepted whatever food came his way, and indeed, was hardly aware of what he ate. The pain and sorrow he had felt after his dream were diffused to a vague and indefinable sadnessa sadness that was a constant but not harsh reminder of the terror he had passed through.

It was not long, however, before he felt the first stirrings of reviving desire, and began again in a cautious way to choose and select. He could not bear to open the notebook (though he did not discard it), but sometimes found himself thinking at breakfast of delicacies he would like to eat later in the day. One morning, for example, it occurred to him that it had been a long time since he had tasted Camembert. He searched his memory for the kinds of clues that might help him find it, and passed two tables that day because he saw no cheese on them. Although his search was unsuccessful, his disappointment was slight and did not last long.

More and more, he discovered that after he had had a series of successful days, his desires would rise and push him into more careful planning and more energetic activity. But when he failed to carry out his plans, his failure moderated his ambitions and he was satisfied in attaining more modest goals. If Camembert was hard to obtain, at least ripe olives were reasonably plentiful and afforded him some satisfaction.

Only this distinguished his new life from that of his boyhood: then he had never been pressed for time, and his leisure had never been interrupted by thoughts of uncompleted tasks. What he should do from moment to moment had presented no problem. The periodic feelings of hunger and fatigue, and the sight of a distant dining room had been his only guides to purposeful activity.

Now he felt the burden of choicechoice for the present and for the

future. While the largest part of his mind was enjoying its leisureplaying with his thoughts or examining the muralsanother small part of it was holding the half-suppressed memory of aspirations to be satisfied, of plans to be made, of the need for rationing his leisure to leave time for his work. It would not be fair to call him unhappy, nor accurate to say that he was satisfied, for the rising and falling tides of his aspirations always kept a close synchrony with the level of the attainable and the possible. Above all, he realized that he would never again be free from care.

These thoughts were passing through Hugo's mind one afternoon during a period of leisure he had permitted himself. He now had time again for occasional reading, and he was leafing the pages of his Bible, half reading, half dreaming. As he turned a page, a line of the text called his mind to attention: ". . . and when the woman saw that the tree was good for food, and that it was pleasant to the eyes. . . ."

This time no recollection of apples seen or tasted impeded the abstraction of his thought. The meaning was perfectly clearno more obscure than in the other stories he enjoyed in this book. The meaning, he knew now, lay not in the apple, but in him.

Of course, we have no way of knowing for sure what that meaning, so clear to Hugo, was. We can only conjecture, empathizing with the trials of his journey, interpreting them in the light of our own experiences. My own conjecture is that Hugo found a meaning not very different from the one I have arrived at, journeying through the maze of my own life. If it were not so, my experience would have falsified my theory, the model from which "The Apple" was drawn.

Chapter 12
Roots of Artificial Intelligence

The most important years of my life as a scientist were 1955 and 1956, when the maze branched in a most unexpected way. During the preceding twenty years, my principal research had dealt with organizations and how the people who manage them make decisions. My empirical work had carried me into real-world organizations to observe them and occasionally to carry out experiments on them. My theorizing used ordinary language or the sorts of mathematics then commonly employed in economics. Although I was somewhat interdisciplinary in outlook, I still fit rather comfortably the label of political scientist or economist and was generally regarded as one or both of these.

All of this changed radically in the last months of 1955. While I did not immediately drop all of my concerns with administration and economics, the focus of my attention and efforts turned sharply to the psychology of human problem solving, specifically, to discovering the symbolic processes that people use in thinking. Henceforth, I studied these processes in the psychological laboratory and wrote my theories in the peculiar formal languages that are used to program computers. Soon I was transformed professionally into a cognitive psychologist and computer scientist, almost abandoning my earlier professional identity.

This sudden and permanent change came about because Al Newell, Cliff Shaw, and I caught a glimpse of a revolutionary use for the electronic computers that were just then making their first public appearance. We seized the opportunity we saw to use the computer as a general processor for symbols (hence for thoughts) rather than just a speedy engine for arithmetic. By the end of 1955 we had invented list-processing languages for programming computers and had used them to create the Logic Theorist, the first computer program that solved non-numerical problems by selective

search. It is for these two achievements that we are commonly adjudged to be the parents of artificial intelligence.

Put less technically, if more boastfully, we invented a computer program capable of thinking non-numerically, and thereby solved the venerable mind/body problem, explaining how a system composed of matter can have the properties of mind. With that, we opened the way to automating a wide range of tasks that had previously required human intelligence, and we provided a new method, computer simulation, for studying thought. We also acquired considerable notoriety, and attracted critics who knew in their hearts that machines could not think and wanted to warn the world against our pretensions.

In this and the following chapters I will give a blow-by-blow account of our research during 1955 and 1956, and place it in the intellectual atmosphere, the zeitgeist, within which it took place. I will present, of course, an egocentric view of that atmosphere, emphasizing how it influenced the ideas of our research group.

To understand how the Logic Theorist came about, we have to roam over several disciplines, including psychology, logic, and economics, and examine their views of the world just before our effort got under way. Those world views both shaped our own outlook and defined the constraints and assumptions we had to modify in order to go forward.

Cognitive Psychology before 1945

On the American side of the Atlantic Ocean, there was a great gap in research on human thinking from the time of William James almost down to World War II. American psychology was dominated by behaviorism, the stimulus-response connection $(S \rightarrow R)$, the nonsense syllable, and the rat. Cognitive processeswhat went on between the ears after the stimulus was received and before the response was givenwere hardly mentioned, and the word *mind* was reserved for philosophers, not to be uttered by respectable psychologists.

My footnotes in *Administrative Behavior* show that William James and Edward C. Tolman were my principal sources among American psychologists. Tolman was the farthest from the dominant behaviorists (except for immigrant Gestalt psychologists from Europe). In his principal book, *Purposive Behavior in Animals and Men* (1932), he treated humans (and rats) as goal-seeking, hence decision-making, organisms, whose behavior was molded by the environment. But although well respected, Tolman remained at the edge of mainstream American psychology.

In Europe, psychologists were less preoccupied with rigor and were willing to use data from verbal protocols, paying more attention to complex behavior. In *Remembering* (1932), the English psychologist Frederick C. Bartlett examined how information is represented "inside the head" and modified by the processes that store and retrieve it. The Würzburg school in Germany, and Otto Selz and his followers, had similar concerns for the processes of complex thought and the ways in which the information used in thought is organized and stored in the head.

This point of view, carried forward by the Gestaltists Max Wertheimer and Karl Duncker, was hardly known in the United States until after World War II. Similarly, before the war, Jean Piaget's work on the development of thought in children was familiar to some American educational psychologists (and to me through Harold Guetzkow) but to hardly any American experimental psychologists.

Apart from Tolman, one other viewpoint in prewar American psychology departs from behaviorism: the "standard" viewpoint of physiological psychology, well expressed by Edwin G. Boring in the preface to *The Physical Dimensions of Consciousness:*

> [T]he simple basic fact in psychology is a correlation of a dependent variable upon an independent one. Ernst Mach made this point and B. F. Skinner took it up about the time this book was being written. He created the concept of "empty organism" (my phrase, not his), a system of correlation between stimulus and response with nothing (no C.N.S., the "Conceptual Nervous System"his phrase, not mine) in between. This book does not go along with Skinner . . . , but rather argues that these correlations are early steps in scientific discovery and need to be filledfor the inquiring mind dislikes action at a distance, discontinuities that remain "unexplained." Thus my text undertook to assess the amount of neurological filling available in 1932how much fact there was ready to relieve the psychophysiological vacuum. [Boring 1933, pp. vi vii]

This last sentence of Boring separates his psychophysiological viewpoint (typified also by Karl Lashley) from *both* behaviorism and our own approach. He assumes that: (1) the "empty organism" is to be filled with explanatory mechanisms, an assumption accepted by all psychologists except radical behaviorists like Skinner; but also (2) the explanatory mechanisms are to be neurological.

Al, Cliff, and I did not share this second assumptionnot because of in-principle opposition to reductionism but because we believed that complex behavior can be reduced to neural processes only in successive steps, not in a single leap. Physics, chemistry, biochemistry, and molecular biology accept *in principle* that the most complex events can be reduced to the laws of

quantum physics, but they carry out the reduction in stages, inserting four or five layers of theory between gross biological phenomena and the submicroscopic events of elementary particles. Analogously for psychology, a theory at the level of symbols, located midway between complex thought processes and neurons, is essential.

In agreement with Boring and in contrast to our view, almost all American psychologists who were not behaviorists identified explanation in psychology with neurophysiology. This confounding continued into the postwar period with Donald Hebb's influential *The Organization of Behavior,* and the confusion has today been inherited by those cognitive scientists who espouse parallel connectionist networks ("neural nets") to model the human mind.

Since information processing theories of cognition represent a specific layer of explanation lying between behavior (above) and neurology (below), they resonate most strongly with theories that admit constructs of this kind. Would the forerunners of our own work, principally Selz, the Gestaltists and their allies, be pleased to be labeled "information-processing psychologists," and would they accept our operationalizing their vague (in our eyes) concepts? With or without their consent, we acknowledge our debt to them.

The Influence of Formal Logic

To build a successful scientific theory, we must have a language that can express what we know. For a long time, cognitive psychology lacked a clear and operational language. Advances in formal logic brought about by Giuseppe Peano, Gottlob Frege, and Alfred North Whitehead and Bertrand Russell around the turn of the century provided it.

The relation of formal logic to psychology is often misunderstood. Both logicians and psychologists agree nowadays that logic is not to be confused with human thinking.* For the logician, inference has objective, formal standards of validity that can exist only in Plato's heaven of ideas and not in human heads. For the psychologist, human thinking frequently is not rigorous or correct, does not follow the path of step-by-step deductionin short, is not usually "logical."

How, then, could formal logic help start psychology off in a new direction?

* An influential coterie of contemporary artificial intelligence researchers, including Nils Nilsson, John McCarthy, and others, believe that formal logic provides the appropriate language for A.I. programs, and that problem solving is a process of proving theorems. They are horribly wrong on both counts, but this is not the place to pursue my quarrel with them, beyond the comments in the next paragraphs.

By example, it demonstrated that manipulating symbols is as concrete as sawing pine boards in a carpentry shop; symbols can be copied, compared, rearranged, and chunked just as definitely as boards can be sawed, planed, measured, and glued. Symbols are the stuff of thought, but symbols are patterns of matter. The mind/body problem arises because of the apparent radical incongruity of "ideas"the material of thoughtwith the tangible biological substances of the brain. Formal logic, treating symbols as material patterns (for example, patterns of ink on paper) showed that ideas, at least some ideas, can be represented by symbols, and that these symbols can be altered in meaningful ways by precisely defined processes.

Even a metaphorical use of the similarities between symbol manipulation and thinking liberated my concept of thinking. Influenced by Rudolf Carnap's lectures at the University of Chicago and his books, and by my study of Whitehead and Russell's *Principia Mathematica,* I very early used this metaphor explicitly as the framework for my thinking about administrative decision making: "Any rational decision may be viewed as a conclusion reached from certain premises. . . . The behavior of a rational person can be controlled, therefore, if the value and factual premises upon which he bases his decisions are specified for him" (Simon 1944, p. 19).

Exploiting this new idea in psychology requires enlarging symbol manipulation to embrace much more than deductive logic. Symbols can be used for everyday thinking, for metaphorical thinking, even for "illogical" thinking. This crucial generalization began to emerge at about the time of World War II, though it took the appearance of the modern computer to perfect it.

Parallel to the growth of logic, economics, in close alliance with statistical decision theory, constructed new formal theories of "economic man's" decision making. Although economic man was patently too rational to fit the human form, the concept nudged economics toward explicit concern with *reasoning* about action. But the economist's concern only for reasoning that was logical, deductive, and correct somewhat delayed recognition of the common interests of economics and psychology.

In striving to handle symbols rigorously and objectivelyas objectslogicians gradually became more explicit about their manipulation. When, in 1936, Alan Turing, an English logician, defined the processor now known as a Turing machine, he completed this drive toward formalization by showing how to manipulate symbols by machine. I did not become aware of Turing's work until later, but I did glean some of the same ideas from Claude Shannon's Master's thesis (1938), which showed how to implement the logic of Boolean algebra with electrical switching circuits.

Finally, at this time the belief was also growing that mathematics could

be used in biology, psychology, and sociology as it had been used in the physical sciences. Alfred Lotka's *Elements of Physical Biology* (1924), embodying this view, foreshadowed some of the central concepts of cybernetics. My former teacher Nicholas Rashevsky was another pioneer in this sphere. Although there had been little use of mathematics in psychology before the war, a few psychologists (including Clark L. Hull) had begun to show strong interest in its potential.

The Postwar Setting for Machine Intelligence

The developments I have been tracing came to public notice at the end of World War II under the general rubric of *cybernetics,* a term that Norbert Wiener devised to embrace such elements as information theory, feedback systems (servomechanism theory, control theory), and the electronic computer (Wiener 1948). In other countries, particularly behind the Iron Curtain, the term *cybernetics* was used even more broadly to encompass, in addition, game theory, mathematical economics and statistical decision theory, and management science and operations research. Wiener presented, in the first chapter of *Cybernetics,* his version of the history of these developments. The computer as symbolic machine played a minor role in the early development of cybernetics; its main underpinnings were feedback and information theory, while the computer was simply "the biggest mechanism."

World War II did not produce the cybernetic developments; more likely, in fact, it delayed them slightly. Their ties with formal logic had been evident earlier, as I mentioned, in Shannon's Master's thesis, as well as in a closely parallel paper by Walter Pitts and Warren McCulloch (1943) that provided a Boolean analysis of nerve networks, and in a paper by Arturo Rosenblueth, Norbert Wiener, and Julian Bigelow (1943) that provided a cybernetic account of behavior, purpose, and teleology. Most of the prominent figures in these developments had early in their careers been deeply immersed in modern symbolic logic. Wiener had been a student of Russell's in Cambridge; much of von Neumann's work in the 1920s and 1930s had been in logic; Shannon's and Pitts and McCulloch's use of logic has just been mentioned.

Work that too far anticipates its appropriate zeitgeist tends to be ignored, while work that fits the contemporary zeitgeist is recognized promptly. Von Neumann's contributions to game theory in the 1920s were known to few

persons before 1945; Lotka was read by a few biologists; a few logicians were aware of the rapid strides of logic and the esoteric discoveries of Kurt Gödel, Alan Turing, Alonzo Church, and Emil Post. All this changed in the early postwar years.

My own experiences in hearing lectures by Carnap, Rashevsky, and Schultz document this dramatic shift in the climate of ideas. Through these teachers, I learned of Lotka, of recent developments in statistical decision theory, of Gödelbut not immediately of Church or Turing. I found a few other teachers and fellow students who shared this vague sense of the zeitgeist. My dissertation reflected the intellectual climate of about 1940 to 1942.

The "invisible college" operated with some efficiency, then as now; news of the new contributions, published in widely scattered journals and books, spread rapidly. My attention was called to most of them either before publication or shortly thereafter. Similarly, the decision-making approach of my dissertation rapidly became known in economics and operations research.

Biology and the behavioral sciences did not long stay aloof from cybernetics. Its feedback notions soon were being used, particularly by the physiologically inclined, and most enthusiastically in Great Britain (Ashby 1952; Walter 1953). The computer-as-brain metaphor suggested itself almost immediatelyfollowed almost as immediately by warnings against taking too literally the analogy between the neurological organization of the brain and the wiring of the computer (von Neumann 1958). Turing was one of the first to see the more fruitful analogy at a different level, the abstract level of symbol processing.

Feedback concepts had considerable, but relatively unspecific, impact on psychology, but the influence of the Shannon-Weaver information theory was clear and precise (Miller and Frick 1949). W. E. Hick proposed and tested a relation between response time and amount of information contained in the response, while others sought to measure the information capacity of the human sensory and motor channels. Limits on the applicability of information theory to psychology gradually became clear, and by the early 1960s the theory had become only a specialized tool for the analysis of variability.

In the early postwar years, the European work on thought processes was just beginning to reach the United States through translation and migration. The translations of Duncker's *On Problem Solving* and Wertheimer's *Productive Thinking* appeared in 1945; Humphrey, in *Thinking* (1951), provided the first extensive English-language discussion of Selz's research and

theories; Katona's *Organizing and Memorizing* and a number of Maier's papers on problem solving had appeared by 1940 but were little noticed until the end of the war.

Information theory, statistical decision theory, and game theory had roused new interest in concept formation, and had suggested new research methods and theoretical ideas. Carl Hovland's "A 'Communication Analysis' of Concept Learning" was published in 1952; while in the critical year 1956 there appeared both George Miller's "Magical Number Seven" paper, setting forth a theory of the capacity limits of short-term memory, and Bruner, Goodnow, and Austin's *A Study of Thinking,* a book that brought to the study of concept formation notions of strategy borrowed from game theory.

The war had produced a vast increase of research in human skills and performance ("human factors" research). Because much of this work was concerned with the human members of complex man-machine systemspilots, gunners, radar personnelthe researchers could observe the analogies between human information processing and the behaviors of servomechanisms and computers. The title of Donald Broadbent's "A Mechanical Model for Human Attention and Immediate Memory" (1954) illustrates the main emphasis in this line of inquiry. The research in human factors and in concept formation provided a bridge, also, between psychology and the newly emerging field of computer science, and a major route through which ideas drawn from the latter field began to be legitimized in the former.

A long-standing concern with formalization in linguistics received a new impetus with Zelig Harris's *Methods in Structural Linguistics* (1951). The work of Noam Chomsky, which was to redirect much of linguistics from a preoccupation with structure to a concern with processing (that is, with generative grammars) was just beginning (see Chomsky 1955). I do not know to what extent these developments in linguistics derived from the zeitgeist I have described. Some connections with logic are clear (for example, Chomsky 1956). But early efforts in the mechanical translation of languages lay outside the mainstream of linguistics (see Locke and Booth 1955 for the history).

Digital Computers Enter the Scene

Digital computers developed rapidly in the early postwar era. While logicians understood that they were universal machines (Turing machines), others viewed them primarily as arithmetic engines, working with numbers rather than with general symbols.

The use of mechanisms (robots, not computers) to illustrate psychological theories and enforce operationality had a long history, predating the computer. Boring (1946) surveys that history in his article "Mind and Mechanism." And the developments in cybernetics produced a new pulse of mechanical robot building (Walter 1953; Ashby 1952), with which I had some contact at RAND beginning in 1952.

But all of these efforts were rather separate from simulation on the computer, which tended not toward activating mechanical beasts but toward programming game playing and other symbolic activities. The first checker program was coded in 1952 (Strachey), and other game-playing schemes are described in Bowden (1953). And Turing (1950), in a justly famous discussion, "Computing Machinery and Intelligence," had put the problem of simulation in a highly sophisticated form, proposing the Turing Test to determine the ability of a computer to give answers to questions indistinguishable from human answers. The stage was set for the appearance of artificial intelligence.

Chapter 13
Climbing the Mountain:
Artificial Intelligence Achieved

The completely new turn that my life took in 1955 was the unanticipated result of my work in the Systems Research Laboratory at RAND and my contact there with computers. I have described in earlier chapters my experiences with the punched-card predecessors of modern computers, first at Chicago in 1938 for producing the statistical tables of the *Municipal Year Book,* then in Los Angeles at the IBM Service Bureau in 1941. During World War II, I heard vague rumors about more powerful computers being built at Aberdeen Proving Ground in Maryland for ballistic calculations. Soon after the war, these rumors became more concrete, and by 1945, when I first met John von Neumann, I was at least vaguely aware of modern digital computers.

My awareness and knowledge grew rapidly when I read, in 1949 or 1950, Edmund Berkeley's *Giant Brains* (1949), an excellent account of the new machines. Berkeley was also selling a little toy do-it-yourself computer (the "Geniac"), constructed of nothing more than batteries and wires, which could be "programmed" by rewiring to do a variety of tasks. Buying one, I got some hands-on feel for the way computers did their work.

In 1950, I made an optimistic speech to business executives on the prospective use of computers in business, mentioning both linear programming and game theory as keys to sophisticated applications. A brief quotation from the talk later published in the journal *Advanced Management,* will convey its flavor:

> In describing these developments, which are now engaging the efforts of . . . scientists in a dozen locations, I do not wish to create undue anxiety in the minds of executives about their prospective obsolescence. Before today's business executive and management engineer take their place with the mastodons,

quite a span of years if not generations is likely to elapse. Nevertheless, I think it is fair to regard these researchesalong with the social-psychological researches mentioned earlieras portents that we are in time going to have theory in managementtheory of the kind that predicts reality, and not the kind that is contrasted with practice. When that time comes, managers will be those who can handle and apply that theory. [Simon 1950, p. 4]

Computers were within my sphere of attention, but only computers used as number crunchers. In spite of the "giant brain" metaphor, there is little suggestion in this 1950 talk that the most important application of computers might lie in imitating intelligence symbolically, not numerically. Arriving at that insight was the critical step that was required for genuine artificial intelligence to emerge. Both Al Newell and I arrived at it in the early 1950s, but by somewhat different routes.

Allen Newell

I must say something about my closest partner in the venture, who remains a close associate and friend to the present day. Although this volume is speckled with vignettes of various of my associates, I can provide only a scanty one of Al. Our paths have meshed so intricately over such a long time that telling about our collaboration and friendship would require writing another book. I will, however, say a bit about the young Al Newell, as I first encountered him.

When I first met Al at RAND in 1952, he was twenty-five years old, and fully qualified for tenure at any universityfull of imagination and technique, which is what it takes to make a scientist. I suspect he was ready for tenure soon after birth, Athena springing fully armed from the brow of Zeus. His energy was prodigious, he was completely dedicated to his science, and he had an unerring instinct for important (and difficult) problems. If these remarks suggest that he was not only bright but brash, they are not misleading.

His earliest and probably most important education as a cognitive psychologist came when, an undergraduate physics major at Stanford, he took several courses from the distinguished mathematician George Polya, who recorded many of his ideas about problem solving in a widely used book called *How to Solve It* (1945). Polya introduced Al to the word *heuristic* and to the idea behind that word. A year as a graduate student in the rarefied atmosphere of the Mathematics Department at Princeton convinced Al that his interests lay in applied, rather than pure, mathematics, and that for the

immediate future he wanted to be involved in hands-on research rather than graduate studies. He then accepted a position at RAND, where I found him.

If imagination and technique make a scientist, we must also add dollars. I learned many things in the postdoctoral training I took with Al, few more important than how to position the decimal point in a research proposal. My first lesson came from the Systems Research Lab, a grandiose project if there ever was one outside physics and space science. Al and his three colleagues simply took it for granted that it was reasonable for the air force to build an entire simulated air defense station and to staff it for years with an air force unit, enlisted men and officers. It was, indeed, reasonable, but I am not sure that would have occurred to me before I saw it happen.

Thinking big has characterized Al's whole research career, not thinking big for bigness' sake, but thinking as big as the task invites. Al learned about research funding through his early association with physicists, and it is a lesson that we behavioral scientists still need to study with him. (He has been teaching us this lesson at Carnegie Mellon University, with the funding of research in cognitive science and artificial intelligence and, more recently, with the computer networking of our campus.)

From our earliest collaboration, Al has kept atrocious working hours. By this I don't mean that he is more of a workaholic than I amperhaps a dead heatbut that he works at the wrong time of day. From the start, he preferred sessions that began at eight in the evening and stretched almost to dawn. I would have done most of my day's work by ten that morning, and by ten in the evening was ready to sleep, and not always able not to.

Perhaps his greatest pleasure (at least as judged by his behavior) is an "emergency" that requires him to stay up all night or two consecutive nights to meet a deadline. I recall his euphoria on our visit to March Air Force Base in 1954, when the air exercise extended over a whole weekend, twenty-four hours per day.

Some of these memories are frivolous, but high spirits, good humor, and hard work have characterized my relations with Al from the beginning. We have not been closely associated in joint research projects since the mid-1960s, certainly since our book *Human Problem Solving* appeared in 1972. But, however much our working paths have diverged, we still find, whenever we are together, that remarkable community of beliefs, attitudes, and values that has marked our association from the first ten minutes of meeting in February 1952.

In describing our style of work during the years, especially from 1955 to the early 1960s, when we met almost daily, I will paraphrase an interview I gave Pamela McCorduck about 1974, when she was writing *Machines Who Think*. It worked mostly by conversations together. Al probably talked

more than I; that is certainly the case now, and I think it has always been so. But we ran those conversations with the explicit rule that one could talk nonsensically and vaguely, but without criticism unless you intended to talk accurately and sensibly. We could try out ideas that were half-baked or quarter-baked or not baked at all, and just talk and listen and try them again.

Aside from talking shop, Al and I have frequently shared our personal concerns and problems. And after Lee Bach left Pittsburgh, Dorothea and I designated the Newells in our wills as guardians of our children, an indication of the closeness and trust we felt toward them. But mainly, we talk about our research, except sometimes when dining or socializing with Noël, Al's wife, and Dorothea.

Whatever hobbies and recreations we have outside our work, we have pursued separately. My own guess is that, when together, we would not resist taking up again those issues that are central to the lives of both of usour science. The content of our talk would vary little, whether climbing a mountain together or talking in Al's study or my living room.

The Research Gets under Way

At RAND's Systems Research Laboratory I became fascinated by the method that Al and J. C. (Cliff) Shaw, an outstandingly talented and sophisticated systems programmer, had devised for using a card-programmed calculator to produce imitations of radar maps for air-defense simulation. In this application the computer was generating not numbers but locations, points on a two-dimensional map. Computers, then, could be general symbol systems, capable of processing symbols of any kindnumerical or not.

This insight, which dawned only gradually, led Al and me even more gradually to the idea that the computer could provide the formalism we were seekingthat we could use the computer to simulate all sorts of information processes and use computer languages as formal descriptions of those processes.

In the summer of 1954, I taught myself to program the 701, IBM's first stored-program computer, and computers were much on my mind. Driving together to March Air Force Base for the air exercises I have mentioned, Al and I had a long discussion of the possibility of simulating human thinking by computer.

My earlier thoughts about chess programming, during the 1952 RAND summer seminar, had produced only a verbal description of a program. Now Al initiated in earnest an effort to program a digital computer to learn

to play good chess. His work led to a published paper on the subject, but not immediately to a running program. He was stimulated by the work of Oliver Selfridge (1955) and Gerry Dinneen (1955), which showed how computers were to become truly non-numerical processors. Selfridge and Dinneen had produced a (primitive) program for recognizing patterns. A seminar that Selfridge gave at RAND about this work impelled Al to begin his chess project. Al and I discussed the project on numerous occasions, but my role in it was wholly consultative.

In the first published description of his plan for a chess program, Al said: "This is the means to understanding more about the kinds of computers, mechanisms, and programs that are necessary to handle ultracomplicated problems" (Newell 1955, p. 101). The research was thus identified as artificial intelligence (though that name was not used); but the task to be examined was one of great psychological interest and had already been studied extensively by Adriaan de Groot (1946) in the Netherlands.

The initial approach also established the precedents, followed in all of our subsequent work, that artificial intelligence was to borrow from psychology, and psychology from artificial intelligence. Thus, Al's programmatic description of his chess-learning proposal, like my 1952 sketch (see chapter 10), used aspiration values and notions of "satisfactory solution" in evaluating chess moves, and discussed the necessity for "rules of thumb" (later called *heuristics*) to reduce to manageable size the enormous search spacethe space containing all the branches in the tree of possible chess moves and replies. By using heuristics and settling for satisfactory moves, the search space could be explored very selectively, avoiding any attempt at an impossible exhaustive search. Always following this course, the research kept one eye on its potential for psychology.

Al soon added to the team Cliff Shaw, who had worked with him on the radar map problem. When John von Neumann had designed a powerful computer for RAND (which, over his protests, was named JOHNNIAC), Cliff had played a major role in constructing the programming systems for it. We judged that JOHNNIAC, with a 4,096-word high-speed store supplemented by a drum with about 10,000 words of usable capacity, would be large enough and fast enough to meet the needs of a chess program. Besides, nothing larger or faster existed.

Al had come to RAND without taking his Ph.D. He wanted sooner or later to acquire this union card, but he didn't want to interrupt his exciting research. Without great difficulty, I convinced my colleagues at GSIA that it was wholly appropriate for a business school to award a doctorate for a thesis in what we would now call artificial intelligence. (We ultimately

awarded about a dozen such degrees before computer science became a separate discipline at Carnegie.)

After considering this and other alternatives, Al decided to move his base of operations and his chess research to Carnegie Tech, and the RAND Corporation agreed to keep him on the payroll, making him their Pittsburgh outpost. With Noël and their new son, Paul, Al was installed in Pittsburgh in the spring of 1955 and work on the chess project got under way.* I wanted to spend as much time as I could working with Al on the chess problem, and stated my intention to continue working on human problem solving and the theory of "Hugo-like" maze models (see "The Apple" in chapter 11).

We agreed to meet each Saturday, roaming on these occasions over a wide range of topicsparticularly problem solving and the chess language Al was trying to devise. Al tended to supply ideas starting from the language and computer end, I starting from human problem solving and what we knew of the heuristics there. This is one of the role specializations that, subject to strong qualifications, we mildly adhered to for some years. In the course of these discussions, we considered illustrative problems from areas other than chess, including Euclidean geometry, Katona-type matchstick problems, and symbolic logic (*Principia Mathematica* was on my bookshelf).

Since the program we were planning to build was to run on JOHNNIAC, in Santa Monica, Cliff and Al communicated by teletype, an early semiautomated computer network that ran up phone bills of $500 a month, a sum that didn't faze Al (or RAND) but seemed colossal to me.

During the third week of October 1955, Al and I attended the meetings of the Institute of Management Science in New York. I arrived a day early for a morning meeting with Bernie Berelson at the Ford Foundation. On that afternoon, a beautiful sunny day, I decided to take a walk along the Hudson on Morningside Heights. I believe I had an appointment on the Columbia campus late in the afternoon, probably with Paul Lazarsfeld or Bob Merton.

As I walked I pondered about how one solves geometry problems. The example I had in mind had to do with angles inscribed in circles and semicircles (I think there were several cases depending on whether one line fell inside or outside the others). Suddenly I had a clear conviction that we could program a machine to solve such problems. I jotted some notes on a piece of paper and thought hard about it for a few minutes, the conviction re-

* This chronicle of the autumn of 1955 is based on a memorandum I wrote on July 9, 1957, soon after the events described, and does not rely on my memory of long-ago events.

maining very strong. I think the conviction arose from the fact that I could see the heuristic I was using and how it cut down the search space.

That evening Al and I met in the hotel room of Merrill Flood, the operations research specialist who had first invited me to RAND, and after discussion we agreed to try to program a geometry machine before Christmas, two months away. We both felt strongly that we had an excellent chance to succeed. I have a clear picture of that room, and of where each of us was sitting.

Considerable attention was meanwhile being given to the programming language required for the project. On the basis of their previous experience, Cliff and Al knew that it would be difficult to write our programs directly in the machine language of the computer. In artificial intelligence programs, you cannot predict what data structures the system will need to build and store, or how these structures will interact and be modified in the course of the computation. The utmost flexibility is required, and information stored in memory must be indexed in ways that will make it accessible whenever needed.

These are also requirements, of course, for human memory. We cannot assume that the Lord has assigned us specific locations in memory for storing Latin verbs, and others for algebra. The storage must be dynamic, reassignable. And if human memory is any guide to artificial intelligence, the memory should be associative: Each symbol should lead to other symbols that are linked to it, and these links should be acquired through learning.

Moreover, there seem to be two kinds of associations: simple and directed. Presented with the simple stimulus *dog,* most (English-speaking) subjects respond *cat*. But presented with *superordinate, dog,* they respond *animal;* and presented with *subordinate, dog,* they respond *dachshund,* or *collie*. A computer language for A.I. would have to handle both simple and directed associations. In the language we built, the directed associations were called *descriptions*.

We needed a higher-level language, congenial to the human programmer, which would do automatically much of the "housekeeping" in the computer and which would be translated automatically by the computer itself into machine language. And memory structures would have to be highly modifiable. Although all three of us participated in its design, Al and Cliff took primary responsibility for constructing such an information-processing language (IPL) or list-processing language. This task was a major preoccupation during 1955 and into the spring of 1956. My role consisted primarily of comparing proposed language designs with the analogous human functions. We had, for example, numerous discussions on how to handle descrip-

tionsparticularly how to avoid limiting their generality. We were agreed on the need for flexibility and avoiding of dimensionalization.

A memo written by Al on April 2, 1956, marks a major breakthroughthe use of an association memory in the form of "list structures" to make search dimensionless. The idea had a dual source in machine technology and in the idea of human association nets, and extended to both networks of lists and lists of descriptions. Al and Cliff solved the implementation problems soon thereafter.

The Logic Theorist Is Conceived

After the Management Science meeting in New York, Al and I worked on geometry every Saturday. Facing the question of how to do the diagrams, we saw that we still had to deal with some basic problems of perception. Around the first of November, symbolic logic began to emerge as an alternativespecifically because it involved no diagrams.

The first note I have in writing, dated November 15, 1955, consists of an analysis of the proof of theorem 2.15 of *Principia*. During this time I was reviving my skill in logic by studying the proofs in chapter 2 of that treatise. By the beginning of December I was beginning to have pretty clear ideas about some pieces of the heuristic (for example, working backward in proofs by substitution). I was doing most of the actual work on the proofs, supplemented by our Saturday discussions. Al, after a burst of activity in November or October, was somewhat bogged down by studying for his written doctoral examinations.

Al's notes pick up from about December 6, by which time we had most of the pieces but little of the organization of the program. During the subsequent week we conferred frequentlyalmost dailyfor short periods and I worked almost every night on the proofs. On Thursday, December 15 (having felt I was getting increasingly close during the week), I succeeded in simulating by hand (in literal imitation of a computer program) the first proof, using a program reasonably close to that published in our Institute of Radio Engineers paper the following September. During the subsequent several days, Al and I worked hard to sharpen the procedure and put it in a form that we agreed was programmable on the computer.

I don't want to create an impression of specialization (Did Hillary or Tenzing touch the summit of Everest first?). Most of the actual paper-and-pencil work on developing the strategy of the LT program was done by me, just as most of the actual work on the language was done by Al and Cliff.

We were in the closest communication during the whole period and, through long association, had developed an extraordinary capacity to communicate even our subtleties to one another, and the whole product must be regarded as joint and inseparable. No one or two of us had much chance of completing it.

I have always celebrated December 15, 1955, as the birthday of heuristic problem solving by computer, the moment when we knew how to demonstrate that a computer could use heuristic search methods to find solutions to difficult problems. According to Ed Feigenbaum, who was a graduate student in a course I was then teaching in GSIA, I reacted to this achievement by walking into class and announcing, "Over the Christmas holiday, Al Newell and I invented a thinking machine." (If, indeed, I did say that, I should have included Cliff Shaw among the inventors.) Of course, LT wasn't running on the computer yet, but we knew precisely how to write the program.

We were not slow in broadcasting our success. In a letter to Adriaan de Groot, on January 3, 1956, I reported:

> You will be interested to learn, I think, that Allen Newell and I have made substantial progress on the chess-playing machineexcept that at the moment it is not a chess-playing machine but a machine that searches out and discovers proofs for theorems in symbolic logic. The reason for the temporary shift in subject matter is that we found the human eye and the portions of the central nervous system most closely connected with it to be doing too much of the workat the subconscious levelin chess-playing, and we found this aspect of human mental process (the perceptual) the most difficult to simulate. Hence, we turned to a problem-solving field that is less "visual" in its content.

> Two weeks ago, we hit upon a procedure that seems to do the trick, and although the details of the machine coding are not yet worked out, there seem to be no more difficulties of a conceptual nature to be overcome. By using a human (myself) to simulate the machineoperating by rule and without discretionthis simulated machine has now discovered and worked out proofs for the first twenty five or so theorems in *Principia Mathematica*. The processes it goes through would look very human to you, and corroborate in many respects the data you obtained in your chess studies.

While awaiting completion of the computer implementation of LT, Al and I wrote out the rules for the components of the program (subroutines) in English on index cards, and also made up cards for the contents of the memories (the axioms of logic). At the GSIA building on a dark winter evening in January 1956, we assembled my wife and three children together

with some graduate students. To each member of the group, we gave one of the cards, so that each person became, in effect, a component of the LT computer programa subroutine that performed some special function, or a component of its memory. It was the task of each participant to execute his or her subroutine, or to provide the contents of his or her memory, whenever called by the routine at the next level above that was then in control.

So we were able to simulate the behavior of LT with a computer constructed of human components. Here was nature imitating art imitating nature. The actors were no more responsible for what they were doing than the slave boy in Plato's *Meno,* but they were successful in proving the theorems given them. Our children were then nine, eleven, and thirteen. The occasion remains vivid in their memories.

Our success was announced publicly, but briefly, in the unpublished RAND Report-850, "Current Developments in Information Processing" (May 1, 1956), a paper read by Newell in Washington, D.C., May 2, 1956. It was not until August 9, 1956, that the Logic Theorist, programmed on JOHNNIAC in IPL-II, produced its first complete proof of a theorem (Theorem 2.01 of *Principia)*, and September 1956 that a formal description of the scheme, in Information Processing Language I (IPL-I), was published.

We did not wait long after the first computer proof had been found by JOHNNIAC before communicating to Bertrand Russell the news of our success. Here is our first letter, and his reply:

October 2, 1956

Earl Russell
41 Queen's Road
Richmond, Surrey
England

Dear Earl Russell:

Mr. Newell and I thought you might like to see the enclosed report of our work in simulating certain human problem-solving processes with the aid of an electronic computer. We took as our subject matter Chapter 2 of *Principia,* and sought to specify a program that would discover proofs for the theorems, similar to the proofs given there. We denied ourselves devices like the deduction theorem and systematic decision procedures of an algorithmic sort; for our aim was to simulate as closely as possible the processes employed by humans when systematic procedures are unavailable and the solution of the problem involves genuine "discovery."

The program described in the paper has now been translated into computer language for the "Johnniac" computer in Santa Monica, and Johnniac

produced its first proof about two months ago. We have also simulated the program extensively by hand, and find that the proofs it produces resemble closely those in Principia. At present, we are engaged in extending the program in the direction of learning (of methods as well as theorems) and self-programming.

Very truly yours,
Herbert A. Simon, Head
Industrial Management Department

2 November, 1956

Dear Mr. Simon,

Thank you for your letter of October 2 and for the very interesting enclosure. I am delighted to know that *Principia Mathematica* can now be done by machinery. I wish Whitehead and I had known of this possibility before we both wasted ten years doing it by hand. I am quite willing to believe that everything in deductive logic can be done by a machine.

Yours very truly,
Bertrand Russell

A year later, we wrote Russell again to report the results of experiments in learning with LT.

September 9, 1957

Dear Mr. Russell:

The enclosed reprints will indicate our further progress in simulating human problem-solving processes with a computer. In work subsequent to that reported in the reprints, we have accumulated some interesting experience about the effects of simple learning programs superimposed on the basic performance program. For example, we obtain rather striking improvements in problem-solving ability by inducing the machine to remember and use the fact that particular theorems have in the past proved useful to it in connection with particular proof methods.

The proofs the Logic Theorist has discovered have generally been pretty close to those in *Principia,* but in one case it created a beautifully simple proof to replace a far more complex one in the book. In the case of proposition *2.85, $pvq. \rightarrow .pvr:\rightarrow:p.v.q \rightarrow r,$ it noted that this can be derived by an application of syllogism from: $pvq. \rightarrow .pvr: \rightarrow : - q.v.pvr,$ using the associative law, *1.5, to rearrange terms on the right-hand side. This new expression, in turn, can be obtained by modus ponens in *2.05 directly from *1.3. Q.E.D. The machine required something less than five minutes to find the proof. Since the machine's proof is both straightforward and unobvious, we were much struck by its virtuosity in this instance.

You may also be interested in the evidence on page 229 of our paper that the learned man and the wise man are not always the same person. Of course this has been known for a long time, but it is nice to have such definite evidence to bring against the pedant. In general, the machine's problem solving is much more elegant when it works with a selected list of strategic theorems than when it tries to remember and use all the previous theorems in the book. The contrasting Figures 7 and 8 are typical of the differences. I am not sure that these facts should be made known to schoolboys.

Sincerely yours,
Herbert A. Simon
Professor of Administration

21 September, 1957

Dear Professor Simon,

Thank you very much for your letter of September 9, and for the enclosure. I am delighted by your example of the superiority of your machine to Whitehead and me. I quite appreciate your reasons for thinking that the facts should be concealed from schoolboys. How can one expect them to learn to do sums when they know that machines can do them better? I am also delighted by your exact demonstration of the old saw that wisdom is not the same thing as erudition.

Yours sincerely,
Bertrand Russell

When the same novel proof of LT that Bertrand Russell enjoyed was sent to Stephen Kleene, the distinguished editor of the *Journal of Symbolic Logic,* in a paper co-authored by the Logic Theorist, he rejected the paper as not representing a new result. Since the methods of *Principia Mathematica,* he said, were now outmoded, it was no accomplishment to prove a theorem using that system. It would be rude to suggest that the difference between Kleene's and Russell's responses was further proof of the difference between learning and wisdom. In any event, we lost this opportunity to boast to logicians about LT's prowess.

The letters to Russell show that from the beginning we were interested in simulating *human problem solving,* and not simply in demonstrating how computers could solve hard problems. Further, they show that we were quite aware of simpler ways of proving these theorems outside the system of *Principia,* and that we used that system only by way of exampleany system would have served as well. Later, the logician Hao Wang and others, taking computational efficiency as their only criterion, designed faster com-

puter proof procedures using truth tables or the method of natural deduction, and denigrated the Logic Theorist as primitive. They simply misunderstood the objectives of the research on LT.

Finally, the letters show that from the very beginning we were aware of the possibilities of machine learning and of automatic programming (and, indeed, we pursued some of these possibilities in the following years).

We were prompt in spreading the word of our success to our scientific colleagues. I have mentioned the brief talk on LT that Al gave in Washington in May. A summer workshop in artificial intelligence had been organized at Dartmouth for June 1956, by John McCarthy, Marvin Minsky, Nat Rochester, and Claude Shannon. Participants in the workshop included most of the persons who were thinking actively about artificial intelligence at that time: the four organizers, along with Oliver Selfridge, Ray Solomonoff, Trenchard More, and Herbert Gelernter. Al and I spent about a week there.

Although many ideas for programs to solve problems, recognize patterns, or play games were in the air, the two concrete schemes brought to the conference were our Logic Theorist and a program for proving theorems in the propositional calculus by the method of natural deduction, devised by Trenchard More (1957). More had constructed a flow diagram and had hand-simulated his scheme. The description of LT later presented in September in Cambridge was distributed to the group, and early debugging outputs were exhibited. We also had long discussions with John McCarthy at Dartmouth about the list-processing languages we had invented to program LT.

Marvin Minsky's well-known essay "Heuristic Aspects of the Artificial Intelligence Problem," although not published until several years later as *Steps Toward Artificial Intelligence,* was first drafted as a technical report late in 1956. It reflects very well the general body of knowledge in artificial intelligence that was pooled at the Dartmouth conference.*

Prior to the workshop, several persons and groups (including Minsky and ourselves) had given some attention to proving theorems in geometry. This was an important topic of conversation at Dartmouth, giving impetus to the successful work of Herb Gelernter and Nat Rochester on this problem, which followed shortly thereafer.

Next, we attended a meeting of the Institute for Radio Engineers (IRE, the predecessor of the Institute of Electrical and Electronic Engineers) at M.I.T. in September 1956, where a session was to be devoted to reporting

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* Minsky's essay was considerably modified prior to publication. It is the earliest, unpublished, version of 1956 that best reflects his interpretations at the time of the Dartmouth conference.

the results of the Dartmouth conference. Someone proposed that John McCarthy should give a summary report of all the work. Since our own work represented the only tangible (programmed) example of artificial intelligence presented at Dartmouth, and since the work predated the conference, Al and I dissented loudly, and after negotiationin the course of a long walk with the chairman, Walter Rosenblithit was agreed that Al and I would give a paper on our work, and John a summary paper on the conference.

Thus, very soon after LT was actually operative, its capabilities and structure were known to virtually everyone interested at that time in the potential of computers for intelligent behavior, and to much of the wider computer community as well. Whoever missed the news had further opportunities to pick it up from our paper in the *Proceedings of the Western Joint Computer Conference* (1957), and soon from many other sources. Our light was hidden under no bushel. The broader story of its dissemination and impact will be taken up in the next chapter.

In reporting our work, we had to solve the problem of the public perception of the collaboration between Al and me. Although, formally, Al came to Carnegie as my student, his actual role was colleague and collaborator. He was my partner, not my protégé. But I was a well-known social scientist, while he was officially a graduate student, eleven years younger than I, with few publications. Both to be fair and to make a lasting collaboration possible, the parity between us had to be recognized. We followed a deliberate strategy to accomplish this.

Our names nearly always appeared alphabetically in our joint publications. Al's name thus came first. (The exceptions were a few invited lectures I gave, not reporting new research.) People could interpret this as Al being the senior partner or as an alphabetical listing, but not as my being senior. We generally took turns attending public meetings. Since we were interchangeable parts, there was no sense in both of us going. Prior to 1965, Al was our sole spokesman abroad.

When I made references to our work, I was careful to mention both of us. When people sent me their manuscripts for criticism in which our names were mentioned in reverse order, I put them back in alphabetical order. But it turned out not to be a major problem, mainly because everyone who met Al discovered that he was a big boyhe wasn't anyone's protégé.

We also had to be careful that Cliff Shaw's major role in our discoveries was properly recognized. This was made more difficult by Cliff's taciturnity and great modesty. We made sure that he was included as co-author of the important early papers. We were embarrassed that he did not share the

Turing Award with us in 1975, and we insisted that his partnership in inventing list-processing languages be acknowledged in the citation that accompanied the award.

The Discovery of List-processing Languages

Writing and testing the Logic Theorist was only half of what we had accomplished in 1956. We had also invented a whole new class of computer-programming languages known as list-processing languages, the general nature of which has already been briefly indicated. These languages were the direct ancestor of John McCarthy's LISP, which has been the standard A.I. language for thirty years, as well as embodying most of the ideas of what is now called object-oriented programming.

The basic idea is that, whenever a piece of information is stored in memory, additional information should be stored with it telling where to find the *next* (associated) piece of information. In this way the entire memory could be organized like a long string of beads, but with the individual beads of the string stored in arbitrary locations. "Nextness" was not determined by physical propinquity but by an address, or pointer, stored with each item, showing where the associated item was located. Then a bead could be added to a string or omitted from a string simply by changing a pair of addresses, without disturbing the rest of the memory. One could store as many Latin verbs as desired without preassigning storage for them. Each string of beads was called a *list*. Generalizing, any item on a list, any bead, could be the name of another list, with a pointer to its first bead. Now the structure of memory was no longer restricted to simple strings, but could include great branching treelike structures.

But a further generalization was required to represent the directed associations that the experiments of N. Ach (1905) and others had shown to exist in human memory. This was accomplished by the *description list* (nowadays known more commonly as a *property list*). The odd beads on a description list held the names of the attributes, the even beads next to them, the values of those attributes.

Thus, the attribute "color," stored as seventh bead, say, on a description list, could be followed by the value "red," as eighth bead. Again, a value might itself be a list structure of arbitrary complexity. So description lists could be associated with objects, forming schemas describing those objects. Schemas are now widely used in programming under such labels as *scripts, frames, objects,* and *semantic nets*.

Finally, a list-processing language requires processes that can insert and

delete items, find the next item on a list, find the value of an attribute of an object, assign such a value, create objects, and so on. About a dozen such processes are more or less essential to manipulate list structures. That's basically all there is to list processing. You may say that it's quite enough.

From the beginning, the new artificial intelligence community accepted list processing as the programming tool for A.I. That is still largely true today, more than thirty years later. The rest of the programming profession, however, did not greet the innovation with open arms. They observed that list processing threw half the memory away (a heavy cost with the very small computer memories then available). That was unavoidable in order to store the "next" address with each list item. Moreover, list-processing programs had to be executed by interpreters, which slowed execution time by a factor of nearly ten. To conventional programmers these languages seemed ridiculous, if not suicidal.

A few years of experience showed why the condemnation was egregiously wrong. Within a decade, the value, or even necessity, of list processing had become evident in many kinds of programming environments, and chapter 2 of Knuth's influential *Fundamental Algorithms* (1968) gave list processing wide credibility outside the A.I. community. Today list-processing ideas are commonplace in sophisticated programming.

The languages our group developed were called IPLs (Information Processing Languages). IPL-II, the first one that actually ran on a computer, was the language of the Logic Theorist on JOHNNIAC. Of these languages, IPL-V was the most widely used on the IBM650, IBM704, and many other computers. It is now almost a dead language, although at least one version is still active (on a PC!). It was largely superseded by LISP, designed by John McCarthy, and today there are other competitors as well.

The IPLs were precocious in other respects besides being the first list-processing languages. They appeared only slightly later than FORTRAN, thus were pioneers among higher-level programming languages. They also anticipated later ideas of so-called structured programming, a set of heuristics for constructing computer programs that would be debuggable and intelligible as well as efficient.

An interesting, but probably undecidable, historical question is whether IPL-V contributed significantly to the development of structured programming. The IPL-V manual is quite explicit in its advocacy of top-down programming and independent closed subroutines, two of the central features of structured programming. The manual warns against instructions in one subroutine that refer to another, a no-no for structured programming, and characterizes processes solely in terms of inputs and outputs, a desideratum of structured programming.

Since list processing was in low regard among mainstream systems programmers during the early years of computing, however, they probably reinvented structured programming without much awareness that it was already preached and practiced in the A.I. community.

So we come to the end of the second panel, the winter of 1956-57, with list-processing languages operating on our computers and the Logic Theorist searching out the proofs of *Principia Mathematica*. Our project had achieved these two major successes remarkably quickly, launching the related disciplines of artificial intelligence and information-processing psychology.

For me, there was no question of turning back to my earlier interesting, but much less exciting, research on organizational decision making. In computer programming languages, I found tools that classical mathematical languages had not provided for exploring the processes of human thinking and for attaining accuracy and rigor in the behavioral and social sciences. I had passed a crucial branch point in the maze and was now committed to a future in cognitive psychology and computer science.

The rest of the storyat least the research part of ittells how our research group moved forward from that success, and tells about its impact, first in helping create the new discipline of computer science, then in producing a major revolution in cognitive psychology, and finally, in introducing essential new ideas into economics and engineering design, not to mention epistemology.

THE THIRD PANEL
VIEW FROM THE MOUNTAIN

Chapter 14
Exploring the Plain

The mountain pass over which Al, Cliff, and I crossed with the realization of the Logic Theorist opened up vast views. We saw before us the whole domain of artificial intelligence and the whole domain of human cognition. Numerous scientists have shared with us the adventure of exploring these lands, both at Carnegie and at many other institutions. This chapter recounts some of the explorations of our own research group, at first the three of us, soon a growing circle of graduate students and faculty colleagues, during the years from 1956 to 1978.

No detailed master plan mapped our research from the initial success of LT through the 1970s. We followed something like the flexible tactics advocated by the British military expert Liddell Hart, and gratefully borrowed by the Germans for the 1940 blitzkrieg: Push across the front; when you find a soft spot, wherever it may be, pour your reserves through and keep going. Research, groping through the uncertain and the unforeseeable, must be flexible to grasp and exploit every sign of progress. Let me try to describe how we carried out that strategy.

The Research Strategy

The initial motives that led us to the Logic Theorist, in the GSIA environment, influenced our research strategy. We were interested both in business applications that exploited the newfound powers of computers for symbolic processing, and in extensions that would contribute to understanding human thinking. Nowadays, we usually call the former artificial intelligence and the latter cognitive science.

Along both fronts, our strategy was to select promising tasks that call

for intelligence and to write and test programs capable of handling them. In the cognitive science part of our research, we also ran experiments with human subjects doing the same tasks to see how closely the computer simulations paralleled human behavior. As we gained a reasonable understanding of each task, we moved on to another. The precise order in which we took up tasks was partly a matter of chance, although we tended to start with those that seemed simple and progress toward the more complex. Eventually, we wanted to model the whole (cognitive) human beinga goal that would keep us busy indefinitely.

The artificial intelligence side was the easier to relate to the goals of a business school. Among the graduate students, Fred Tonge was soon building a program that used heuristic search to balance assembly lines (to find the best arrangement of workers, tasks, and work stations), while Geoffrey Clarkson constructed an expert system (as we would call it today) for choosing stock portfolios for bank trust accounts. There was no deep reason for selecting these two particular tasks: they just came to attention, were practically and theoretically interesting, and seemed doable. Doability and significance are always good bases for choosing research problems. We want a problem whose answer has interest and value, but only if we have some ideas for approaching it.

The significance of a problem for cognitive science could be judged by how much attention psychologists paid to it and whether it illuminated important human capabilities. Doability depended on the current state of the programming art and whether we had any good ideas for experiments. Problem solving was an obvious research candidate, both because we had already made a start with LT, and because it is a critically important human mental activity. A substantial fraction of cognitive science research over the three and a half decades since 1956 has been directed toward understanding human problem solving.

Rote verbal learning was another obvious candidate, as it had been studied more than any other subject by American psychologists. The standard experiments were modeled on the way people traditionally learn foreign language vocabulary (paired-associate learning) or memorize poetry (learning by serial anticipation). Apart from experimental work we might do ourselves, there were thousands of experiments already in the published literature that we could use to test our simulation models.

Extrapolating letter series was chosen as yet another task for several reasons. It was an example of serial behavior, whose importance to psychology had been emphasized by Lashley. The task had been selected by designers of intelligence tests as a good measure of significant mental abilities. And, as it was a law-discovering task, it could open the path toward

illuminating the processes needed for scientific discovery and other creative activities.

A more speculative venture, undertaken as a doctoral thesis by Bob Lindsay, was to explore how computers might *understand* information, taking genealogical charts as the objects to be understood. In another important dissertation task, Ross Quillian was to design semantic memory structures that could display some of the characteristics of human associative memory.

Modeling Verbal Learning:
EPAM

The research on LT had borrowed more from psychology to advance artificial intelligence than from artificial intelligence to advance psychology. Psychology was very much on the research group's mind, however. Almost from the beginning of 1956, we began thinking about elementary perceptual and memorizing processes, motivated partly by the perceptual questions raised by geometry and partly by a growing interest in learning processes for LT. Besides, I was trying to learn Greek at the time, and was introspecting about how I learned.

My first file memorandum sketching a possible approach to verbal learning is dated February 18, 1956. The name applied to the scheme, EPA-MINONDAS, betrays its interactions with my Greek studies. In particular, I developed ideas about the usefulness of redundancy for memory. This provided, in turn, a motivation for our later investigations of learning by hindsight. During this period, Al and I had numerous conversations about discrimination nets and sorting schemes that later were reflected in EPAM.

The notion of simulating behavior in classical verbal learning tasks emerged while I was searching the psychological journals for clues about associative memories. Eleanor Gibson's classic paper on stimulus and response generalization (Gibson 1940) was an important source of ideas that Ed Feigenbaum subsequently incorporated in EPAM.

The General Problem Solver

Serious attempts to interpret LT as a psychological theory of problem solving got under way in the autumn of 1956. We had learned that O. K. Moore and S. B. Anderson had used problems in logic (disguised as "decoding" exercises) to study problem solving at Yale University (Moore and Anderson 1954a and 1954b). Their formal system was close enough to Whitehead and Russell to suggest using their task to compare human behavior with

the behavior of LT. To that end, Peter Houts, a graduate student, began to tape-record thinking-aloud sessions with subjects doing the Moore-Anderson logic task.

The first tapes, transcribed in the spring of 1957, made clear that LT did not fit at all well the detail of human behavior revealed by the protocols. The problem was discussed at a research seminar on organizational behavior that met on the Carnegie campus in the summer of 1957. During the week of the discussions, both Al and I, apparently independently, found in a particular thinking-aloud protocol clear evidence that means-ends analysis was the subject's principal problem-solving tool.

Means-ends analysis is accomplished by comparing the problem goal with the present situation, and noticing one or more differences between themfor example: I am here, I want to be there; I am five miles from my goal. The observed difference jogs memory for an action that might reduce or eliminate it (take a bike or an automobile; walk). The action is taken, a new situation is observed, and, if the goal has still not been reached, the whole process is repeated.

During the summer and autumn of 1957, we gradually converged to a program embodying the newly discovered means-ends analysis. Because the reasoning processes in the program were independent of the particular topic on which it was reasoning, we christened it the General Problem Solver. The general flow diagram of GPS was produced before the end of October 1957, and the planning method (a scheme for simplifying search by abstracting the problem) was sketched a few days later (Newell, Shaw, and Simon 1962). Thirty years of subsequent research has confirmed that means-ends analysis, as embodied in GPS, is a key component of human problem-solving skill.

Talking to Psychologists

Our first published attempt to communicate directly with psychologists was "Elements of a Theory of Human Problem Solving," in the July 1958 issue of *Psychological Review* (Newell, Shaw, and Simon 1958a). Written more than a year earlier, this paper was based on the experience with LT, emphasizing the broad resemblances between that program and human problem solving without detailed comparisons of behavior.

In the paper, we described behavior in terms of programs of "primitive information processes," which could be executed on computers. The theory could be tested by matching the computer simulation to actual behavior revealed in thinking-aloud protocols. In our paper this information-

processing theory of thinking was compared with neurological, associationist, and Gestalt explanations.

Rather than emphasizing the novelty of our theory and proclaiming a new "school" in psychology, we tried to show the continuity of our approach with the work of both our associationist and Gestalt predecessors. This paper was thus the first explicit and deliberate exposition of information-processing psychology, but without using that or any other trademark name.

GPS (including the planning method) was first publicly described (but not named) in "The Processes of Creative Thinking," a paper read on May 14, 1958, at a University of Colorado symposium and in June at a RAND summer seminar; it was not published until 1962 (Newell, Shaw, and Simon 1962). This paper contained the first fragment of informal comparison of computer trace (hand-simulated) with a human thinking-aloud protocol. The suggestion that computers could simulate even creative activity created a stir at the Colorado meeting, not unmixed with a large quantity of skepticisma reasonable reaction.

Chess:
The Drosophila of A.I.

Work on a chess program, nearly dormant since 1955, was resumed toward the end of 1957.* Meanwhile, our awareness of Adriaan de Groot's *Het Denken van den Schaker* [The chess player's thinking] (1946) led to friendship and a collaboration with de Groot and his colleagues in Amsterdam that has continued down to the present (although not without some deep differences between us in methodological and conceptual outlook).

Chess has become a standard tool in cognitive science and artificial intelligence research (a standard "organism," like Drosophila or Neurospora in genetics). Powerful programs, now at grandmaster strength, employ the speed and power of modern computers, sometimes analyzing fifty million or more possibilities before they make a move. Although they also use extensive chess knowledge, such programs belong to A.I., not to cognitive science.

Our own research on chess has been aimed at understanding human chess players, who at most may analyze a hundred branches in a difficult position. Our NSS program (1958), like Al Newell's 1954 proposal, reasoned about

* A history of early computer chess programs, and a description of the Newell-Shaw-Simon (NSS) program, can be found in "Chess-playing Programs and the Problem of Complexity" (1958, particularly pp. 322 31).

positions in terms of goals, and did only a little analysis. (It also played poor chess, to the temporary delight of Hubert Dreyfus, the well-known professional critic of artificial intelligence.)

Working with my son, Pete, and subsequently with George Baylor, a graduate student, I constructed another chess program, MATER (Baylor and Simon 1966), that, using highly selective and humanoid methods of analysis, was formidable in finding deep mating combinations but useless in other aspects of chess playing. In its special domain, it demonstrated the power of selective heuristics to avoid extensive search, supporting the claims I had made in the appendix to my "Behavioral Model" paper (1955a).

With another student, Michael Barenfeld, I worked with some of these same ideas to simulate the eye movements of an expert chess player scanning a novel chess position, thereby refuting arguments advanced by Gestalt psychologists that computers could not model the intuitive, "grasp-it-all-at-once" processes of human experts. We showed that human eye movements, which appear to indicate a grasp of the whole position, could be produced quite simply, using commonplace chess heuristics and without any special holistic Gestalt processes.

The research on perception led to work with Kevin Gilmartin, and ultimately to studies on expertise in chess with Bill Chase. These studies of perception solved many of the problems that had confronted us in 1955 and that had diverted us from chess to logic as our first task of computer simulation.

The Rand Summer Seminar, 1958

By the spring of 1958 we had carried out extensive experiments with the Logic Theorist; the General Problem Solver had been conceived and hand-simulated; the Newell-Shaw-Simon chess program was running; the EPAM program was under construction; and human protocol data were being gathered and analyzed. Research was also now progressing at a lively pace at M.I.T. (with Marvin Minsky and John McCarthy), but with emphasis upon artificial intelligence rather than cognitive science.

Communication was still weak between our efforts and other main lines of development in information-processing psychology. Among the most important of these were the burgeoning field of psycholinguistics, whose leading representative within psychology was George A. Miller, then at Harvard; work in concept formation deriving from information-theoretic points of view (by, among others, Carl Hovland at Yale, Jerry Bruner and colleagues at Harvard); and research focusing on "vigilance," attention, and

the processes of short-term memory, Donald Broadbent's laboratory in England being an important example.

The Ford Foundation had given the Social Science Research Council a small sum of money to be spent on cognitive psychology. Responsibility for spending it was assigned to a committee consisting of Hovland and Miller, who then co-opted me. The committee in turn asked Newell and me to organize a summer seminar in 1958 at the RAND Corporation in Santa Monica, aimed at acquainting a wider circle of social scientists with computer simulation and its use in psychology. Lectures and seminars were conducted principally by Newell and me, but also by Hovland, Miller, Minsky, and Shaw. Programming instruction was provided in IPL-IV, then running on the RAND computer, and the curriculum was built mainly around the principal programs already constructed or under constructionespecially LT, GPS, the NSS chess program, and EPAM.

In addition to the group that organized the seminar, the participants included a number of persons who subsequently played a significant role in developing computer simulation methods, relating those methods to classical psychological approaches and naturalizing them on university campuses.* Dan Berlyne, in *Structure and Direction in Thinking* (1965), examined the relations among information-processing psychology, Piagetian psychology, and Hullian learning theory. Bob Abelson's "hot cognition" research (1963) was one of the earliest attempts to bring motivation and emotion within the scope of the information-processing paradigm. Bob Abelson, Jim Coleman, and Bill McPhee were among the principals who organized the Simulmatics Corporation, which sought to import simulation methods into social psychology.

Bert Green led the team that produced the BASEBALL program (Green et al. 1961), an important early application of artificial intelligence ideas to information retrieval. He also directed the RAND summer seminars in 1962 and 1963. Hovland, with his student E. B. Hunt, constructed information-processing models of concept formation (see Hunt 1962). Don Taylor extended the information-processing approach to problems of motivation, and wrote several expository reviews on problem solving (1960). Roger Shepard made the case for information processing to the (then still very skeptical) verbal learning psychologists (1963).

Many other activities can be traced to the 1958 seminar or received an impetus from it. Perhaps most important, in terms of its subsequent influence

* The effects of this seminar were further reinforced by additional workshops of the same kind held at RAND in the summers of 1962 and 1963. A fourth was organized at Carnegie Mellon in 1972.

in psychology, was *Plans and the Structure of Behavior,* a book Miller wrote in collaboration with Galanter and Pribram during the year 1958 59. As the authors were firmly rooted in the psychology establishment and, prior to writing that book, in behaviorism, the book gained wide attention for information-processing psychology as a radical alternative to the prevailing behaviorist paradigm.

The writing of the book caused a quarrel with George Miller, which strained my relations with him for a short while but which was settled wholly amicably and has not prevented us from being good friends ever since. I will let George tell the story in his own words:

> The next year [i.e., after the RAND seminar] I spent at the Stanford Center for Advanced Study in the Behavioral Sciences, and Eugene Galanter and Karl Pribram were there. And I'd come along with all this material from this summer seminar. We began meeting together, and our discussions got rather interesting, so we decided we should record them; and the first thing we knew we'd written a book. We showed it to Newell and Simon, who hated it. So I rewrote it, toned it down, and put some scholarship into it, and it is now the book *Plans and the Structure of Behavior* (1960).
>
> Newell and Simon felt that we had stolen their ideas and not gotten them right. It was a very emotional thing. Since then I've discovered the good thing about Herb is that he can be shouting at you one minute, and the next minute have a drink with you. You just don't back off with Herb Simonotherwise he'll bully the hell out of you. His aspect is different from any other person's I ever knew. I had to put the scholarship into the book, so they would no longer claim that those were their ideas. As far as I was concerned they were old familiar ideas; the fact that they had thought of it for themselves didn't mean that nobody ever thought of it before. [From an interview with Bernard J. Baars, in Baars 1986, p. 213]

The references and footnotes in *Plans* give a good picture of the role that the RAND seminar and the previous work of the RAND-Carnegie group played in its conception (there were some eighteen references to it, twice as many as to any other work), as well as the influence of the other channels of information-processing psychologyparticularly the linguistic and "human factor" channels. The book also gives an excellent picture of the "prehistorical" zeitgeist that complements the account I have given here.

Stating the Theory:
Human Problem Solving

From an early date, Al and I had decided to write a treatise on human problem solving based on our research with human subjects and computer simulation. The project may have been launched as early as 1958 (the evidence is inconclusive); the published volume appeared in 1972.

Human Problem Solving begins by introducing information processing, computer simulation, and problem solving by heuristic search. Then it describes our empirical investigation of three task domains: logic, cryptarithmetic, and chess, and concludes with an exposition of the theory of problem solving we inferred from the evidence.

LT and list-processing languages were used to illustrate the introductory discussions. GPS provided the machine for simulating human behavior in the Moore-Anderson logic tasks. The NSS chess program and other chess-playing programs were compared with evidence on human chess playing.

The cryptarithmetic puzzles led us to an important new insight. In a cryptarithmetic puzzle, the subject is given a pseudo-arithmetic problem, for example: SEND + MORE = MONEY. The problem is solved by substituting digits for letters in such a way that the result is a correct sum. For example, 9567 + 1085 = 10652 (where *S* has been replaced by *9, E* by *5, N* by *6, D* by *7,* and so on) is a solution to the problem just given. The general method of means-ends analysis, the workhorse of GPS, seems to apply to these problems as to the others.

The basic GPS control structure that determined what step a subject would take next did not predict our protocols well, however. In the protocols we saw something that looked like a *production system,* a form of organization already well known in computer science that had not been applied to psychological systems (except to the extent that any primitive stimulus-response, S-R, connection can be regarded as a production).

In a production system, each elementary instruction has an if-then form: If conditions C are satisfied, then take action *A. Whenever* the conditions of a production are satisfied, the action is taken. When the conditions of several productions are satisfied simultaneously, these conflicts are resolved by priority rules. (For example, the productions may be listed in order of priority.)

Since the mid-1960s, when we introduced productions into psychological theory, they have been widely adopted, both to explain how human experts make "intuitive" decisions by recognizing familiar cues directly, and as the basis for so-called expert systems in artificial intelligence. Experts, human and computer, do much of their problem solving not by searching selectively

but simply by recognizing the relevant cues in situations similar to those they have experienced before.

Production systems were important for the shift in cognitive science and artificial intelligence in the 1960s from systems like GPS, which relied on general problem-solving skills, to systems that relied on large stores of specific knowledge. Of course, most expert systems (human and computer) rely on both.

We might trace the production system idea, as applied in psychology, back along several paths. First, a production, Condition → Action, bears some resemblance to the stimulus-response connection, S → R, of behaviorist psychology. The stimuli are the conditions that trigger the response. When we examine details, we find many differences, but the analogy is close enough so that causation cannot be dismissed out of hand.

Second, we can find early examples, in the literature on problem solving and decision making, of references to intuitive solutionthat is, solution by recognition. The following passage is from a talk I gave on August 26, 1957:

> One can train a man so that he has at his disposal a list or repertoire of the possible actions that could be taken under the circumstances. . . . a person who is new at the game does not have immediately at his disposal a set of possible actions to consider, but has to construct them on the spot, . . . a time-consuming and difficult mental task. . . . [T]he decision maker of experience has at his disposal a checklist of things to watch out for before finally accepting a decision. . . .
>
> A large part of the difference between the experienced decision maker and the novice in these situations is not any particular intangible like "judgment" or "intuition." If one could open the lid, so to speak, and see what was in the head of the experienced decision maker, one would find that he had . . . at his disposal repertoires of possible actions; that he had . . . checklists of things to think about before he acted; and that he had mechanisms in his mind to evoke these, and bring these to his conscious attention when the situations for decision arose. Most of what we do to get people ready to act in situations of encounter consists in drilling these lists into them sufficiently deeply so that they will be evoked quickly at the time of the decision.

But we can find the idea of production systems, or a similar idea, in the psychological literature much earlier. The idea of condition-action pairs was central to Otto Selz's theory of problem solving. In 1924 he wrote:

> Intellectual processes are not a system of diffuse reproductionsas association psychology thoughtbut rather, like a system of body movements, particularly

of reflexes, they are a system of specific reactions in which there is as a rule an unambiguous relation between specific conditions of elicitation and both general and special intellectual operations.

In computer science, production systems, derived from formal logic (see, for example, Post 1941), were applied to the design of so-called string-processing languages and to systems programming tasks in the early or mid-1960s. At Carnegie Tech production systems quickly infiltrated into thesis projects. One student, Tom Williams, used a production system language in a 1965 dissertation, and another, Steve Coles, used one in a 1969 thesis (descriptions of both of these systems can be found in Simon and Siklóssy 1972). Meanwhile, Al and I were using production systems by 1965 to analyze chess and cryptarithmetic protocols.

It appears, then, that the idea that expert "intuition" is to be explained by recognition mechanisms was already abroad in 1924 (and probably much earlier), and that production systems were being used for cognitive simulation as early as 1965. And these ideas seem not to have a single line of antecedents, but several different ones.

Representation and Meaning

In most of the work incorporated in *Human Problem Solving,* Al and I collaborated closely. But around 1960, as we each began to work with our own graduate students, our paths began to diverge toward many projects separately. For me, the work with doctoral studentsEd Feigenbaum on EPAM, Bob Lindsay on inference and natural language understanding, and Ken Kotovsky on letter series extrapolationwere among the first instances of that divergence.

Meanwhile, I was becoming less directly involved in the GPS research, which Al continued with his student George Ernst. Nevertheless, during the 1960s, Al and I continued a frequent and close discussion of all the issues on which we were working, as indicated by the acknowledgments in our respective papers. We continued to have common concerns, but we began to develop somewhat different strategies for pursuing them: I tended to build models for specific tasks, testing them against human data; Al devoted more attention to general issues in the design of complex systems. It is easy to make the distinction sharper than it was, however, as Al worked on protocols in the cryptarithmetic, logic, and chess tasks, and my efforts extended the information-processing theory to account for motivation and emotion, perception, and creativity.

The specialization of efforts can be seen in the bibliography of *Human*

Problem Solving. In the years 1956 to 1962, there are no entries in which Al Newell is sole author, but nineteen co-authored by some combination of Newell, Shaw, and Simon. Similarly, from 1957 to 1961, there are no entries in which I am sole author. From 1963 on, I begin to appear frequently as co-author with students and with other faculty colleagues. Newell appears as co-author with Ernst during this same period, and as sole author of a sequence of well-known papers on the architecture of intelligent systems. Toward the end of the decade, his publications also deal with analyses of protocols that were later incorporated in *Human Problem Solving.*

I spent the year 1960 61 in Santa Monica, on leave at RAND, where I worked on two main projects in addition to my continuing collaboration with Al. One was to begin a major revision of EPAM, the system that Ed Feigenbaum had built for his dissertation. The other was a study of automatic programminggetting the computer to write its own programswhich eventuated in a system I called The Heuristic Compiler (HC). HC generated programs automatically, using a GPS-like mechanism. While it never went beyond a toy, HC provided a stock of ideas that were later drawn on by others investigating automatic generation of programs.

Questions of Representation and Learning

Problems in the real world are sometimes presented in the form of natural language statements (problems in textbooks), sometimes in the form of visible situations (the road in front of our car), sometimes in a combination of natural language text and pictures and diagrams (a scientific article). The steps that translate a problem from the form in which it is presented to an internal form on which the available problem-solving processes can operate are a crucial initial component of every problem-solving activity.

An explanation of problem solving is grossly incomplete if it does not account for what goes on in *understanding* the problem, or, what amounts to the same thing, in forming an *internal representation* of it. In a program like GPSindeed, in all the early problem-solving programsthe internal representation had to be provided to the problem solver by the user, thereby bypassing this important part of the problem-solving process.

Creating the internal problem representation requires a semantics, that is, information on what the representation *denotes* in the outside world. A semantics is needed both when problem solving begins and, subsequently, when changes in the external situation need to be known by the solver. This requirement is bypassed in problem-solving systems that operate wholly internally (that work out solutions in their "heads"), as most A.I. problem

solvers do today; but it becomes critical in robots that interact with a real-world physical environment (for example, autonomous vehicles).

Creating a problem representation from descriptive statements or pictures is also a form of learning. The information that lies in human memories is produced by transforming information acquired from outside. LT had learning capabilities, for it could store the theorems it had proved and then use these to help prove later theorems. EPAM was primarily a learning system, for it not only stored away response symbols but grew a discrimination net that allowed it to sort or recognize stimuli and thereby gain access to the appropriate responses in memory.

Learning is crucial to a system like the human mind that cannot be changed directly by opening the cover of the box and inserting a new program. Human memory can be altered only by learning. Reflecting on the limitations of the first generation of intelligent programs, we concluded that much of our research should focus on semantics, representation, and learning.

During the 1960s, my students, colleagues, and I focused on these topics, and some of our work was published in a 1972 book titled *Representation and Meaning* (Simon and Siklóssy 1972). Our studies aimed at finding basic mechanisms for understanding, not at achieving detailed matches with human data. They were closer to artificial intelligence than to cognitive science, but paid attention to both.

Thomas Williams (1965) and Donald Williams (1969) (no relation) explained how an information-processing system can use external information to learn to perform a task thereby contributing to all three topics of understanding, representation, and learning. They dealt with two quite different kinds of external information: Thomas Williams with instructions of the sort one finds in Hoyle's book of games; Donald Williams with examples of items on intelligence tests. How does one learn the rules of poker from Hoyle, or the requirements of a test from some illustrative items?

Steve Coles (1969) and Laurent Siklóssy (1968) showed how to extract meaning from combinations of pictures and natural language sentences that described the pictures. Coles used information from the pictures to remove syntactic ambiguity from natural language, while Siklóssy's system learned to produce natural language sentences that described corresponding pictures ("The dog chases the cat" from a drawing of that event). These programs cast important new light on semantics and on how the meanings of symbols can be learned.

Finally, Harry Pople (1969) described a problem-solving system that used two different kinds of internal representations: one described situations with explicit propositions, the other represented them by modeling them. His

work addressed an issue that is still very much alive in artificial intelligence: the respective roles of logical reasoning and selective search using mental models in problem solving.

To my considerable surprise and chagrin, *Representation and Meaning* made no splash at all, and these fine pieces of research appear to have had little impact on later work. They all introduced important new ideas about semantics and opened up paths that had to be re-explored a decade later. Siklóssy's theory of language learning is, in my view, still in advance of anything else that has been done on that topic. I can only think that this work was too far ahead of its time, that it provided answers to questions that other researchers had not yet begun to ask.

Another Carnegie Tech dissertation written under my supervision during this same period and responding to some of the same questions was Ross Quillian's *Semantic Memory* (1966). It had a very different fate from the others. Quillian proposed a network model of memory that spread activation through memory to resolve ambiguities in the meanings of words occurring in sentences. The presence of other words in the same sentences provided a context that could activate the relevant lexical alternatives. In the context of "bird," *flicker* would be interpreted as a large woodpecker; in the context of "light," as a fluctuation in intensity.

Quillian published his dissertation in 1968 in a volume of theses (all from M.I.T. except Quillian's) edited by Marvin Minsky, titled *Semantic Information Processing*. That volume, which contained interesting work by Bobrow, Rafael, Evans, and others, also bearing on semantic questions, was well received and did not lack attention in computer science. When Quillian and Collins carried out some experiments testing the network theory empirically, it began to receive attention in psychology also, and Quillian's system was a direct forerunner of subsequent models that use a spreading activation mechanism. The stark contrast between the success of Quillian in arousing interest and the failure of the others remains a great mystery to me and, except for the case of Quillian, a major disappointment. I have always felt that I somehow let these colleagues down.

The response to the experimental and psychological research of this same period was more satisfactory. Dan Bobrow at M.I.T. had built an early system for solving algebra word problems, using syntactical knowledge almost exclusively—that is to say, the system solved problems "mechanically" without understanding what they were about. With a Harvard undergraduate who came to work with me for a summer, Jeffrey Paige, I ran some experiments on high school students solving algebra problems, to see how closely their behavior fit Bobrow's program.

We discovered that our subjects divided in two groups: one interpreted

the problems syntactically, as Bobrow's program had, but the other used the real-world meanings of the problems. When the problem spoke of a board being cut in two parts, the "syntactic" subjects parsed the sentence; the "semantic" subjects imagined a rectangular figure with a line drawn across it to represent the cut, or two figures after the cut had been made. This paper has become over the years a standard item in the problem-solving literature. It is today evident that successful problem solvers frequently use diagrams to mediate between words and their inference processes.

Similarly, the experimental work with Kotovsky on letter series (Simon and Kotovsky 1963; Kotovsky and Simon 1973) gradually caught on, as did the research on chess perception with Barenfeld (Simon and Barenfeld 1969) and Gilmartin (Simon and Gilmartin 1973). But it was a series of papers with Chase (Chase and Simon 1973a and 1973b) on the memory of experts and novices for chess positions that received massive attention from psychologists. It also established a tradition of expert/novice experiments that persists to the present time and has had major impact both on psychology and on expert systems for artificial intelligence.

Shortly after the chess experiments got under way, John R. Hayes and I built a system that could understand problem instructions expressed in natural language and then encode them into an internal representation suitable for a General Problem Solver. This UNDERSTAND system, being farther from the current concerns of experimental psychology than the chess perception research, was a slower starter, but has gradually caught on as a (partial) theory of the understanding of natural language.

I have given only the briefest descriptions of the contents of these research projects, focusing mainly on the issues they addressed and their reception. Anyone interested primarily in substance should consult *Human Problem Solving* (1972), *Representation and Meaning* (1972), and *Models of Thought,* volume 1 (1979), the latter containing a collection of about thirty of my psychology papers of this period, including most of those I have mentioned. For a shorter and much less technical overview, I recommend chapters 3 and 4 of the second edition of *The Sciences of the Artificial* (1981).

Missionary Efforts

From the account thus far, it can be seen that the discipline of psychology was not unduly eager to embrace the new information-processing paradigm. There had been too big a leap from Hullian S-R psychology (not to mention Skinnerian behaviorism) to computer simulation. The use of thinking-aloud

protocols as data was sometimes misunderstood as an attempt to revive introspection. Even the work of Hebb, which had prepared psychology for a more cognitive approach, helped only a little, for his physiological interpretations of processes left little room for a separate information-processing level of explanation.

Because, at the outset of our plunge into psychology, I had only marginal status as a psychologist (through my work on social and organizational psychology), and since Al had none at all, it was of great importance that two prominent psychologists, George A. Miller and Carl Hovland, were early attracted to the information-processing viewpointarrived at it, in fact, from information theory and wartime human-factors research just as Broadbent had in England. After we joined forces with them in the 1958 workshop at RAND, psychologists generally exhibited cautious interest toward the information-processing approach rather than rejecting it out of hand.

Our cause was aided by the parallel development of research on short-term memory and chronometric studies of perception, which also gradually adopted the information-processing label. (Donald Broadbent [1954] is an important example of this work.) That line of research, although it flew the same banner of information processing that ours did, differed from ours in several aspects.

First, it was strongly in the tradition of experimental psychology, with theory playing a distinctly subordinate role to experimentation. Second, it tended to focus on simpler and more traditional laboratory tasks of perception and choice, rather than the complex problem-solving tasks that we often employed. Third, it was not particularly committed, or committed at all, to computer simulation as a way of formalizing and testing theories. Finally, it relied on more standard experimental designs, using speed and accuracy of response as its principal data. It made little or no use of thinking-aloud protocols as data.

What the cognitive or information-processing approaches to psychology had in common, and what distinguished them from behaviorism, was a willingness to consider what lay between the ears and to use words such as *mental* without blushing. Both were interested not only in the phenomena of thought but in its mechanisms and processes as well. Between them, these two varieties of information-processing research continued to gain adherents, until today the information-processing label has become positively faddish. Everyone is an information-processing psychologist.

The existence of the two varieties of cognitive psychology helps explain why the experimental side of my psychological research (for example, the work with Bill Chase) caught on much more rapidly than did the computer

models (such as the EPAM theory of verbal learning, which still suffers from neglect in spite of the wide range of experimental data it explains).

As for our work in computer science, GPS had an enormous success, but the other systems I have built or helped to build, like the Heuristic Compiler and UNDERSTAND, have had less impact. One possible reason is that the problems I have worked on have tended to be rather specific, and have required attention to psychological detail rather than to more general principles of system architecture.

The Signs of Recognition

I hope that readers will not detect in this account any hint of self-pity. Any claim that the world has neglected our research would fly spectacularly in the face of the facts, and could even be a symptom of acute paranoia. My account aims, rather, at understanding why and at what times particular meteors fall from heaven with a terrific crash while others slip silently and unnoticed into the sea.

There were many tangible evidences of recognition of my work during this period, although it is not always easy to know just what was being recognizedwhether the research on organization and administration, on bounded rationality, on artificial intelligence, or on cognitive psychology. Fairly early, I was invited to give lectures or series of lectures on various university campuses: by New York University in 1959, to speak on management; by Princeton, to give the Vanuxem Lectures in 1961; by Harvard for the William James Lectures on cognitive psychology in 1963; and by M.I.T., for the Compton Lectures on artificial intelligence in 1968. The NYU Lectures produced my book *The New Science of Management Decision* (1960, 1965, 1969), and the Compton Lectures, *The Sciences of the Artificial* (1969, 1981). The Vanuxem and James lectures were not published, for their content was too closely interwoven with the joint work with Al that eventuated in *Human Problem Solving*.

Honorary degrees and awards also began to come my way: degrees from Yale and Case (1963) and Chicago (1964) were the earliest. In 1959, I had been elected to the American Academy of Arts and Sciences (the "Boston Academy") and the American Philosophical Society (the "Philadelphia Academy"), probably as much as anything for the visibility gained through my activity in the Social Science Research Council.

In 1969, the American Psychological Association awarded me its Distinguished Scientific Contributions Award (which troubled me somewhat, for it should have been a joint award with Al, who had to wait until 1987

for his). In 1975, Al and I received the Turing Award from the Association for Computing Machinery, probably delayed some years while the association decided what to do about joint awards. Long before the 1970s ended, Al and I had been fully legitimized in the cognitive science and artificial intelligence communities.

I soon learned that one wins awards mainly for winning awards: an example of what Bob Merton calls the Matthew Effect. It is akin also to the phenomenon known in politics as "availability," or name recognition. Once one becomes sufficiently well known, one's name surfaces automatically as soon as an award committee assembles.

Two decades of research took artificial intelligence and cognitive psychology a long distance across the plain that we saw from the vantage point of the Logic Theorist, and our university was a major center of the research contributing to this advance.

The initial research strategy never changed in any essential way. We identified significant intellectual tasks that people performed, either for recreation (puzzles and games) or as part of their daily occupations (investment decisions, pattern discovery, verbal learning). If we thought our understanding had reached the point where there was a good chance of writing computer programs to perform the tasks, graduate students and faculty members undertook to construct these programs and to test them for effectiveness or for conformity to evidence of human performance. As we came to understand the simpler domains of the mind, we undertook increasingly complex ones. The boundaries of the explored land advanced steadily, but there was always ample terra incognita beyondand the year 1978, with which this chapter ends, gave no signs that it would all be explored soon.

Chapter 15
Personal Threads in the Warp

Beyond my boyhood and college experiences and the first years of marriage, I have so far said relatively little about my personal and family life. Without aspiring to the same frankness I have shown in describing my professional life, I will pick up the thread of my personal life, well aware of the artificiality inherent in separating the two. My professional activities were not unwarmed by emotion; a large part of the pleasure I have had in life has come from them. Nor has this been a purely solitary or intellectual pleasure. Most of my research and all of my politicking have been collegial, social affairs. I have worked with people I liked (and occasionally with some I did not like), and enjoyed deeply the associations and friendships. And, for both my wife and myself, social life has been confounded with professional life, as most of our friends, our hosts, and our guests have been part of academe, often Dorothea's or my departmental or office colleagues and their families. There has been no definable boundary between the sociability of work and of leisure.

As with most families, our life has gone through phases closely synchronized with the growth of our children. During the years in Chicago (1937 39) and Berkeley, we were two. In the second Chicago period, at Illinois Tech, we grew to be five. By about 1961, the children were mostly away at school, and we were two again. So we have remained, except for visits with children and grandchildren. I will try to characterize our life in these various conditions.

Family Life

You have already had some account of my interest in girls and women. If the statistics of the Kinsey Report are at least approximately correct, I would have to judge the strength of my libido and my response to it as lying somewhere close to the middle of the distribution. I cannot remember a time when 1 was not interested in girls and, later, women.*

For more than half a century, my marriage to Dorothea has brought deep companionship and love. Meeting her, on the eve of my twenty-first birthday, was no more, or less, a matter of love at first sight than had been my first meetings with some other beautiful women, but in this case a good beginning progressed to a full and satisfying relationship and a wedding six months lateron Christmas Day, 1937. Dorothea and I have been either very clever or very lucky ever since.

In spite of the expert status that fifty years of the institution should bestow, I have little to say about marriage that has not been said better by competent marriage counselors. In retrospect it seems easy, but the road was not without buried mines that had to be detected and removed. Dorothea and I started out with strong shared interests in political science and liberal politics. I was a little worried (I am not joking) that she did not know calculus, but she promised to remedy that, a promise she fulfilled only many years later. I wanted my wife to share all my interests, which included mathematical social science, but that did not wholly work out.

The interests we did share, soon supplemented by those of our social life and then of raising children, were quite enough. We shared liberal political views and activities; before the children arrived, we wrote and published a number of papers together on municipal government. When we married, Dorothea had been pursuing a Master's degree in political science. We decided that each of us would take time off from our jobs to complete our degrees, and we each finished our requirements while we were living in California.

Although I would not pass muster as a feminist by present-day standards, I certainly did not have my father's hangup about a wife's contributing to the family budget. The problem of sharing housework when we were both employed was solved by eating out often and hiring a housekeeper. The rules under which we launched our undertaking would now be regarded as

* I will soon be able to echo the comment of Justice O. W. Holmes, Jr., eyeing the young secretaries descending the steps of the Supreme Court building as they modestly clutched their skirts against a whipping breeze: "Oh, to be eighty again."

old-fashioned, but at the time were progressive, although perhaps not avantgarde.

It was assumed that my career took priority. We agreed that climate or other geographical considerations were not important in choosing or changing a job. I would go wherever I could do my best work, and Dorothea would go with me. That may seem a major concession, especially since she was a native Californian, but she has never had a particular yen to return there. Nor did it seem a major decision at the time, or even a decision at all, that her employment would have to fit itself to mine.

Dorothea would work as long as she liked, but as we began to raise a family she would probably manage our household for some years. Again, my recollection is that this was more an implicit assumption than an explicit decision requiring discussion. After Kathie's birth, Dorothea did continue working for two years, but then became a full-time mother and housewife, albeit with a heavy schedule of volunteer activities in the League of Women Voters and several other organizations, until the children were grown. At that time, she went back to school to prepare herself for a new career in educational research, and as a result, we again had the pleasure of working and publishing together, this time in cognitive psychology.

Under the rules by which we were playing house, the size of our family would have more impact on Dorothea's life than on mine. Therefore, she had the majority vote in making the decisions, though I don't recall that we disagreed on them. We waited until near the end of our graduate studies to have Kathie, in 1942, and Peter and Barbara arrived two and four years later, respectively. In that era, it appeared that all academic families had three children, so we conformed to the mode.

In that era also, the role of fathers in the birthing process was minimal. I believe I was at the movies when Kathie was born, having been assured at the hospital that nothing was imminent. But I dutifully sat in the waiting room while Pete and Barbara were making their way into the world. (In the case of Pete, I brought along a book on vector analysis to while away the time. I don't recall what my reading matter was when Barb was born.) I learned to change diapers and took my turns on the night shift, but otherwise Dorothea took responsibility for the infants.

By disposition I was the disciplinarian of the family, and differences in our attitudes toward discipline were sometimes a major cause of stress between Dorothea and me, particularly as it began to appear that Pete, impatient of authority from the beginning, was going to have a stormy youth. In any event, the now-grown children tell me that I was a stern father, but reassure me that I am not now a hostile figure in their memories of their youth.

Although a disciplinarian, I probably was not a consistent one, and that for two reasons. First, I have a deep respect for independence of mind and spirit. My graduate students would be willing, I think, to attest to that. When Pete was having his greatest battles with me and with much of the world, I secretly admired his stubborn pluck. I certainly never crushed his spirit, although life might sometimes have been briefly quieter if I had been able to.

The second reason for my inconsistency in discipline is that I have been for most of my adult life a workaholic, toiling (more accurately, enjoying my work) for sixty to eighty hours a week, sometimes more. I enjoyed my children, but was never very good at entertaining them for long periods of time beyond reading to them in the evenings. I had too little patience to listen to them at length or to play many games at their level. Nor had my own father felt responsible for entertaining me much as a young boy, although I did spend many hours watching what he was doing. But his activitiesgardening, carpentry, fishingwere infinitely more watchable than mine of writing papers.

So I was not a stellar father. Moreover, I had strong doubts that I knew what was best for my children, or that I could predict the consequences of one way of treating them over another. Because my own childhood had been handled with a good deal of laissez-faire, my convictions about a father's role were that children should not be overguided or overprotected. The thought of Little League baseball, with its cheering, involved parents, sends chills down my spine. In my boyhood, baseball was self-organized, played in a vacant lot or alley. Adults were not welcome.

In recent years I have felt much better about my fatherhood. I seem not to have done permanent harm. Kathie, Pete, and Barb, now in their forties, are progressing through interesting, challenging lives. They are warm and affectionate, evidently holding no deep grudges against their parents. On visits, we have good times together. Their basic values are ones I can respect (that is, they are much like mine). Dorothea and I have shared many of their problems with them, and they, with us. And for more than twenty years now, we have been watching the same process unrolling with our grandchildren, who now number six. As one grandchild is now married, we may even be greatgrandparents soon. I note with satisfaction that none of the children or grandchildren seems to have been unduly damaged or intimidated by my notoriety in the world.

Then there is the matter of money, a topic that must command some attention in life even from two who can live as cheaply as one. Throughout our married life, income has fortunately never been a serious problem. As has often been observed, there is no difficulty in maintaining solvency if

you spend no more than you earn. In particular, laziness and lack of imagination are great aids to solvency. Buying a vacation house or a sailboat or a second car, or even remodeling one's residence, calls for initiative and energy. Dorothea and I have always seemed to be too busy with other activities to find much time for those things.

During two periods, however, we were a bit pinched on my academic salary. When we bought a house in Chicago in 1948 and had to do it all over again in Pittsburgh in 1949, we had essentially no savings, and it was lucky that my parents could take our second mortgage. Later, when Kathie was in college and Pete and Barbara in private schools, the budget stretched tight, but I was able to supplement my salary from RAND and other consulting. In recent years, I have been in the fortunate position of being able to accept or reject opportunities for consulting and lecturing on the basis of their professional interest and without regard for income.

I allow myself a total of about four days a year to attend to investments and other financial matters. I haven't checked the record in detail, but I have the strong impression that we have done approximately as well as if we had put our savings in an index fundperhaps better. But it is easy to delude oneself in these matters unless one carries out the calculations. My training as an economist has been of help in one important way: It convinced me that the only information that is of value in a financial market is information that other people don't have.

This means that I don't have to pay daily, or even monthly, attention to the stock market, since it tells me nothing about whether I should buy or sell. Hence, the turnover on my investments is very low, to the discouragement of the brokers with whom I do business (and from whom, also, I never take tips or advice). Nothing that I learned while serving on the Finance Committee of the Carnegie Mellon Board of Trustees has shown me that this strategy is wrong. It satisfices; perhaps it even optimizes.

This is not to imply that I dismiss money as unimportant. Dorothea and I have always had enough for our needs, and usually more. If that had not been the case, I would undoubtedly sing a different tune. Moreover, making a lot of money (I mean a *lot*) might be a fun game. It just is not a game I have had occasion to play, and I might have some qualms about its zero-sum aspects, as it is usually played. Apart from the game aspect, I have a hard time imagining why people want the stuff. Such trouble!

One writes about a life in terms of Big Issues and Critical Events. But on rereading these pages, I see how little they convey of our moment-to-moment existencethat is, of most of the hours of my life. Gertrude Stein called it her "daily living"; *infrastructure* is a trendy word for it, and it is as fundamental to a life as to a society. The moment-to-moment life Dorothea

and I have led has been simple. We love our home, and we try to make it attractive and comfortable to ourselves, but have never tried to make it a work of art. We like good food, but eat simply. We entertain little, and that for friendship and not to maintain a social position. We enjoy each other's company (and solitude) sufficiently that we don't go out very much. We like good music and good art, but don't often exert much effort to experience it. The television and stereo are rarely used, the piano (amateurishly) more often. In recent years, we don't even get to the movies frequently. But we are voracious readers with many common tastes.

Sticks in the mud, you might say.

Recreations and Diversions

If we have ever exerted effort in entertaining ourselves, it has been in our travels, domestic and foreign. In the early days of our marriage, we several times took hiking vacations, which I have described in the first panel. We did not camp with our children, howeverprobably because of laziness about the logisticsbut, usually in the summers, took long auto trips with them: three or four to the West Coast, a trip to Maine and Quebec, trips to Atlanta and Ensenada, and many others. Those trips were good fun (for all of us, I think) and have given me my best memories of being a father.

Our home emptied out rather early. When Pete had serious problems with us and with the local schools, we found a school for him in New England that guided him with patience and success. When Barbara, after an especially happy year in the Santa Monica High School, was desolate at the prospect of returning to a less congenial Pittsburgh school, we found a private school for her, also in New England. Kathie, meanwhile, had gone to college, first in Ann Arbor, then in New York. By 1961, Dorothea and I were mostly alone in our large house. The glue that holds us together is made of a habit of mutual love and affection, joint professional activities, and a shared curiosity about almost everything around us. We can gossip, not too maliciously, about our neighbors and our friends, about our children and relatives, about our respective daily activities. The encyclopedia, the atlas, the dictionary, and the world almanac frequently arrive at our dinner table to answer a question or settle a dispute. My Washington, or Dorothea's Pittsburgh, committees, science, politics, religion, art, funny or ridiculous things we readall are grist for our mill. And, if all else fails, we sometimes bicker or retire to our respective workrooms.

Since the 1960s, our travels have most often taken us abroad, and have been as much urban as rural. Usually they have been associated with my

professional meetings or lectures, but with more recreational than professional intent, at least until the trip to China in 1972. In chapters 20 and 22, I will give an account of some of the more notable of these trips, especially those to Scandinavia, China, and the Soviet Union. Our many vivid shared experiences provide an increasingly pleasant source of reminiscence, a disease of old age that we do not attempt to resist.

Then, of course, there are the thousands of hours that have been taken up in hobbies. My principal ones have been hiking, piano, chess, painting and drawing, and acquiring foreign languages. As a boy I enjoyed, but was not good at, most sports, and in some other life would have pursued skiing, sailing, and tennis much farther than I did. Of my five main hobbies, only hiking can be sociable (unless you count chess, which provides a curious kind of sociability), and it is the only one I have shared much with Dorothea. She has had her own occupations, especially weaving and other crafts, and gardening.

Time is the tyrant. One cannot be loyal to two occupations any more than one can to two lovers. Whenever I found that one of my hobbies was seriously taking attention from my research, I dropped it. That happened to chess, and then painting, in the late 1950s. In both cases, I found that I was aspiring to professional competence, which obviously would have required an unlimited commitment. It was time to call a halt. It probably says something about my competitiveness that I often found myself getting serious about activities that were begun as diversions.

I spent two years in somewhat serious chess play as a high school student. The city recreation department provided chess-playing facilities in the evening. There I met Arpad Elo, the man responsible for the universally used chess rating system that tells whether a player is a master, an expert, or only an A player. (I never got beyond an A rating.) One evening I played Elo and lost as usual, playing White in the Giuoco Piano. When I got home, I reanalyzed the game and found that I could have beaten him easily if I had made the correct aggressive move with my Bishop on the seventh move, I believe. The next evening I pointed this out to him. "Oh," he said, "but our game was last night."

Wisely, I gave up serious chess in college. Three of us did play a consultation game against Edward Lasker when he visited the campus, and beat him. Many years later, I showed him the score of the game, and he ruefully pointed out where he had made his blunder. Chess remained on the back burner until we began work on the NSS program. Then I began to play regularly at the Pittsburgh Chess Club to raise the level of my sophistication, using the research as my excuse.

Soon I was playing in the city tournament, with a rating of 1,853 that

was rising fairly rapidly. I even beat the strongest player in the city at that time. (He was overconfident against a weaker player and tried for a win when he should have been satisfied with a draw.) Then I began to feel the juices of competitiveness rising within me, and dropped my chess at once. I could not have maintained the pace unless I devoted at least one or two days a week to the game.

With drawing and painting, I never thought I had any real talent, and my color-blindness certainly did not encourage me to cultivate these arts. At Christmastime, I think it was in 1958, and possibly partly to distract myself from personal worries (discussed in the next section), I began making collages and then switched to oil paint. A little later both Dorothea and I took some drawing lessons, and discovered that the ability to represent the world is at least as much a matter of skill as of talent (like everything else).

I was fascinated by painting. First, there were some puzzles to solve: What kind of a palette can a color-blind painter use without ending up with a totally confused canvas (since the green spots and the red spots you have laid down may be wholly indistinguishable)? But beyond this challenge, painting began to grip me and to occupy my thoughts when I might have been thinking about my research. I found it a terribly demanding and satisfying activity. After a year or two, I tapered off and have not gone back to it, but I am tempted from time to time.

My drawing I have kept up a little, when I travel. I sometimes carry a sketchpad, though never a camera, and am now surrounded (in a "hideaway" office on campus to which I can retreat) by sketches of mine that remind me of a favorite Japanese inn, of Hongkong, of Tianjin, of the Swiss Alps, of the Santa Monica beach, and of Panther Hollow in nearby Schenley Park. I have no illusions about the quality of the drawing, but it is great for evoking memories.

Hiking is not a competitive sport, so it poses no serious problems of loyalties. I do not aspire to become a world-class hiker and would be scared out of my wits if I developed aspirations to climb serious mountains.

It is harder to explain why I have not been more serious about my pianistic skills. As a child, I was reluctant to practice and, to avoid my teacher's censure, had to develop a considerable skill in sight reading. The advantage of this is that I can leave the piano for long periods of time and return to it still able to maneuver through Mozart sonatas, Bach preludes, and the like, with some approximation to accuracy and nearly up to tempo. My playing is no worse, and no better, than it was in my seventeenth year, and I feel badly about that only intermittently. Oddly, I never commit a piece to memory, no matter how often I play it.

I have devoted a little time, but only a little, to musical composition, and

have engaged in research on computer programs capable of doing musical analysis. (You can find a report on one of them in volume 2 of *Models of Thought*.) Here, as in painting, I know that my aspirations could rise rapidly, and I have been correspondingly wary about becoming involved.

I count reading in foreign languages among my hobbies, for I have probably spent more time in that than in the other four combined. But I have already given some account of my language interests. And while we are on that topic, why haven't I mentioned reading in general? It is, of course, much more than a hobby. It is one of life's main occupations. As with eating, so with reading, I am nearly omnivorous. But my stomach for words is hardier than my stomach for rich foods, so I do not ration myself.

I am often complimented on the range of my interests. But you can see from this account that I control them severely. Moreover, the fact that understanding human thinking is my reigning passion has a curious consequence. For I can rationalize any activity I engage in as simply another form of research on cognition (and perhaps emotion as well). I have published on chess, on musical pattern, on the Chinese language, and on many other topics I simply stumbled into. I have also encouraged and followed the work of others in computer drawing (notably Duane Palyka and Harold Cohen), although I have not published anything myself. In a way, I can always view my hobbies as part of my research. It's the best of all possible worlds.

Love and Marriage

Love has played not a small part in my life. By the broadest, Stendhalian or Proustian, definition of love, I have never been in love with a girl or woman who was not beautiful, nor wholly out of love with one who was.* The criterion of beauty is not necessarily classical: A face must be interesting, not just flawless. And beauty has difficulty showing itself unless the face and eyes are lit by intelligence. I have always felt that, since faces have greater variety than do bodies, beauty resides somewhat more in the former than in the latter. When I encounter a woman who matches my idea of beauty, I am immediately stirred. But the attraction can wear off in minutes if there is no intelligence.

Love is as important in marriage as in the maneuvers leading up to it.

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* Alistair Cooke quotes from Charlie Chaplin's autobiography: "Procreation is nature's principal occupation, and every man, whether he be young or old, when meeting any woman, measures the potentiality of sex between them" (Cooke 1977, p. 26).

But my admission of susceptibility to beauty might raise questions about how well I have adapted to monogamy.

There are two mind/body problems. The classical one asks how a physical system can have thoughts. It has been answered definitively by the appearance of electronic computers that think. The second mind/body problem, quite different, is that of sacred and profane love. The solution is not so clear as to the first, and attempts to solve it make up a large part of what we call literature. As a young man I had to try to shape my personal answer.

For some years, I thought that body could be detached from mind for some purposesthat sexual attraction might be a precondition for love, but was certainly not synonymous with it (I still believe that), and that sexual acts with a loved one or with another had no implications for one's love (I no longer believe that). I think I arrived at these conclusions in response to my body's wants and not as a result of reading tracts on free love.

With what I now regard as great good fortune, I never fully acted on these earlier principles, and have always held strictly to the law, if not exactly to the spirit, of monogamy. Drunk, and occasionally sober, I have sometimes made advances to other women, but two things have prevented matters from progressing far. The first is my sense of vanity. To be attracted to a woman and find that I was not attractive to her would prick my pride. One sure safeguard is never to make the test, to move with great tentativeness and wait for a response. That is not a very powerful strategy in either love or war. But, in love, it does protect vanity and promote monogamy.

My second defense against profligacy I like to think of as a form of honesty. I have never been able to tell a woman I loved her when I felt only sexual attraction. Don Juan instructs us that most women are not very vulnerable to attacks that do not promise love, and preferably exclusive love. For more than fifty years I have been deeply in love with my wife, and I have been unwilling to say that I was not. None of the standard gambits for philandering were open to me. I could not say that I needed a woman because my wife misunderstood me. Sometimes Dorothea did misunderstand me, and I her, but crying on another shoulder did not seem a way to solve that problem while love remained.

A good logician, examining the sophistry of the preceding paragraphs, will see that I am not yet out of danger. I have not denied (and cannot deny) the possibility of being genuinely in love with two women at once, and this has occasionally happened to me.

About six or seven years after my marriage, a young woman enrolled in one of my classes; I'll call her Karen. She was a couple of years younger than I and possessed of a remarkable, poised, aristocratic beauty. Bright

and imaginative, she was educated in the arts and humanities but not in the sciences. She had most of the attitudes that go with such an education, including a slight distrust of technology and some mild tendencies to mysticism. It was a delight to watch her beautiful and intelligent face during class sessions. We became friends, but on such proper terms that I was never able to decide whether she was sexually attracted to me. I soon learned that she was married and that her husband was away in the army. There were suggestions that the marriage was not successful.

Our conversations were on such topics as city planning and the arts, not on love. After the school year ended, I saw her only infrequently, and never under circumstances that could have led to intimacy. A few times she came to my office at ICMA. She was often late for appointments, sometimes very late, which put me into a tizzy of expectation. I had no doubt that I was in love with her, and no doubt that I was at least as much in love with Dorothea. I resolved to do nothing about the situation, a resolution that was easy to keep, as Karen, though a warm person, never hinted that she wanted more than friendship. Nor did I.

On one occasion, perhaps more, she came to our home (with a suitor in tow). Dorothea liked her and, I think, was not jealous at least there was no sign of jealousy except one afternoon when I arrived home quite late because of a meeting with Karen. I did not think of her often when I was with Dorothea, but on my travels, especially during the summer of 1948 that I spent in Washington, helping to organize the Economic Cooperation Administration, I sometimes dreamed of each of them (never both together). My contacts with Karen became infrequent, and were mostly exchanges of brief notes. I was aware of a divorce, a new marriage, and a second divorce. I usually knew her whereabouts. And I did not forget her.

In the summer of 1958, I was riding an emotional crest. Our initial forays into artificial intelligence had succeeded and were beginning to be recognized. The RAND summer seminar that Al and I had organized had just concluded triumphantly. The last evening, before departing for the Los Angeles Airport, I took a walk along the Santa Monica beach and was swept up in a tide of people who were moving in a dense mass toward the newly restored amusement pier that was just opening that day. I was carried along with them as far as the pier. Then, having to turn back and move against the tide, I was oppressed by a great depth of loneliness and emptiness in my isolation from that mass of happy, chattering humanity. But by the time I boarded the plane, my euphoria had returned.

A few weeks later, I was off to lecture to executive groups in two cities, an activity that keeps my adrenaline flowing. I drove from the first to the second meeting on a beautiful sunny summer afternoon, singing aloud much

of the way, not something I often do. My second lecture was scheduled for the following day. I had a pleasant dinner and a good night's sleep. After the next morning's meeting, a participant introduced himself and said he had been asked to convey greetings to me *from Karen!* She would be arriving that afternoon and hoped to see me.

Once at Rockmarsh (see chapter 2), I had grasped the wire of the electric fence while standing in the stream. The jolt I had just received, amplified by the euphoria I had been feeling for some weeks, hit me as hard. With difficulty, I fixed my attention on the business of the day.

Karen arrived toward the end of the afternoon (on time), and the three of us drove back to New York together, she driving the car and I trying to convey my feelings while being discreet in the presence of our companion, a task I found very hard. The same evening, I flew home to Pittsburgh, having plunged over the brink of an emotional precipice.

Having told Karen of my love for her, during the next few months I tried desperately to find a way to meet her on more than just a friendly basis, without denying my commitment to Dorothea. Karen acknowledged that she found me attractive (thus assuaging my vanity), but would not enter into an affair with a married man (divorce was never discussed).

Soon, my feelings of guilt at carrying on this negotiation were depressing me so seriously that I had to level with my wife. The friendly but Platonic conversations with Karen were no longer supportable for me. Both women observed that I was asking for the moon, which I was, and Karen opined that I was probably in love with an imaginary woman, which I may have been, though I did not think so. The two even got together once without me, and psychoanalyzed my problem to their satisfaction.

I did the obvious thing (obvious if you are not in love): I stopped seeing Karen, with just a couple of lapses, each of which was followed by such painful depression that it strengthened my resolve. The salt slowly dissolved from the wound. Today I can recount this tale in bittersweet terms, a sentimental old man retrieving his lost youth.

If I did not acknowledge the importance of this episode, I would be falsifying my life. Moreover, the experience added an important corollary to my theory of love: You can love two or more women at once denying that would be denying my own emotions but you cannot be loyal to more than one. The dichotomy of profane and sacred love is not enough. Unless accompanied by loyalty and commitment, love, even love that goes far beyond sexual attraction, provides an insubstantial base for marriage or for a satisfying continuing association.

The budget of time from which we never escape imposes priorities on us, priorities of values and priorities of people. Commitment in marriage means

that the needs of one other person must hold a special priority in our life. That person must be able to count on us as we count on him or her, and the needs of two persons cannot share the same urgency. It is this combination of love and commitment that has made my fifty-three years with Dorothea so central to the meaning of my life and, I hope, of hers. It took the experience I have recounted to make me understand that. I am embarrassed to be such a slow learner; I am not sorry to have had the experience.

There is a sermon hidden somewhere here, with a moral about the new concept of "relationship" that developed and spread in our society in the 1960s and 1970s. But I guess I will simply leave it to the new generations to work out their own definitions of loyalty. Perhaps they will discover something that I missed.

Chapter 16
Creating a University Environment for Cognitive Science and A.I.

Upon my return to the Graduate School of Industrial Administration after my 1960 61 leave of absence at the RAND Corporation, Lee Bach informed me that, for reasons of health, he was going to resign the deanship. That was very unpleasant news; it was hard to think of GSIA operating under another dean. The national and international success of the school had been so spectacular that Lee's successor would be fortunate to hold the ground gained. It would be like becoming the heir of a Roman emperor who had just made a large addition to the empire.

I was the obvious heir, but the excitement of my cognitive science research combined with my previous administrative experience, including the year as acting dean, had convinced me that I did not want to spend my days in deanly duties. Meanwhile, Dick Cyert, now about forty-one, had been exerting a good deal of leadership within the school. For several years he had been head of the undergraduate Industrial Management Departmentthe job I held when I came to Carnegie Techand he had formed a group of the younger faculty to build an innovative management game for use in the Master of Science curriculum. He and Jim March were finishing their book, *The Behavioral Theory of the Firm* (1963), and he had other good research to his credit.

Bill Cooper soon started a faculty drive in support of Dick's candidacy. Lee Bach was somewhat negative, believing that we should look for an outside candidate; Dick just didn't seem distinguished enough for the deanship of a school of GSIA's prominence. But faculty support for Dick built up rapidly.

I had generally positive views toward Dick, and had been the principal mover in appointing him as department head. I had only one reservation: He seemed to enjoy power too much, a worrisome trait in a leader. (Leaders

should *exercise* power, but enjoying it is another, and more dangerous, matter.) After a long and frank conversation with Dick, I retained my concern about his attitude toward power, but was persuaded that he also had a strong attachment to the goals of GSIAthe power would be used responsibly, not just to advance his career. I also concluded that since he now understood my concerns, we could get along welland we did.

Migration from GSIA

Dick was appointed dean in 1962, serving in that position until he became president of the university in 1972. I had (and have) serious reservations about the direction in which GSIA moved during this decade, but many factors were involved besides the actions of the dean. His most serious shortcoming was that he was bedazzled with mathematics and formal methods. As a result, senior faculty tolerance for nonquantitative research decreased, as well as for empirical work not based on formal theory. Notwithstanding Dick's previous involvement with the behavioral theory of the firm, this research was one of the first victims of the new bias. The mathematically inclined faculty we were recruiting had little taste or talent for empirical research that did not start (and sometimes end) with formal model building.

Over time, a coalition of neoclassical economists and operations research specialists came to dominate the GSIA senior policy committee, making decisions that produced a growing imbalance in the composition of the faculty. Although I had never thought I lacked sympathy with mathematical approaches to the social sciences, I soon found myself frequently in a minority position when I took stands against what I regarded as excessive formalism and shallow mathematical pyrotechnics. The situation became worse as a strict neoclassical orthodoxy began to gain ascendancy among the economists. It began, oddly enough, with Jack Muth.

Jack, as a graduate student, had been a valuable member of the Holt-Modigliani-Muth-Simon (HMMS) team in the dynamic programming research. He was (and is) very bright, and an excellent applied mathematician. In our project, he investigated techniques for predicting future sales and, generally, for dealing with uncertainty. Shortly after completing his dissertation, which was related to the project, Jack published in *Econometrica* in 1961 a novel suggestion for handling uncertainty in economics. He clearly deserves a Nobel for it, even though I do not think it describes the real world correctly. Sometimes an idea that is not literally correct can have great scientific importance. To economists his idea is known today as "ra-

tional expectations." I will explain it here only roughly; a detailed account would take us deep into technical matters that are irrelevant to the story.

The theory of rational expectations offered a direct challenge to theories of bounded rationality, for it assumed a rationality in economic actors beyond any limits that had previously been considered even in neoclassical theory. The name of the theory reveals its general idea: It claims that people's rationality extends even to their expectations about an uncertain future, such expectations being derived, in fact, from a valid model of the economy, shared by all decision makers.

Jack's proposal was at first not much noticed by the economics profession, but a decade later it caught the attention of a new young assistant professor at GSIA, Robert Lucas, who had just completed his doctorate at the University of Chicago.* Beginning in 1971, Lucas and Tom Sargent, who was also with us for a short time, brought the theory of rational expectations into national and international prominence. It is not without irony that bounded rationality and rational expectations, two of the major proposals after Keynes for the revision of economic theory (game theory is a third), though entirely antithetical to each other, were engendered in and flourished in the same small business school at almost the same time.

Not only did they flourish, but they were represented, along with Keynesian theory, in a four-man team that worked closely and amicably together for several years on a joint research project. The HMMS research team harbored simultaneously two Keynesians (Modigliani and Holt), the prophet of bounded rationality (Simon), and the inventor of rational expectations (Muth)the previous orthodoxy, a heresy, and a new orthodoxy.

The rational expectationists, and the neoclassical mathematical economists generally, gradually made GSIA less and less congenial to me. To oppose the trend and secure more tolerance for other points of view, I would have had to devote most of my time to the politics of GSIA, which was not where my interests then lay. It is not clear whether I would have won the struggle had I undertaken it.

Amid these controversies, I slowly retreated from GSIA, beginning shortly after Dick Cyert became dean, although I have worked hard (and relatively successfully) to make sure that there is room elsewhere on the campus for economists of other persuasionsin the School of Urban and Public Affairs and, subsequently, in the Department of Social and Decision Sciences within

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* The Cowles Commission had migrated from Chicago to Yale, and the Economics Department at Chicago, under the influence of Milton Friedman, had become ultra-orthodox in its adherence to the neoclassical faith, and completely intolerant of alternative religions. Bob Lucas was a product of the new Chicago School.

the College of Humanities and Social Science. Eventually, around 1970, I moved my office to the Psychology Department, but continued to participate in GSIA policy meetings and retained the position of associate dean"without portfolio," I was fond of saying. Actually, to say that I retreated from GSIA is only partly correct; I was also drawn to the Psychology Department and the burgeoning new activity around the computer by the shift in my own research interests.*

Politics on the Campus

Throughout my career, I have devoted much time to the politics of science, both inside Illinois Tech and Carnegie Mellon and at the national level. Perhaps this is a good place to explain how Carnegie Institute of Technology became Carnegie Mellon University, because this happened in 1967, between the time when Dick Cyert became dean of GSIA and when he assumed the presidency of the university. In 1967, during the presidency of Guyford Stever, a merger was arranged between CIT and the Mellon Institute, a nonprofit industrial research organization in Pittsburgh that had been endowed by Andrew Mellon. The two institutions, and the names of their major benefactors, were merged.

I do not know whose decision it was for us to become a university. The professionals at the Mellon Institute were scientists, principally chemists, who would have been quite at home in an Institute of Technology. Somehow the merger was seized on as an opportunity to proclaim a broader mission for Carnegie Mellon by dubbing it a university.

Organization theorists will be interested to know that the change in name has not been without consequence. It has supported arguments such as, "We are now a university; universities have Philosophy Departments, therefore CMU ought to have a Philosophy Department." What's in a name? A great deal, it would appear.

Campus politics and administration need to be guided by two goals: excellence and innovation. Money does not guarantee excellence. Although university salaries and faculty quality are correlated, the correlation is far from perfect. Insisting on excellenceon the university's getting what it pays for, and more if possibleat the time of critical personnel decisions (hiring, reappointment, promotion, tenure) can turn a mediocre faculty into a first-rate one.

* The defeat of bounded rationality and organization theory in GSIA was still a real blow to me. I have always liked the quote from General Stilwell, who, when driven with his troops from Burma, pushed aside excuses with, "I say we took a hell of a beating."

When making tenure decisions, members of a faculty are inclined to sacrifice quality to humaneness, particularly when close associates and friends are being judged. Acting humanely is an admirable human trait, but it is easy to misconstrue what is at issue. A faculty tenure committee is not determining how many people will be employed in the society, but *which* people will be employed in a *particular* university. Retaining a faculty member who is less able than others who could be recruited is as inhumane to the (possibly unknown) replacement as it may be humane to the incumbent. Faculty members who are denied tenure don't go on the breadline. They move to other universities or other occupations. Universities achieve high quality when they keep these facts in view.

Innovating means not simply generating ideas but disseminating them. Ideas can be disseminated by talking and writing, and the dissemination can be greatly facilitated by building institutional homes for them. At Carnegie, we have had considerable success in generating new ideas, in creating organizations to nurture them, and in propagating them through the wider educational and scientific communities. The first innovative activity I was involved in at Carnegie was founding GSIA; that organization and its worldwide influence on business education has already been described. The second was building a psychology department that has been an international leader in developing and diffusing computer simulation and information-processing psychology. The third one was introducing computers at Carnegie Tech and building there one of the world's earliest and leading computer science departments.

A fourth effort at innovation, still developing, is reconstructing design as a scientific activity and reintroducing design into the engineering curriculum. A fifth is strengthening effective education at Carnegie, by emphasizing problem solving and the blending of liberal with professional values and approaches. The institution building associated with these innovations has largely occupied the part of my life that has been devoted to university policies and politics.

This activity is not at all separate from the main stream of my research, for the Carnegie campus provided the intellectual environment where innovative ideas could be developed and then communicated to the rest of the world. Behavioral theories of economics, bounded rationality among them, gained their visibility through the joint activities of our research group in GSIA during the 1950s. The Psychology Department provided the platform for launching the cognitive revolution in psychology. A sequence of organizations, culminating in the Computer Science Department, provided the corresponding platform for artificial intelligence.

The New Cognitive Psychology

The new research on cognitive psychology that was described in chapters 13 and 14 was launched from GSIA in 1956. Within a year, Lee Gregg in the Psychology Department began to take part, but no other interest was shown by that department. Lee, seeing the promise of the new approach, moved rapidly to it from the behaviorist empiricism of traditional experimental psychology in which he had been trained at the University of Wisconsin.

GSIA had had connections with psychology, in social and organizational psychology, and Harold Guetzkow had a joint appointment in GSIA and the Psychology Department. Because I was a Fellow of the Division of Social Psychology of the American Psychological Association (on the strength of my research on organizations), I also had at least minimal legitimacy in psychology. I began to propagandize for more participation of the Psychology Department in the cognitive revolution we had started.

Some GSIA funds were used to hire young experimental psychologists whom we thought might be seduced in the new directions, but that plan was not very successful. The traditions of the discipline and concerns about a successful career in psychology were too strong to allow untenured psychology faculty to join the revolt. By the time I went to RAND on my sabbatical, in 1960, I was beginning to doubt that we could accomplish the revolution from the foreign territory of GSIA, without a firm base also in the Psychology Department. I resolved to do something about it when I returned to Pittsburgh.

As I assessed the situation in the autumn of 1961, there had been little progress, and Haller Gilmer, chairman of the Psychology Department, unpersuaded by my particular vision of the future, was unwilling to promise there would be more. I decided to use some of my brownie points with the administration to bring about a rapid change. My method was abrupt, justified in my mind by the importance I attached to the goal. The depths of my convictions on matters important to me had not gone unnoticed by my colleagues. In his autobiography, Leland Hazard, the retired general counsel of Pittsburgh Plate Glass who taught very effectively in GSIA for many years, mentions an incident that occurred in 1960:

> At the time of the School's [GSIA's] tenth anniversary we held a symposium called, "Management and the Corporation, 1980." There were a dozen participants of national and international prominence. Barbara Ward (Lady Jackson) was seated next to me and Herb Simon was across the semicircle. "He has

the face of a fanatic," Barbara Ward said to me. Before I could reply the television lights came on. [Hazard 1982, p. 29.]

Whatever my face may reveal (it isn't a poker face), I do act with coldness and calculation when important goals are at stake, even with a certain disrespect for the norms of politeness. I suppose that is as good a definition of fanaticism as any. I do not enjoy hurting people, but I do not always act to optimize human relations. When it looks like the effective thing to do, I can lose my temper, or appear to.*

In the case of psychology, I thought a great deal was at stake, and after a tense luncheon session with Haller (I did the shouting; he was calm), I wrote him the following memorandum summarizing our conversation:

November 2, 1961

Dear Haller:

I have given further thought to my course of action on the matters we discussed yesterday. I shall presently talk to President Warner, preferably after a new Dean has been found for GSIA, as follows:

1. I can fruitfully carry on my work at Carnegie only if there is on campus a strong graduate psychology program with emphasis on the area of cognition and simulation of cognitive processes. Since the local resources—financial and environmental—cannot be expected to support a very general graduate psychology program of the first quality, this implies more specialization and focus in the department than now prevails. Apart from my own personal requirements, a specialized program of this sort is the only kind that makes sense on this campus in relation to the activities of GSIA and the new program on systems and communications sciences [computer science].

2. While we have made some progress in this direction in the past five years, we have made it only because GSIA was willing to supply the financial resources, and there is little evidence that the other resources of the Psychology Department have been oriented toward this goal. I have had the feeling that nothing happened except when I pushed, a feeling further confirmed by lack of movement during the year I was away from the campus.

3. To reach the goal will require vigorous leadership in the Psychology Department from a chairman who is thoroughly sold on the objective. Because of what I perceive as a drift in the department over the past two or three years, and because of your own statements of the limits of what

* Very likely, the kind of calculated anger I sometimes exhibit is less forgivable than the spontaneous, uncontrolled kind. Many years ago, a friend said to me, "The great thing about my mother is that she never struck us except in anger." I suspect that this is the normal reaction, that loss of temper is a better excuse for aggressive behavior than is calculated severity.

you can do, I no longer have confidence that you will provide that leadership. I do not wish to continue exerting the pressure I have had to exert in the past to keep the department turned in what seems to me the only promising direction for development of its graduate work.

4. The integral relation of the behavioral science programs in Psychology and GSIA needs to be further emphasized by placing formal responsibility for the administration of the graduate programs in GSIA. The psychology graduate program cannot be satisfactorily supervised by the Dean of Graduate Studies in Engineering and Science, and the present semi-formal arrangement is too ambiguous to members of the Psychology Department.

These are not conclusions I have reached hastily, for I have examined these questions many times in the past year. I would have raised them with you earlier this fall had Lee not decided to retire from the Deanship.

Herb

The deanship of Humanities and Social Science, the division in which the Department of Psychology resided, was also vacant, but was filled at just this moment by the appointment of Jack Coleman, an economist from GSIA. Two days after I sent this memo, I also wrote to Jack, indicating my intention to appeal to Carnegie's president if needed, to bring about the changes in the Psychology Department that I thought were necessary. It closed with "very best wishes for success in your new assignment, and apologies for precipitating your first administrative crisis within the first ten minutes of your welcoming ceremony."

After I had sent these memoranda, I met with Keck Moyer and usLee Gregg, who were exercising active leadership in the Psychology Department, and reassured them that there would be ample room in the department for first-rate faculty in areas other than my brand of cognitive psychology. Believing that our goals were not in conflict, they agreed to major changes in the department.

Haller had already decided to resign,* and we persuaded Bert Green of M.I.T., who was already involved with artificial intelligence, to head the department. During Bert's five years at Carnegie, he and Al Newell secured a research grant from the National Institute of Mental Health which provided the major support for our cognitive science research during the twenty years during which it was renewed. It was a broad grant that enabled us

* Haller stayed on as a member of the department, and I was very pleased that we could become friends again not too long after these events. Perhaps his recognition that I was riding the zeitgeist took the personal edge off our encounter. After he retired from Carnegie Mellon, at around age seventy, he went on to have a very productive career for a decade at Virginia Polytechnic Institute, helping to develop their programs in industrial psychology and working with other institutions in Virginia as well.

gradually to build a cohesive group of information-processing psychologists in the department. But even with generous funding the path was not smooth, because it was not at first easy to recruit young psychologists willing and technically competent to take this new route.

In 1965, the department initiated an annual spring symposium in cognition, which continues to the present day. The symposium brought many distinguished visitors to the department, where they could see what was going on and interact with our local talent. The published proceedings also gave growing visibility to our research program, about half the papers being authored by our faculty.

Nevertheless, progress was agonizingly slow as long as our little island was still surrounded by a great national sea of almost pure behaviorismnearly the same problem that orthodox economics had posed for the behavioral theory of the firm in GSIA. But in this case, the historical trend was on our side and we gradually won out.

Progress was also slowed somewhat by the student Troubles, which I will give an account of in chapter 18. For several years they required much faculty attention, and gave aid and comfort to competing views about the proper role of psychology in the university. The situation was only fully stabilized about 1973, when Lee Gregg took over the chairmanship of the Psychology Department, which he held until his premature death in 1980.

Computer Science

Establishing a computer science program at Carnegie was much easier than introducing cognitive psychology, because we were simply filling a vacuum rather than pushing against entrenched ideas. Soon after 1956, when the IBM 650 and Alan Perlis arrived on campus, faculty and students in four departmentsGSIA, electrical engineering, mathematics, and psychologybegan to take a strong interest in computing. About 1961, a steering committee was set up with representatives of these departments, under the rubric of Systems and Communications Sciences (S&CS).

Various members of the S&CS committee were offering, in their respective departments, courses that we would now regard as computer science courses, and because we had worked hard to maintain the permeability of departmental and college boundaries at Carnegie, students from many departments took these courses. The S&CS committee next decided to construct and administer a comprehensive exam at the doctoral level in computer sciences (in S&CS). Any department that wished could incor-

porate this exam as part of their examinations for the doctorate, and all four departments represented in the committee did so.

Soon, we were awarding degrees that were essentially computer science doctorates in the four departments. The university's Committee on Graduate Studies learned of this several years after the fact, but by then it was too late to do anything but give it a blessing. In that way, we became one of the first universities in the countryin the worldto train students in computer science at the doctoral level.

By 1965, the desire was widespread to take the next stepto establish a separate Computer Science Department. It was created that year, with Alan Perlis as its first, and extremely effective, chairman. From the beginning, Carnegie Mellon, M.I.T., and Stanford were regarded as having the three leading computer science programs in the nation, a rank we continue to hold.

The Computer Science Department kept close ties with the departments that had formed the S&CS committee, and there have always been joint appointments of faculty among them. At present, four faculty members hold joint appointments in psychology and computer science. Computer science remained in the College of Science until 1987, when it became a separate college.

Engineering Design

One cannot inhabit engineering schools for several decades without acquiring views about engineering education. I formed such views very early during my tenure at Illinois Techbut probably mainly inherited them from my father. I was even moderately active in the Society for the Promotion of Engineering Education (now the American Society for Engineering Education). My initial views were that engineering education needed less vocationalism and more science.

With my experience in GSIA and a wider view of the world, I began to see things a little differently, and began to see, too, the similarities in education for various professions, especially engineering, business, and medicine. Our goal in GSIA was to balance a professional with a scientific orientation.

As I began to understand the trends in the stronger engineering schools, I saw that the same things that were happening to them were happening to the New Model business education: science was replacing professional skills in the curriculum. I looked a little further, and saw the same thing going on in medicine. More and more, business schools were becoming schools

of operations research, engineering schools were becoming schools of applied physics and math, and medical schools were becoming schools of biochemistry and molecular biology. Professional skills were disappearing from the curricula, and professionals possessing those skills were disappearing from the faculties.

The distinction between the scientific and the professional is largely a distinction between analysis and synthesis. Professionals not only analyze (understand) situations, they act on them after finding appropriate strategies (synthesis). In business, they design products and marketing channels, organize manufacturing processes, and find new financial instruments; in engineering, they design structures and devices and processes; in medicine, they design and prescribe treatments and perform operations. But analysis had driven synthesis from all these curricula.

This had happened for a good reason. Analysis is at the heart of science; it is rigorous; it can be taught. Synthesis processes are much less systematic; they are generally thought to be judgmental and intuitive, taught as "studio" subjects, at the drawing board or in clinical rounds or through unstructured business cases. They did not fit the general norms of what is properly considered academic. As a result, they were gradually squeezed out of professional schools to enhance respectability in the eyes of academic colleagues.

The discovery of artificial intelligence changed this situation radically. Artificial intelligence programs generally carry out design, or synthesis. Programs were designing electrical motors, generators, and transformers as early as 1956 and, by 1961, selecting investment portfolios. Such computer programs destroyed the mystery of intuition and synthesis, for their processes were completely open to examination. We could now understand, in whatever rigorous detail pleased us, just what a design process was. Understanding it, we could teach it, at the same level of rigor that we taught analysis.

As I gradually came to understand both the dilemma of the professional schools and the solution being offered by A.I., I began to urge that Carnegie Tech restore design and designers (or theorists of design) to its Engineering College. In the early 1960s the message fell on deaf ears. The scientists then in the Engineering College neither understood engineering nor believed it could be taught. They educated engineers by giving them a lot of physics and math, hoping that their students would later be able to design safe bridges or airplanes.

In 1968, I was invited to give the prestigious Karl Taylor Compton Lectures at M.I.T. I titled my lectures "The Sciences of the Artificial," and devoted one of them to the science of design, setting forth the view I have just sketched and filling it out with a prescription (a design!) for a curriculum

in design. The curriculum was motivated by my description, in the preceding lecture, of what our research had taught us about human thought processes, including design processes. There was no immediate seismic response to the lectures, but, in their published form, they began to attract more and more notice, in this country and abroad.

Gradually, Carnegie was able to recruit to the engineering departments a few faculty members who shared this view of design. Gary Powers and Steve Director were among the first. They came together in a Design Research Center, whose activities have burgeoned into a large network of research studies on synthesis processes of many kinds.

The research, in turn, is beginning to reflect back on curriculum, so that Carnegie Mellon is today a recognized leader in restoring professional skillsdesign skillsto engineering education. Of course, we are not bringing back the drawing board. We are teaching not just an art of design but a science of design. The main vehicle is the study of expert systems and other artificial intelligence systems that do design, thereby revealing its anatomy and physiology.

These developments have afforded me great satisfaction, particularly because, aside from providing the initial propaganda for them, I have not had to be very actively involved in bringing them about. They are now firmly rooted in the soil of the Engineering College and are proceeding under their own momentum. If one must be a reformer, that's the best kind of reform.

New Presidents for Old

John Christian (Jake) Warner, who had assumed the presidency of Carnegie Tech just after my arrival there, retired from office in 1965. There was little faculty participation in the choice of his successor, and I recall only one meeting on the subject at which I was present. The new president was Guyford Stever, who left in 1972 to become director of the National Science Foundation.

In the 1972 presidential search, coming just a few years after the Student Troubles and the concessions to faculty and student democracy that they had brought about, there was much more active faculty participation, and even the students were brought into the process to a limited extent.

A few months before the 1972 search began, I had been invited by Stanford University to join its Board of Trustees. At that time many boards were co-opting an academic member or two from other campuses. I was both flattered and tempted by the invitation, but finally decided that if I was going to spend substantial time thinking about issues of educational policy

in universities, I would prefer doing it for Carnegie, where I might have some influence, rather than for one of our leading competitors.

I was an obvious possibility for the Carnegie presidency. When I told the chairman of the trustee search committee, after a week or so of deliberation, that I would not be a candidate, he invited me to join his committee, which I did. I also told him I had declined a board membership at another university, but that I would not reject an invitation to join the Carnegie Board. After he had consulted his board colleagues, his response was positive, but it was agreed that nothing would be done until a new president had been selected. The announcement of my appointment to the search committee specified that I was serving as an individual, not as a faculty representative.

Dick Cyert was the other obvious inside candidate for the presidency, and by his skillful and assiduous campaigning, he soon gained rather solid faculty support. Although some of the science faculty thought that the president should be a natural scientist, Dick was able to allay their worries. The trustees' committee was also inclined to look for a scientist or an engineer from outside, but in the end, the faculty committee won over the trustees to their preference for Dick.

I was not soon persuaded to support Dick, because, as I have already said, I was unhappy with the way GSIA was going, and blamed at least part of the problem on Dick's policies. Finally, I concurred with the others; there were no spectacularly good alternatives. It was no secret to Dick that I had been almost the last to climb on his bandwagon, but he harbored no visible resentment. But I thought it would be unfair for me to accept the board membership that had been promised if he were opposed to, or even mildly uncomfortable with, the idea. On the contrary, he responded positively. The invitation was extended, and I accepted.

It was of course anomalous for me to be simultaneously a tenured faculty member of the university and a member of its Board of Trusteesand not as a representative of the faculty. If it made anyone uncomfortable, I never knew about it. I have always tried to remember, at any given moment, which hat I was wearing, and not to wear them both at once. For a number of years I avoided committees of the board that dealt with internal academic affairs, devoting most of my effort to the Finance Committee and its subcommittee on investments. During these years, the university changed completely its way of handling its endowment, gradually entrusting it to a small set of money managers. An enormous amount of time went into fashioning the new arrangements and selecting the managers. Later, I served also on the Audit Committee.

My membership on the board was useful in two other ways. First, it enabled me, from time to time, to interpret the university to fellow trustees.

Few members of the Carnegie Board were very close to the university, and the knowledge that many of them had of it was based largely on memories of undergraduate student days (at Carnegie or elsewhere). On appropriate occasions, I could remind them of other important aspects of the university's operations. I could even remind them that one-third to one-half of the university's revenues were raised by the entrepreneurial activities of the facultyfar more than was coming in from gifts and endowment income. They needed to have a realistic understanding of what a research university is like, for that was more and more what Carnegie was becoming.

Second, my board membership enabled me to maintain an open relation with Dick Cyert. We fell into the custom of meeting periodically at breakfast, our conversations roaming over the whole range of university affairs. This relation was tenable only if I could avoid strong advocacy of my own hobbies and the university activities with which I was most closely associated. I did not want to become an influence broker. As long as I looked at the whole of Carnegie first, and its parts second, I could be useful. I think I have usually been able to do this, but I would be surprised if there have not been some lapses.

All of this was possible because Dick Cyert was a strong president. No one could imagine that he was an easy target of persuasion, nor did he and I always agree on policies. So we remained close friends over the eighteen years of his presidency. And there have been no doubts on campus about who was in charge.

Since a major reason I did not seek the presidency myself was to reserve time for my research, I involved myself only selectively in university administrative affairs, which have accounted for only a small part of my work week. The university went along very well under Dick's direction without my intervention. Dick did not feel obliged to keep me especially informedany more than any other trusteeabout what was going on, except on matters he wanted to discuss with me.

On the other hand, I am not so naïve as to suggest that my faculty colleagues, much less the deans, were unaware of my dual role. I am sure that I was often treated more tenderly than I otherwise would have been, that I was kept better informed, that my views and agreement on proposed policies were sought. Sometimes I was used as a channel to bring problems to Dick's attention. I recognized that I had more clout than I would have had without board membership, but I tried to use it responsibly and without adding more confusion to the organization structure than was already there. (Carnegie has never had a neat organization chart, and Dick has never restricted his contacts to "channels.")

Finally, it is impossible to unconfound the influence I enjoyed by virtue

of being a trustee from the growing influence I derived from my national and international scientific reputation. There are many ways in which sacred cows become sacred.

Why I Am Not a College President

In 1961 I was the obvious successor to Lee Bach in the deanship of GSIA, and from there the presidency would not have been a very long step. I made two specific career choices in favor of research over administration. These were among the half dozen most important occasions in my life when I had to opt for the left or the right path of the branching maze. I declined to be considered for the deanship of GSIA when Lee resigned (I would nearly certainly have been appointed); and I declined to be considered for the university presidency when Guy Stever resigned eleven years later.

Although I now imagine that I always wanted a research career, the documentary evidence does not bear this memory out. While I was a high school student, I thought I had a serious interest in the law (fed by Uncle Harold and my debating experience?). I took a vocational interest test, and when I scored nearly off the scale on introversion, the counselor allowed as how law might not be my vocation. Whether the test judged me correctly is an interesting question. I do find it difficult to take the initiative in cultivating other people.

The vocational interest test by no means decided matters. As late as 1942, I weighed the prospects of a career in the civil service, and even had thoughts of a political career. The latter I ruled out because (1) I was not a veteran, and (2) I was Jewish. Even though, as I explained earlier, I sometimes enjoy an underdog role, those two strikes against me in politics were too much. No attractive civil service opportunity presented itself before I was caught up in my academic career.

Why did I, after many years of administrative responsibility (directing the measurement research project at Berkeley, chairing departments at Illinois Tech and Carnegie Tech, and serving as associate dean of GSIA) decide not to stand for the deanship? A year as acting dean, while not quite the same thing, convinced me that it was not what I wanted. It was too disciplined a life; there would be no opportunity to pursue intriguing ideas that presented themselves; I felt a distaste for getting my satisfactions mainly from stimulating the contributions of others and needing to cultivate people to get their cooperation or their money, and especially to initiate such contacts. Perhaps the vocational interest test had been right. After weighing the possibility seriously, I decided I did not want to be a candidate.

The decision on the presidency was easier, both because success was less certain and because I could simply re-evoke the feelings of the previous decision. Success was less certain because Dick Cyert then had ten years of deanly experience that I had rejected, and because my sharp tongue and fierce infighting in behalf of cognitive psychology and A.I. had made me less than beloved by parts of the faculty.

However that may be, I did not seriously consider taking on the contest. I equivocated only long enough to assure myself a strong position in the university's decision processes. I have never regretted the decision, especially in view of Dick's stellar performance on the job, a performance made possible by a "deviousness" that our colleague Leland Hazard admiringly attributed to him, and that I surely did not possess.

Perhaps I had cast the die even earlier. When I was first listed in *Who's Who,* at about the time I came to Pittsburgh, I made sure that my political affiliation (Democratic) and my religion (Unitarian) were placed in the public record. In moving from public administration to business administration, I did not want to be tempted to compromise my liberalism. That overt inflexibility is perhaps not wholly conducive to success in an administrative position that must mediate among half a dozen constituencies, including conservative business ones.

In fact, the close association with the business community that is essential for effective performance as president of a university such as Carnegie Mellon would have been uncomfortable for me. I find it nearly as easy to associate with businesspeople as with academicians, although, since I am not good at small talk and lack an interest in golf or sports, conversation sometimes languishes. But when the talk turns to current affairs and politics, I cannot conceal my liberal views, different from the views held by most businesspeople.

Perhaps the most serious problem I have in hobnobbing with the rich, is that, however attractive their other qualities, intellectual and personal, they are (in my experience) nearly uniformly humorless about money. They believe that it is very important, and they usually behave (I don't know what goes on inside) as if they possessed it by right and not by the grace of God or fortune. Somehow, Dorothea and I lack a proper respect for wealth, even our own. We both think the income tax is too low.

I once phoned a very wealthy man, with whom I was on warm first-name terms, to ask him to donate a company product worth $400 to scientists in a Third World country. Without a moment's hesitation he replied, "I'll split it with you." I regard this man, whom I like very much, as intelligent, interesting, and possessed of enlightened social viewsnot as liberal as mine, but far from reactionary. What struck me about his response was its

automaticitya knee-jerk reaction. Even trifling amounts of money were not to be disbursed casually.

In view of my attitudes, should I be embarrassed that GSIA, the school that housed my most important research and educational contributions, was founded by William Larimer Mellon, who built the Gulf Oil Company; that it resides in a university created by the multimillionaire Andrew Carnegie; and that the chair I have held for a quarter of a century was endowed by the very wealthy banker Richard King Mellon? Not at all. Giving money away is often the best thing you can do with it, and I do not object to being its beneficiary for good causes.

Liberal-professional Education

My adventures in teaching at Illinois Tech illustrate my experimental attitude toward instruction (see chapter 7). I have never confused teaching with delivering orderly lectures (to be assembled into a textbook) "covering" the subject matter. Nor did that confusion exist in the minds of the teachers that President Robert Doherty, Jake Warner's predecessor, had assembled at Carnegie Tech beginning in 1936. Carnegie had pioneered in two important movements in engineering education: providing a substantial liberal arts component within the engineering curriculum, and shifting emphasis from teaching subject matter to teaching problem-solving skills.

Carnegie Tech was one of the engineering schools that led the way toward allocating about one-quarter of the undergraduate curriculum to nonengineering, nonscience subjects. It also undertook some pioneering steps to make that quarter of the curriculum something more than a hodgepodge of electives.

Carnegie Tech, under President Doherty, also introduced the Carnegie Plan, a statement of its fundamental educational objectives, implemented by courses specifically designed with those objectives in view. A succinct statement of the goals of the Carnegie Plan can be found in Doherty's paper "Education for Professional Responsibility," from which I quote:

> Three changes in professional education are needed. First, a new philosophy and new outlook which will comprehend the human and social as well as the technical. Second, the development in all professional men of genuine competence in the professional way of thoughta way of thought which embodies an analytical and creative power that is as effective in the human and social realm as that developed in engineering. . . .

> Third, the development of the ability to learn from experience so that in the

unfolding future they can continue to expand their fundamental knowledge, deepen their understanding, and improve their power as professional men and women and as leading citizens. [Doherty 1948, pp. 76 77]

This can be read as pious sentiment. What made it more was Doherty's rethinking of the curriculum and teaching methods to subordinate subject matter coverage to instruction in problem-solving skills. In these efforts, he was aided by his provost, Elliott Dunlap Smith, by Dick Teare, who became engineering dean, and by many others.

Doherty retired in 1950, Smith in 1958, and their influence on the university has gradually been dilutedbut not wholly forgotten by those of us who were young faculty members during those years: Erwin Steinberg in English, Ted Fenton in History, and Dick Cyert among them. For that reason, faculty sophistication about educational philosophy and practice remains higher at Carnegie Mellon than at most other universities with which I am acquainted.

When I came to Carnegie, the school did not offer degrees in the social sciences and humanities (with the partial exception of Margaret Morrison Carnegie College for Women). History, English, languages, and psychology were "service" departments, and their faculty members, somewhat second-class citizens. During the Stever administration (1965 72), Margaret Morrison College was combined with the College of Humanities and Social Science, we renamed ourselves a university (upon merging with the Mellon Institute of Research), and we began offering undergraduate degrees in several liberal arts subjects.

I had mixed feelings about these changes, because I had mixed (read negative) feelings about the viability of contemporary liberal arts education, especially in the humanities. Particularly, I was not impressed by the demeaning attitude of these fields toward "vocationalism." They sometimes seemed to propose uselessness as an essential criterion for proper liberal studies, all the explicit emphasis being on knowledge, not skill.

Of course, practice is another matter. If there is any place in a university where skill is the name of the game, the language departments could perhaps claim it. But language teachers prefer to imagine that learning to read, write, understand, and speak is just an unfortunately necessary preliminary to immersion in literature, history, and culture. In actual fact they spend almost all their time teaching these preliminaries, but that is just one of life's misfortunes. (English teachers have generally the same problems rationalizing their preoccupation with grammar and spelling.)

As far as Carnegie Mellon was concerned, I thought that the basic philosophy of the Carnegie Plan might be transported into the new social science

and humanities curricula under the banner "liberal-professional education," buttressed by some thoughts about what that phrase might mean in practice. We could have no comparative advantage in the liberal subjects in competition with the Ivy League schools unless we offered something different and arguably better than they did. If we had no comparative advantage, we would not achieve quality; and if we did not achieve quality, we should not be in the business.

But to extend the application of the Carnegie Plan to the liberal arts, people had to be convinced that there was no conflict between "liberal," properly interpreted, and "professional" properly interpreted. Liberally educated people are skilled people; and the skills of well-educated professional people are infused with liberal values and knowledge. Those of us who were infected with the Carnegie Plan have had considerable, if far from complete, success in promoting the idea of liberal-professional education on our campus, especially in the College of Humanities and Social Science.

The whole story is complex and has not yet reached its denouement. Its essence can be conveyed in the following passages from a talk I gave to the faculty in 1977, which created quite a stir. At that time, Dean Pat Crecine, of the College of Humanities and Social Science, and his Associate Dean, Lee Gregg, were just instituting a core curriculum in the College, with less than 100 percent support of the facultyalthough many had been won over. On April 5, I gave a well-attended lecture on the subject of liberal education, aimed at provoking serious discussion of important educational issues on the campus. I think I succeeded. I began with a definition of *liberal education* that challenged its frequent disparagement of skill and the tension between "liberal" and "professional":

> There is remarkable agreement that liberal education, both etymologically and in every other way, means education for a free man, a free person. Disagreement only begins when you ask what kind of education a person needs in order to become and remain free.

> . . . Its charter describes Yale, for example, as a school "wherein youth may be instructed in the arts and sciences who through the blessing of Almighty God may be fitted for public employment both in Church and civil state." . . . [From] classical times, to prepare someone to be a free person was to prepare him or her to take a station in society. If that station involved performing as a citizen then it was preparation for citizenship. If that station involved productive work then the free person's education included training for appropriate employment. [Simon 1977a, pg. 1]

Next I presented an illustrative examination (ten questions) as an operational definition of liberal education, and provided alternative answers to

one of the questions. "Notice," I said, that we're not simply examining knowledgewhether you have read your Homer and your Virgil. We're examining skills. To answer the question you have to apply the skills of poetry writing, or the skills of computer programming, or some other skills."

I suggested that we must draw on the social sciences to design a proper education: first, to understand the motivations of our students; second, to "capture a large part of the out-of-class time for the educational processperhaps even insinuate in the dinner conversation some things that are relevant to becoming an educated person." To do this, the school must appeal differently to students whose motivations are intellectual, to those whose motives are professional, to those whose motives are social, to those who are conventional, and to those "not elsewhere classified." "The third thing we need from the social sciences . . . is an empirically based theory of knowledge and skills and their mutual relation."

How can we motivate our students to acquire a liberal education? I argued for a core curriculum, to provide common topics of conversation outside the classroom. Coverage was not importantwas, in fact, impossible. The core must be constructed by *sampling*. It must aim at developing skills as well as knowledge: problem-solving skills, skills of cross-examining experts, and skills of perceiving and appreciating.

Then came the punch line: "A faculty that is not liberally educated can't provide liberal education. American college faculties, including our own, aren't liberally educated. . . . How many of the licensees in your own field, if you walk up to them with a question, will say, 'Oh, I can't answer that; that isn't my period'?" I concluded by proposing that the university establish a program of liberal education for the faculty, requiring every CMU faculty member to pass the comprehensive examinations on the common core of that program within the next four years. This, I said, was the way to prepare ourselves to provide liberal-professional education to our students.

As a consequence of my talk, my history colleagues invited me to join a team that was teaching the freshman core course in their subject. Of course I had to accept the invitation (and was quite delighted to do so). The course focused on the French Revolution (*examples,* not *coverage,* was the watchword), and included an exercise requiring students to test hypotheses they found in standard works on the Revolution against computerized files (in English) of the *Cahiers de Doléances* submitted to Versailles by the French provincial assemblies in 1787. Did the *Cahiers* support the claims of the books or didn't they? I thus spent an instructive semester teaching and learning historymore learning than teaching. I have not repeated the experience, but mainly from laziness and not because it would not be fun

to do itor something similar in English literature or the French novelagain.

My proposed school for teachers has not yet been established at Carnegie Mellon. But I am patient, and realize that social reform cannot be accomplished in an instant.

In looking back on my talk, I find myself speculating on the origins of my educational views. The strong belief in liberal-professional education undoubtedly derives from my experiences in engineering schools, both Illinois Tech and Carnegie Tech, combined with a view that the humanities, in their traditional forms, have had an exaggerated role in liberal education. I have never reconciled myself with the view that professional education need be narrowly vocational, or that *skill* is a dirty word. Nor do I believe that the contemporary humanities have demonstrated a special competence to interpret the human condition.

The importance I attach to the university as a social system and to a core curriculum for enriching the educational experience undoubtedly stems from my experiences in the College at the University of Chicago, with its enlightening survey courses addressing every domain of knowledge. Since this was not yet the Chicago of Hutchins's and Adler's Great Books, I did not become committed to a specific canon. Such a commitment would make the idea of sampling unacceptable, hence would make a core unworkable.

And although it peeks through only a little into this talk, my view that the social sciences (and especially cognitive science) have an essential contribution to make to university education stems directly from my research in recent years on learning and problem-solving processes. Contemporary cognitive science provides knowledge that is vital for the improvement of educational processes. It also reveals the commonality of human thought processes across the most diverse fields, giving us reason to believe that effective communication can be established and maintained among the many specialized cultures that make up professional, intellectual, and artistic society today.

Chapter 17
On Being Argumentative

My account of my life in the university, both in GSIA and afterward, makes clear that I have not avoided controversyindeed, have often been embroiled in it. I like to think that it was not a love of battle that was responsible for this, but that confrontation was necessary to achieve some of the goals I thought important for the university.

If controversy occurs in the life of the university, it also occurs in research. A good deal of my research has been directed at unseating established positions, first in public administration, next in economics, and then in psychology. In one sense, that is to be expected. Research is supposed to produce something new, and new is different from old. Nevertheless, much research, including very important discoveries, some of them revolutionary in the Kuhnian sense, does not contradict the old, but builds upon it. Even if it ultimately undermines the old order, its subversive consequences are not always obvious at the outset.

I have usually announced my revolutionary intent. The published "essential portion" of my dissertation was sedate enough, "Decision Making and Administrative Organization," published in 1944 in the *Public Administration Review*. But it was followed in the same journal in 1946 by "The Proverbs of Administration," which asserted that the basic principles of the classical theory of administration (those of Luther Gulick and L. Urwick 1937) were not principles at all, but proverbs, full of wisdom but always occurring in mutually contradicting pairs.

The message was mostly criticalI showed that classical theory was in bad shape but proposed only very general remedies: "We need more research to establish when and under what circumstances which proverb is valid." It was only in the mid-1950s that I had an opportunity to begin

validating empirically the alternative theory I sketched in *Administrative Behavior*.

The "Proverbs" article got plenty of attention, not all of it favorable. Urwick never quite forgave me this attack on his life work, but Gulick was quite friendly in later years. Presumably he made allowance for the hubris of a young man. Hubris, arrogance, or whatever, that article secured my instant and permanent visibility in public administration. It is still frequently cited.

In chapter 4, I described the behavioral movement in political science, spearheaded by Charles Merriam's department at Chicago and the volume edited by Herbert Storing, *Essays on the Scientific Study of Politics,* that devoted individual chapters to flaying the chief behaviorists, including me.

Replying to this attack would have itself required a book almost as long as *Administrative Behavior,* one that I was never tempted to write. It seemed to me that *Administrative Behavior* was its own best defenseand my judgment seems to have been vindicated, as I do not see that its reputation has suffered over the years.

It is true that I am still accused of "positivism" as though that were some kind of felony, or at least a venial sin; and there still seems to be widespread lack of understanding of why one cannot logically deduce an "ought" without including at least one "ought" among the premises. But I think these difficulties have little or no connection with the Storing book. They arise from the general tendency today to use positivist as a pejorative term without any clear notion of what *positivist* as believe.

In economics, combat was slower in getting under way. My initial forays into economics consisted of a few articles (on tax incidence and technological change) that kept well within the neoclassical framework, and then some papers suggesting the need to recognize the limits on rationality in order to create a more lifelike picture of the business firm. In none of these papers did I challenge the foundations of economic theory strongly, or its macroeconomic applications, although the raw materials for such a challenge were certainly provided in *Administrative Behavior* and my subsequent pieces on the firm.

The first hostilities took the form of counterattacks from the opponents of bounded rationality: from Edward S. Mason (1952) and Fritz Machlup (1946), the former claiming that my revisions of the theory of the firm were not very relevant to economic theory, the latter that people, whatever the appearances, really maximized. But the blame for war is not easily assignedhere or elsewhere. Certainly my colleagues in economics at Carnegie soon knew of my skepticism. Franco Modigliani, while remaining a

close friend during his Pittsburgh years and ever since, never mistook me for an ally in matters of economic theory. And Jack Muth, in his announcement of rational expectations in 1961, explicitly labeled his theory a reply to my doctrine of bounded rationality. Lunchtime debate with my colleagues and disputes about personnel decisions in the economics faculty undoubtedly contributed to the gradual escalation of my conflict with the profession. By the time I returned to a concern with economics in the 1970s, the war was open and declared.

Here again the task was to go beyond skepticism about the foundations of neoclassical economics and to provide an alternative. You can't beat something with nothing. *Administrative Behavior* was a beginning, which I followed up in the early 1950s with papers on organizational equilibrium, the theory of the employment relation, and a behavioral model of rational choice. All three papers met economic theory on its own ground, and all three were published in mainstream economics journals.

Mason's objection, echoed by others in the profession, that the revision was simply irrelevant to the main concern of economics with industries and the economy as a whole, induced me to broaden the attack to challenge the evidence supporting neoclassical macroeconomics. This shift is visible in my economics papers written after the mid-1970s. Here I developed the distinction between procedural and substantive theories of rationality; insisted on the need for a computational theory of economic decision making; challenged the evidence usually cited for believing that demand and supply are equilibrated at the margin in real markets; and showed that auxiliary assumptions, quite independent of the central assumptions of optimization, accounted for the ability of economists to explain real-world phenomena. These papers are overtly combative. Perhaps my stridency will abate as economics embraces more of the heresy (as it appears now to be doing).

My work in cognitive psychology with Al Newell and Cliff Shaw, starting with the Logic Theorist in 1956, accented the positive. We had specific, concrete proposals for both methodology (computer simulation and thinking-aloud protocols) and substance (problem solving using heuristic search conducted by a physical symbol system). Of course, these proposals flew in the face of the predominant behaviorism, especially in its strong Skinnerian form; but we set forth our theory and the evidence for it without much overt challenge to the prevailing religion. In fact, in our *Psychological Review* paper (Newell, Shaw, and Simon 1958a), we claimed explicitly to be natural descendants of both the behaviorists and the Gestaltists, and to provide a reconciliation of these contending schools.

Disagreement does not have to be announced in order to be perceived.

Psychologists, as soon as they stopped ignoring us (and we were not ignored for very long), recognized the revolutionary import of what we were saying. But the dispute in psychology proceeded very differently from those in public administration and economics, due, no doubt, to the stronger empirical orientation of the first of these fields. To be sure, it has its share of philosophical pronunciamentos and methodological discourses, but these have to take place within bounds defined by the steady accumulation of empirical findings. However it turns out in the long run, and the empirical evidence will decide that, the revolution achieved a great success in the 1970s.

Neither Allen Newell nor I has devoted much time or energy to explicit replies to the criticsbehaviorist, Gestalt, or phenomenological. We have adopted the policy (was it the anarchist Bakunin's? or Sorel's?) of "propaganda of the deed, not propaganda of the word." The best rhetoric comes from building and testing models and running experiments. Let philosophers weave webs of words; such webs break easily.

A number of the most important psychological models that I have constructed were conceived while I thought through the implications of critical attacks on the information-processing paradigm. For example, when Ulric Neisser asserted, in a paper he published in 1963, the impossibility of computers responding to emotions or entertaining multiple goals, I took the challenge seriously and was able to produce the model of motivational and emotional controls of cognition that is contained in my 1967 paper bearing that title.

Similarly, the work that Barenfeld and I did on chess perception, published in 1969, was aimed at refuting the claims of Tichomirov and Poznyanskaya (1965) that a computer scanning a chess board was incapable of grasping chess relations as Gestalts, as these masters could. And a major motivation for my continuing interest in models of scientific discovery has been to show that, contrary to the claims of phenomenologists, programs could be designed to discover laws and invent new concepts. Constructive proofs like these were far more effective in answering criticism than rhetoric, however eloquent, would have been.

Mention of computer simulation and philosophers raises the most contentious issue of all: whether one should speak of computers as thinking. Cognitive psychologists, even those sympathetic to simulation, often avoid the issue by talking of "the computer metaphor." No one would deny metaphorical thoughts to computers. But the question, "Can computers *really* (not just metaphorically) think?" raises the warmest passions in philosophers, and sometimes in the common person in the street as well.

There is a knock-down argument that is supposed to settle the question

instantly. It goes like this: Computers are machines; machines cannot think; hence computers cannot think. It is not regarded as an adequate riposte to say: Human beings are also (biological) machines; therefore, if machines cannot think, human beings cannot think.

Argumentation, unlike formal logic, appeals to premises that are not always made explicit but are presumed to be already stored in the memory of the reader, evoking these beliefs and drawing inferences from them. That is the structure of the argument I have just given. Most readers already "know" that machines cannot think and that computers are machines. Those premises don't require evidence.

In fact, most readers will accept the statement, "Computers can't think," without any explicit argument at all, because the requisite premises will be evoked from memory as soon as the conclusion is stated. On the other hand, the riposte fails because many readers do not believe that human beings are machines; or, if they believe it, do so with substantial qualifications and restrictions.

In a debate between two protagonists, that party has a decided advantage whose case rests on beliefs solidly established in the minds of most readers. In arguing that machines think, we are in the same fix as Darwin when he argued that man shares common ancestors with monkeys, or Galileo when he argued that the Earth spins on its axis. Wearied by the prospect of having to establish our nonintuitive premises as well as our conclusions, Al Newell and I have usually let a steadily accumulating collection of programs (our own and others') demonstrate the thinking of computers. The computers, speaking for themselves (figuratively and literally), will in time convince all but the most hardened fundamentalists that they think.

In amplification of these views, I reproduce here a letter I wrote to my daughter Barbara well over a decade ago.

> May 21, 1977
>
> Dear Bar:
>
> You ask about Weizenbaum and all that. It's a long and complicated story, on at least three different levels. First, there is the whole set of questions about what artificial intelligence has achieved and will achieve by way of imitating human thought and other human processes. In principle at least, that is an empirical question that ought to be answered dispassionately after looking at the facts.
>
> Second, there are the questions about what the accomplishments of artificial intelligence, however great or meager, mean for human society and people generally. Those are also empirical questions, but their answer depends partly on the answers to the first set of questions.

Third, there are the questions about how people feel about intelligent machines and their relations to people. These are questions of emotion and value that are not susceptible to demonstration and proof.*

Now it would be nice if we could settle the first and second sets of questions, the factual ones, more or less independently of the third, the emotional ones. But it has not worked out that way. From the very beginning of A.I., back in the 1950s, it has generated in the hearts of some people strong fears, anxiety, and even anger. In this respect, A.I. has had much the same kind of effect as Darwin's announcement of the Theory of Evolution. Both aroused in some people anxieties about their own uniqueness, value, and worth. Thus, very early on, an engineer named Mortimer Taube (I think) wrote an angry letter to *Science* about the first GPS piece that Al and I published there, and an even angrier book. He was followed by Richard Bellman . . . Bellman was followed by the humanistic philosopher Hubert Dreyfus (a brother of Bellman's research associate), and Dreyfus by Weizenbaum. There have, of course, been many others, but those are the ones who have gotten most attention.

In general, I have not answered these attacks. . . . You don't get very far arguing with a man about his religion, and these are essentially religious issues to the Dreyfuses and Weizenbaums of the world. Weizenbaum's book, for example, vacillates among the positions that (1) the claims for A.I. are much overstated, (2) there is a danger that these claims will be realized, (3) it is immoral for people to try to realize them (he sometimes uses the word "obscene," and compares such people with the Nazis). Now I understand some of the reasons why Joe is upset (he was himself a refugee from the Nazis), but not why he has fixated on computers as the objects of all of his anxieties. In any event, I see no point in arguing with him.

My own posture has been this: my scientific work, and that of other A.I. researchers, will, in the long run, determine how many of Man's thinking processes can be simulated. I believe that ultimately all of them can, but feel no great urge to try to prove that to others who feel differently. In science, it is the facts that give us the final answers.

With respect to social consequences, I believe that every researcher has some responsibility to assess, and try to inform others of, the possible social consequences of the research products he is trying to create. Al and I tried to discharge that responsibility [in 1958] in our OR paper with its predictions, and I have also in the three editions of my *New Science of Management Decision* and *Sciences of the Artificial*. Of course I cannot expect that others will always or often agree with my predictions. What I sometimes lose my cool about, however, is to be attacked for making them. . . . However, while I regret such attacks, I am not surprised by them. Cool is . . . seldom pre-

* More precisely, the nature of the feelings is fact, but its justification involves emotions and values.

served in scientific controversies, and I should not expect it to be in this case, where the nerves that are being touched are so sensitive. . . .

Just a couple of final comments. First, I don't think we have to assert any particular uniqueness of man, or separateness from the rest of nature, in order to find value in life. Personally, I find it more agreeable to think that man is a part of nature than that he is apart from and above it, but others may have different tastes in this matter. Second, I think those who object to my characterizing man as simple want somehow to retain a deep mystery at his corea denial, again, of his integral relation with the rest of nature. For my money, to show that something whose behavior looks very complex and erratic is really built from the combinatorics of very simple components is beautiful, not demeaning. It would seem to me that every scientist would have to think so, for the whole purpose of science is to find meaningful simplicity in the midst of disorderly complexity.

But enough of philosophy for now. I hope these comments will give you some idea of my reaction to the critics.

Love,
Dad

I have given here only a fragmentary picture of the scientific disputes in which I have been engaged. In addition to those of revolutionary scope, there have been others of more restricted range. In spite of my protestations on earlier pages, on a number of occasions I have responded to specific attacks, or have myself attacked the work of others. I won't attempt a complete catalog of instances, but will mention some of the more salient examples.

In 1955 I published in *Biometrika* my initial paper on skew distributions. After I had drafted the paper, my attention was called to related work by D. G. Champernowne, G. Udny Yule, and Benoit Mandelbrot (more recently of fractal fame), which I duly acknowledged in the published article. I had also had direct discussions and correspondence with Mandelbrot about the paper.

In 1960 I discovered that Mandelbrot had attacked my paper the previous year in *Information and Control*. Neither author nor editor had informed me or given me an opportunity to reply. I wrote to Mandelbrot, suggesting that we might write a joint paper explicating our points of agreement and difference, but not aiming at a verdict. He agreed, and we exchanged some drafts. Since it quickly became clear that we were escalating, not converging, we gave up the idea of a joint article, and I submitted my own reply for publication.

Mandelbrot then wrote a new reply, to which I replied, to which he replied, to which I wrote a final reply. The long-suffering editor agreed to

this procedure after I pointed out that the process would converge rapidly if the length of each reply was constrained to a fixed proper fraction of the length of the previous one.* That worked, although the reader who reviews the battle will note that my fractions were always smaller than Mandelbrot's. The exchange was carried on by both parties in rather purple prose. If I have ever had second thoughts about the vividness of my rhetoric in those replies, I have had none about the basic correctness of my position.

Why not let the reader judge that too, instead of asserting it? Because it would take many hours of reading and thought to understand Mandelbrot's and my arguments fully and to adjudicate between them—probably as much time as each of us spent in constructing them. Few readers are willing to pay this cost. The most one can achieve in such an exchange is to persuade readers that there is something to be said on both sides—and both disputants will receive about the same credit. Is this a reason for engaging in debates or for avoiding them?

A few years later, in the British journal *Applied Statistics,* I discovered another attack on my *Biometrika* paper, this time by a linguist named G. Herdan, who claimed that the model I was proposing for word frequency distributions did not even remotely fit the data. On examination, his argument proved to rest on fallacious statistical procedures. I sent off my reply to the editor, accompanied, unfortunately, by a letter suggesting that Herdan's paper could not possibly have been recommended by competent referees.

The editor, in an angry response, said that Herdan's paper had been recommended for publication by two distinguished statisticians. He then added a page of his own criticisms of my paper. I wrote a lengthy (and chastened) reply, apologizing for my insinuations about the editorial process, but insisting that I should have an opportunity to reply to Herdan. I never received an answer to this letter. I recently recovered this correspondence from my files. My reply to Herdan still seems to me correct, and his paper as full of large holes as ever. Perhaps *Applied Statistics* now has a new editor and I should try again.

These are not the only occasions when my prose has roused the ire of a mathematician. Two other examples produced rhetoric as vivid as the exchange with Mandelbrot. The first involved a Norwegian mathematician named Karl Aubert (1982); the second, an American named Neal Koblitz (1988). What astonished me about all three exchanges were the violence

* The idea was not original. E. G. Boring had adopted this policy for handling interchanges when he became the first editor of *Contemporary Psychology,* the American Psychological Association's review journal.

and ad hominem character of the language used in the initial attacks. (I should add that I entered into the spirit of the fray in my replies, but did not set the tone.) With both Aubert and Koblitz, I think I won the argumentit was a clear knockout in both instancesbut I have no illusions, any more than in the exchange with Mandelbrot, that readers will study the exchange closely enough to be able to make up their minds about it.

I write of these incidents with more levity than I felt while they were going on. It is embarrassing to be caught out in a significant mathematical error in a published paper. The logician Richard Montague nailed me in one a few years ago. I acknowledged the error and then went on, in a later publication, to say what I should have said in the first place. The exchange left no permanent scars on my psyche.

It is far more annoying to be ridiculed for committing an alleged mistake that in fact was not one. When the attack comes, you of course don't know whether or not you are guilty, and you feel not a little anxious as you reexamine each step in the argument. With each further exchange, the same process must be repeated. But then, after you have convinced yourself that you stand on solid ground, you find that, for reasons already set forth, the ground is not very solid at all. As long as your opponent holds tight to his position, the rest of the world will never know who was correct. It is like a criminal case in the courts. Whether the verdict be innocent or guilty, there will always be room for doubt.

In one respect, the journal dispute is even worse than the criminal case, because at the end there is no verdict of innocent or guilty. The spectators are left to find their own verdicts as best they can. And since where there's smoke there's fire, the innocent are never fully acquitted.

In recent years, sociologists and historians of science have shown us that the world of science is highly competitive and, as a consequence, often combative. Each scientist wants to discover the correct theory, and wants to discover it first. There are many vigorous disputes about who is correct and who was first. As we know, even the great Newton was prone to such quarreling, in his contentions with Leibniz and Robert Hooke among others.

As my partial account of my own combat shows, my strategy was not consistent. On broad issues, like behavioral versus traditional political science, bounded rationality versus optimization, cognitive psychology versus behaviorism, and machine thinking, I have seldom replied directly to critics, preferring to make my case on my own terms, to define the issues myself rather than to debate within the framework defined by opponents.

I am not sure that I was aware of the rationale for my strategy, but as I look back on it, it seems to have worked pretty wellboth because it allowed me to devote most of my effort to developing the empirical evidence

for my theories, and because it allowed me to focus attention on the "right" question. On broad intellectual and philosophical fronts, it is important to maintain the intiative in framing the issues. Attack is more effective than defense.

When specific publications of mine have been criticized as technically flawed, however, I have often responded. In these cases the issues are relatively clear, and, although I have just expressed my doubts about whether the outcome of this kind of debate is ever conclusive, I have never felt quite comfortable in leaving wrong mathematics unchallenged.

My life in science, like the lives of all scientists, has had its share of verbal warfare. It is not the part of science that I enjoy most, but it is evident that I have not shirked my soldierly duties.

Chapter 18
The Student Troubles

Somehow, I began calling the student unrest on our campuses in the 1960s and 1970s "the Troubles" and now always think of them under that label. Since the Carnegie Mellon campus was relatively quiet during the early stages of the Troubles, my first contact with them was through my daughter Kathie, and her husband, David, who were students at the University of California, Berkeley. They were among the 600 students who occupied Sproul Hall, the administration building, and were carried out after some days.

I devote a separate chapter to my involvement with the Troubles not because it was particularly dramaticit wasn'tbut because it reveals a great deal about my attitudes toward the political process, and about my educational views as well. What I said and did in the face of concrete situations will convey my beliefs more reliably than could generalities. Of course, you must take account of the fact that I was a participant, and things said partly for rhetorical effect are not said in exactly the same way as things said in the quiet of one's study.

Advice to Revolutionaries at Berkeley

Perhaps the best expression of my attitude toward the Troubles at Berkeley is the "Manual for Revolutionists" I wrote for Kathie, David, and their friends at that time. In the letter, part of which is reprinted here, I opined that they had lost their cause by espousing the "dirty speech" movementfreedom to utter profanity as a form of free speechwhich alienated middle-class support. (The letter is not irrelevant to the events drawn on a much larger canvas in China and in Eastern Europe during 1989. I will leave

it to my readers to judge whether my advice to revolutionaries would have been useful or counterproductive for the participants in those movements.)

May 23, 1965

Dear Kathie and David:

I am sorry to have been so slow in composing my Manual for Revolutionists.

A revolution aims at bringing about fundamental changes in institutions by employing illegal tactics. What is legal and what a society will tolerate are distinct. When there is sympathy for ends, illegal means may become acceptable and the laws against them unenforceable.

If a revolution aims at overthrowing an entire legal system, the role of illegal action is to arouse an already sympathetic population; to goad the defenders of the legal system to severity that will arouse additional sympathy; to demonstrate strength, hence to reduce fear of the authorities and to increase fear of the revolutionaries; and finally, to seize weapons and strong points. When people no longer believe that the existing laws can be enforced, the first half of the revolution has been won. There remains the task of securing it for the "right" party. This has been the common point of failure for the moderates.

The Free Speech movement at Berkeley and the civil rights movement nationally have different aims than those just described. They are radically defending, rather than attacking, the legal order. Here the laws are "better" than the government *and public opinion* are prepared to enforce. Here the revolution gains from increased acceptance of the legal system, and loses from acceptance of illegal actions. In the past the courts have been the chief bastions of defense of personal liberties. Hence, an increase in people's willingness to accept judicial procedures aids the revolution, a decrease hurts it, making the tactics of civil disobedience, to this extent, dubious.

This revolution has a narrower base of public support than most successful revolutions. Revolutions do not often have the active support of majorities, but they almost always have their *sympathy*. From public opinion polls over the past generation on race and free speech, you can conclude that:

1. For many people, the issues you are fighting for are only peripheral. Most people do not have strongly held opinions about them and, hence, their opinions are subject to rapid and drastic change.

2. These ephemeral shifts in opinion are generally produced by dramatic events. Whether the events evoke favorable or unfavorable attitudes depends on just what is brought to attention. Thus "Manhattan Subway Robbery" (I didn't even mention race) evokes quite different responses from "Negro Scientist Unable to Purchase Home."

3. On racial equality and freedom of speech, favorable attitudes in much of the American public are supported more by guilt than by enthusiasm. Hence, opposition to measures to secure these rights is easily rationalized if information is provided ("California rights students are beatniks") that reduces the guilt.

Securing public support is crucial to any revolution, but particularly critical for one that seeks stronger enforcement of laws. The laws are not enforced now because they go beyond public opinion—they define legality in ways that many people do not think reasonable.

Under these conditions, the revolutionaries must use illegal tactics sparingly, and only when their *ends* are overwhelmingly supported by public opinion. During the autumn, the free speech movement did have widespread student and faculty support. The sit-in might well have been a reasonable tactic. When defending the action in the courts, however, it is important to redirect attention to the ends, and not allow the case to rest on whether you did, in fact, interfere with University business. (The "police brutality" issue is also effective *if* you can make it convincing.)

The "dirty word" issue, however, is an extremely poor one on which to make a stand, for it will seem so outrageous to most people that it will lose goodwill for the more important questions of freedom. The revolution must convince the middle class. The revolutionists cannot have the luxury of denouncing them as "bigots" or "squares."

And a martyr or two never hurts a cause, although it's a bit hard on the martyr. Considering who was in Sproul Hall, I'm glad no one was killed, but the free speech movement would probably be further ahead if someone had been. But then, one isn't much of a revolutionary unless he is prepared for at least a teeny bit of bloodshed, is he?

Revolutionists who accept my Manual of Tactics without being willing to act on it are Expressionists. For an Expressionist, feelings are more important than the success of the cause. (He is cousin to the man who boasts, "I sure told *him* off!").

We are all Expressionists part of the time. Sometimes we just want to scream loudly at injustice, or to stand up and be counted. These are noble motives, but any serious revolutionist must often deprive himself of the pleasures of self-expression. He must judge his actions by their ultimate effects on institutions.

Sitting here, 2,500 miles from California, I have been trying to assess over the past months how effective the revolutionary leadership at Berkeley is, and how much, on the contrary, it is plagued by Expressionism. I thought the score was pretty good until the "dirty words" came along, then I was less sure.

Well, most of the campus revolutionaries of my generation were Expressionists. But, of course, young people today are better educated—we

expect them to know their Machiavelli. I'll continue to watch with interest. Good luck.

Love,
Dad

Student Unrest at CMU

A few years later, at CMU, I had a good deal of contact with students who fancied themselves Maoists and were calling for world revolution and the seizure of power from those of us who were over thirty. Given my earlier contacts with 1930s radicalism, I did not hold out much hope for their naïve idealism, but carried on dialogue with them. Their values were nearly all correct, their ideas for reforming the world nearly all wrong.

The Troubles reached the CMU campus when students supported Pittsburgh blacks in their attempt to penetrate the all-white construction workers' unions at a time when the university was erecting a new science building. It was an admirable cause that I applauded but took no active part in. When a campus-wide meeting was called to protest the administration's lack of initiative on the issue of desegregating the union, the university's president made an unfortunate statement that could have had disastrous results. To emphasize his sympathy with our black students, he said, "Why, I had lunch with some of them yesterday, and found that they're *real people*." He had the best of intent, but a tin ear for language.

An accommodation for desegregation was reached between the construction unions and the demonstrators, and the campus again cooled off and remained relatively cool through most of this period of national turmoiluntil the student newspaper was radicalized.

Encounter with the Tartan

Some time after the construction union episode, student Maoists, mainly in the Architecture Department, gained control of the weekly student newspaper, the *Tartan,* and began to stir the waters about "the poverty of student life," a slogan they had acquired from the French revolutionary student leader Daniel Cohn-Bendit. As at most universities, many members of Carnegie Mellon's faculty (and administration), even those who did not think student life was poverty-stricken, sympathized with the students, or at least were passive about the whole business.

Being annoyed especially by the growing incivility of our students, I decided to get into the act. I wrote a letter to the *Tartan* urging the university

to end its subsidy of the newspaper on the grounds that a kept press could not be a free press:

September 28, 1970
Editor, the *Tartan*
Carnegie Mellon University

Dear Sir:

Freedom of speech is the right of those who have views to express them. It does not impose a duty upon those who disagree with particular views to subsidize them.

The *Tartan,* through the non-voluntary contributions of all CMU students (some $15,000 a year, I believe), subsidizes the propaganda of a little band of self-appointed "radicals" who use it to preach a muddle of tea-party anarchism. It is ludicrous that the "all-powerful" Establishment should foot the bill for the call to revolution. It is more than ludicrous, it is immoral. . . .

The principle is simple: No student should be forced to contribute to the dissemination of political views with which he disagrees. Any system of obligatory contributions to this end, whether funneled through the university or administered by a separately incorporated student association using the university's strong arm as collection agent, is a violation of freedom of conscience, and should be stopped *now*.

I wish to propose to students, my faculty colleagues, the Administration and the Board of Trustees a complete cessation of subsidies to student publications. I will be glad to hear from others who would like to join me in reasserting on this campus the rights guaranteed by the First Amendment.

Sincerely yours,
Herbert A. Simon

My letter drew the *Tartan* editor's outraged fire. In my reply, I objected to the *Tartan*'s devoting excessive space to reproducing the views of Cohn-Bendit, and offered to "provide the *Tartan,* for five weeks, with some alternative views (my own, not canned) about the world." My offer was accepted, and eight (not five) columns that I wrote were published during the semester, under the headline SIMON SAYS.

As the first four columns were printed surrounded by a border of American flags, I decided that I had won the battle when the fifth column appeared without the border. The columns terminated soon after the New Year, long before I had presented my program of university reform, because some students cried uncle, complaining they didn't have space for their own writings. I counted that as my second victory, and quietly withdrew

from authorship. (Producing a weekly column of 1,000 words is a lot of work.)

Simon Says

I would not put my columns forward as literary gemsthe irony is a little heavybut they reveal many of the views I held at that time (and mostly still hold) about revolution, about university education, and about technology and society. In this, as in my other professional activities, I have tried to bring social science knowledge to bear on the issues at hand. Let me, without quoting the columns at length, communicate a bit of their flavor.

The first article warned against the degeneration of reform movements into power struggles, implying that this was already happening with the student movement: "So let's stop talking about the goals of reform . . . and go about the work of getting THEM out of power and US into power. Let's count the number of students (if that's who WE are) who sit on committees. And let's increase that number of students and the number of committees."

In the second column, I talked about the university as "an environment for learning," arguing (pure Elliott Smith) that "learning results from things the student does, and not (except indirectly) from things a teacher does." Then a little aggression against the elective system: "Student demands for unrestricted freedom to choose their courses are always accompanied by demands for close tutorial relations with individual professorsthat is, with demands for emotional and intellectual dependence. If the university is to help students toward self-dependence, we must resist these demands."

My recipe for university reform (pure University of Chicago): "Abolish course requirements and time-serving requirements. Permit each student, whenever ready, to demonstrate his or her skills and knowledge. The demonstrations can take the form of comprehensive examinations and independent papers. Include in the scheme a central core of skills and knowledge that all students must acquire."

The third column returned to the political theme: "How can we move and shake the walls of ivywithout bombs?" We begin by learning how to compromise, to form coalitions: "Those who think they will settle for nothing less than best worlds . . . surrender the chance of working for better worlds." Then we bring knowledge to bear: "If reform is not just a matter of conflict of interest . . . then thought and reason have a place in reform. The power of ideas does not derive from unnegotiable demands, intimidation or violence."

The fourth column took up the relation of liberal to professional education. Acknowledging the energy, environmental, and other problems stemming from technology (prominent targets of the student movement), I argued that science and technology were not only part of the problem but also an essential part of the solution: "Science and technology permit us to reach higher goals—material and moral—hence to set them still higher. Science and technology enable us to see consequences of our acts that were previously hidden from us. We can never again carry disease to strangers in the innocence of ignorance. In both these ways, science and technology become key forces in enriching our moral values and raising our aspirations. In doing so, they make our task harder. But they also give us reason to believe in the reality and continuing prospect of human progress."

In the fifth column, we are back to the political theme, in this case my response to the cynicism of students about our political institutions. The message was that you can't have democracy without politics, they are simply good-guy and bad-guy terms for the same essential, imperfect institutions: "We . . . imagine ourselves alienated from politics without noticing that we have thereby fallen out of love with democracy." And again I sound praise of compromise: "Living with other people—two hundred million of them—means living with other views. And that means learning the morality of bargaining and compromise. Perhaps it is fortunate that Utopia hasn't been proclaimed yet. I have a hunch that if it were, it would be the Utopia of those other fellows who outnumber us, and not ours."

The sixth, pre-Christmas, column takes a lighter tone, discussing the problems an environmentalist might have with Christmas shopping. Then, after a brief silence, came the seventh, post-Christmas, column, which tells how Dorothea and I rang in the New Year of 1971 in Cuzco, Peru. It is quoted here in full, as it captures best the spirit in which I undertook this whole journalistic exercise:

Not "gone with the wind," as was suggested last week by the Tartan's querulous feature editors, but just catching up with the backlog after the midwinter vacation that gave my wife and me a first-hand view of the ongoing social and political revolutions in Chile, Bolivia, and Peru, as well as the possibly impending one in Argentina. "Glimpse" would be better than "view," for I am most skeptical of tourists' impressions, even when the tourist is me. But forming impressions is halt the fun of traveling—just so long as one doesn't report those impressions as geopolitical facts. Impressions, for example, of the New Year at Cuzco.

High on the Peruvian Altiplano, two miles above the sea, Cuzco, dreaming of ancient Inca glory, awaits the new year. On little tables set up under the

arcade that surrounds the central plaza, Indian families are feasting on soup and corn and roasted meats, and drinking chicha and pisco that have been brought in from the countryside and prepared there. At midnight, the bells of the three great churches fronting the plaza will call many of them to Mass. Many will still be in the square at dawn, which comes early on this almost midsummer day in the Southern Hemisphere.

The tourist, who can mill and mingle without comment or notice in the crowds around the plaza, finds it exoticthe Inca peasant costumes, the medley of Spanish and Quechua tongues. And then it all seems very familiar: he senses the same stir, the same mood of expectancy and celebration that he would find on this night in Times Square or in the center of his own hometown.

On New Year's Day, as he boards the train that will take him through a great mountain gorge to the ancient Inca outpost of Machu Picchu, the tourist buys a copy of Cuzco's morning (and perhaps only) newspaper. The lead editorialin conventional trite phrases, which he has read many times beforesets forth the hopes for the next year, for the next decade, and reflections on the year and decade just past. He would read those same hopes and reflections if he opened the paper this morning in New York, in Stockholm, in Tokyo, or even in Peking.

All humanity opens its eyes today toward a new horizon, and plants its feet on unfamiliar paths, but foresees that they have to lead it to higher stages, to ways of life always more just.

And in this mountain outpost of the world, this living museum of the Inca past (preoccupied, one would suppose, with the daily struggle for food and shelter, with its painful effort to bring itself into the modern world to make that struggle easier), how does the editor write of the past and future decades? Does he talk of per capita income, land reform, the growing political power of the Indio, industrialization? None of these. Perhaps they are too obvious and familiar for mention even on this ritual occasion. Perhaps, important as they are, they are not the center of the writer's vision of Man. What is his vision? What does he find worthy of mention from the decade past; what does he hope for the next one?

And so the last decade, with its terrible trials of man against man, passes into memory; the material visit of man to the Moon remains news of first magnitude; a robot replaces Man with advantage in his stubborn urge to commune with the stars; there has been recorded the case of a mortal who kept his own heart as a decoration on his mantel.

The past decade was a Pandora's box of unsuspected marvels; that which commences today promises to be, without fear of disappointment, even more marvelous in the field of science. But it ought to be equally the bearer

of something grander, more sublime, of which both science and man have lost the vision: tranquility of spirit, peace of heart, the source and germ of human happinesshappiness, the eternal and at the same time unreachable dream of man.

And so, seven thousand miles from home, standing by the huge plinth of the Temple of the Sun at Machu Picchu, facing the great, abrupt mountains half hidden in mists rising from the gorge, the tourist joins that unknown writer and all the generations of his Inca ancestors in the traditional and hackneyed, but genuine, hopes for man, but especially for Man's imagination and spirit, in the New Year:

Our sincere and fervid wishes for a destiny in every way superior for our nation, for this Cuzco, which deserves a better lot, for our community, and above all, Peace on Earth for all men.

My final column, an anticlimax, was a plea for a formal group in the university to concern itself with long-term educational planning. Without such a group, planning is everyone's business, hence no one's business. This, like my proposal to provide the faculty with a liberal education, has not yet been implemented by the university. With that eighth column, I terminated my short career as a newspaper columnistan exhausting occupation.

The Troubles Wax and Wane

Perhaps the biggest surprise I experienced from the student movement was the response of faculty members to student incivility, and to official reactions to student violence. A great many university faculty members, it appeared, were emotionally wholly dependent on the love of their students. They could not stand the students being angry at them. When students behaved as though they were withdrawing their affection, or when the university or public authorities took even moderately strong defense measures, these faculty members were devastated and unable to respond in an instrumental way.

This was especially evident in their reactions to the violence in Chicago at the time of the 1968 Democratic Convention. To most faculty members, it was inconceivable that there might be two sides to the story, that the police might be nearly as much sinned against as sinning. Their children had been attacked! (It was much like parents defending their rowdy kids at police court: "Not *my* Johnny; he's a good boy!") On campus, moral indignation was the principal response to such events.

During the period of the Troubles, I had one interesting teaching experience in GSIA (I think *interesting* is the right word). I regularly taught a course on organization theory to Master's students. My biggest problem was finding some substitute for the organizational experience most of the students lacked, so that the course would be more than abstract and the students could acquire some genuine skills, at least skills of diagnosing organizational situations if not of handling them.

Around 1970, amid students' constant demands for more autonomy and power, I decided to see how they would use autonomy when it was given to them. On the first day of my class on organizations, I announced that the purpose of the course was to help them develop their organizing skills. There was a textbook (March and Simon 1958) that might contain some helpful information and advice, and there was an instructor available to them. Their first assignment was to organize themselves in order to plan and manage their educational experiences in this course. I could be found in my office whenever they thought I could be of help to them. I said all of this quietly and pleasantly. Then I left the classroom.

Consternation! Why was he angry at them? Weren't they paying tuition, and wasn't it the job of the teacher to teach them?

They flunked. Most of them never got organized to manage their education. One small group did form a team, and came to me periodically for discussion of their plans. They completed a projectI don't remember its natureduring the semester, and thought the experience valuable. In later years, some other members of that class, seen at reunions or visits to the campus, have also confided to me that it was a valuable, if shocking, learning experience. I never pressed them too far to say just what they had learned.

Most of the students simply stayed angry, went off in a sulk, and, I suppose, devoted the extra time to their other courses. Of course, I did not literally flunk themthat would have been a real hassle. But they were completely pissed off, to use one of their favorite expressions, and on that semester's faculty evaluation, gave me the lowest rating, by several standard deviations, that I have ever received. So not only do teachers need the affection of their students, students need the affection of their teachers. In the initial class session I seem to have communicated that I was rejecting them. Perhaps I was; students were awfully ornery during those years.

The Troubles at CMU tapered off after a tense weekend following the killing of four students at Kent State University in 1970. We managed to prevent the burning of the building that housed the ROTC, held an all-night, all-talk vigil with the students, and brought the campus nearly back

to normal. The same thing was happening on campuses all over the country, and our students were relatively restrained in their response.

Just a couple of years after these events, I recall arriving on the campus on the first day of the autumn semester, noting the general cheeriness on student faces, and saying to myself, "I'm glad *that's* over." Whether the yuppie climate that replaced revolution has been, on balance, an improvement is a question, but along one dimensionthe level of civilitythere is no debate.

Chapter 19
The Scientist As Politician

In chapter 10, I described my activities in the politics of social science, especially as an adviser to the Ford Foundation and a member of the board of directors of the Social Science Research Council. Beginning in the mid-1960s, my focus of political activity moved from New York to Washington and from the social sciences to science in general, and in particular, to the National Academy of Sciences.

For some members of the National Academy of Sciences, the principal activity of that organization is to elect new members. Debates on the membership quota to be allotted to each discipline (the academy is divided into classes and the classes into sections) are carried on with deadly seriousness, and an elaborate nominating and election procedure, working upward from sections to classes to the entire academy, maintains careful checks and balances in the filling of these quotas.

In its choice of members, the NAS is as fallible as any university. While almost all of the 1,600 scientists who belong to NAS have distinguished records of research, of course not all scientists with equally distinguished records have been elected. Whether or not the election process is fallible, few scientists in the United States, when chosen, do not feel a warm glow of pride in this recognition from their peers. Resignations and refusals to accept membership are so rare as to be quirky.

But the NAS is more than a mutual admiration society. Its charter requires it to provide advice, on request, to the national government. To perform this duty, it operates (in collaboration with the National Academy of Engineering and the Institute of Medicine) an organization called the National Research Council (NRC), a vast labyrinth of commissions and committees.

The NRC has a full-time technical staff of about 3,000 persons, I believe, but the members of the advisory commissions and committees are all unpaid

volunteers drawn from the scientific, technical, and professional communities of our country. In its activities, the NAS and the NRC have access to the whole of whatever expertise and wisdom is to be found in the United States; few persons refuse to serve when called, unless prevented by other commitments.

There is every evidence that the advice provided by the NAS and its affiliates to the president, federal agencies, and the Congress is listened to carefullywhich is not to say that it is always accepted. In national policy making in domains where science and technology are relevantand nowadays that means most domainsNAS plays an important role. Moreover, its role is not entirely that of passively providing answers upon request. The NAS can usually put questions on the agenda when it thinks them important and when other players do not seem to be attending to them. It can generally arrange to be asked for advice when it thinks advice needs giving.

The Social Sciences in the National Academy

Until the late 1960s, membership in the NAS was limited mainly to physical and biological scientists. I say *mainly* because some members were anthropologists (principally physical anthropologists and archaeologists) and some psychologists (principally physiologically oriented experimental psychologists). A few, but very few, among these regarded themselves as social or behavioral scientists. Neal Miller and George A. Miller were such exceptions among the psychologists, and George Peter Murdock among the anthropologists. The National Research Council was correspondingly narrow.

Unfortunately, problems of public policy do not divide along disciplinary lines. The problem of air quality, for example, involves meteorology, air physics and chemistry, physiology and medicine, automotive and power engineering, and economics and urban sociology. AIDS calls for medical and biological research, and equally for research on sexual behavior, sociology, medical economics, and other topics. National security and disarmament questions extend all the way from particle physics to political science, leaving few disciplines between untouched.

Membership on advice-giving committees of the NRC was not limited to academy members, nor to natural scientists. Nevertheless, the controlling decisions about committees and their members were in the hands of the physical and biological scientists. It was *their* recognition that social science expertise might be relevant that determined whether social scientists would be brought into the process. This relevance was often not recognized when

it should have been. Natural scientists simply were not sufficiently aware of the social science aspects of policy questions to respond appropriately to them.

To some of us observing the role of the NAS in national policy making, it seemed very important that the social sciences should participate more fully in the advice-giving process. We were wary of the technological fix, and equally wary of amateur psychology, economics, sociology, and political science provided by physicists, chemists, and biologists. To improve the situation, two roads seemed open: to create a parallel social science academy, or to bring the social and behavioral sciences more fully into the existing NAS/NRC structure.* It was my own belief, and that of many others, that the second road was the more promising.

We preferred union to separation for two reasons. First, the social sciences were politically vulnerable. That had been shown when they were struck from the original enabling legislation for the National Science Foundation; and their gaining a foothold in NSF had subsequently been a slow, painstaking process. They were (and are) still far from being full partners in research funding. The social sciences had some friends in Congress, but they also had some virulent enemies. A united front with their hard-science colleagues seemed the politically wise approach.

Second, participation in the NAS would strengthen social sciences' hard wing, which in belief and practice was most like natural science. This has also turned out to be correctperhaps too correct, for in the election of social scientists to the academy in subsequent years, there has been an excessive tendency to restrict candidacy to those anthropologists and psychologists who are most "biological," and those economists, political scientists, and sociologists who are most mathematical. The membership, particularly in psychology and economics, is far from showing a balanced picture of social science.

The campaign for union was led by the small band of social scientists already in the academy. There was a considerable favorable sentiment among younger members of the academy for recognizing the social sciences as genuine scientific disciplines, although there were more than a few diehards (and still are) who were vocal in their opposition. Philip Handler, president of the academy, being generally sympathetic to our demands, was of great help in bringing about the change.

The process began in the NRC, when the initiative was taken by Neal

*At the time of which I am speaking, there was no National Academy of Engineering or Institute of Medicine. I will leave aside the complications that arose when they were brought into the picture.

Miller, George Murdock, Carl Pfaffman, Ernest Hilgard, and others who were active in the division of the NRC that handled activities in anthropology and psychology. A large part of the work of this division consisted in advising the military agencies on scientific questions relating to vision and hearing and on methods for training military personnel.

In the mid-1960s the Division of Anthropology and Psychology of the NRC began to elect to its governing board social scientists who would not qualify, under the existing limits, for membership in the academy. Several of these new members, beginning with the demographer Kingsley Davis and I, were asked to join this board with the prospect of later chairing it. Demography, after all, was almost a "science," as it used quantitative data. I was well known to have interests in applied mathematics and computer science; and, partly as a result of my RAND connection, I had many acquaintances among natural scientists and mathematicians.

The next move in the game plan was to propose these prospective chairs for membership in the academy. The problem of their disciplinary classification could be avoided by using a special procedure (the voluntary nominating group), authorized by the by-laws, which bypassed the disciplinary sections of the academy.

The strategy was successful. Kingsley Davis was elected in 1966 and I in 1967. (On the list of publications presented to the members to document my qualifications, the first item was my paper on the axioms of mechanics, published in *Phil Mag,* a respected physics journal. Whether it was this mild misrepresentation of my identity that swung the vote or something else, I do not know.)

In the next three years, and without depending on the division chair as a subterfuge, we were able to use the voluntary nominating groups to bring Kenneth Arrow, Robert Merton, Tjalling Koopmans, and Paul Samuelson into the academy. Meanwhile, we were using both persuasion and the threat of a separate academy to get acceptance for a comprehensive Social and Behavioral Sciences Class within the NAS. Aided by the fact that the academy was besieged by similar demands from the engineering and medical professions, the necessary changes in the NAS structure were brought about in time for the 1972 academy elections.

The annual election quotas of the various classes were temporarily increased to allow the new sections to be brought up to reasonable strength over a period of a few years. I was heavily engaged in the nominating and election processes during these years, playing a major, although not always decisive, role in shaping the new social science membership. It was neither difficult nor terribly controversial, for there was a substantial backlog of "obvious" candidates.

We made the decision at the outset to bring in younger members, even at the expense of postponing the nominations of such senior, and outstanding, candidates as Ted Newcomb and Paul Lazarsfeld. We did elect those two during their lifetimes, but I always felt a little badly about the injustice of the delay in their elections, however justified it was from a strategic standpoint. Talcott Parsons was also passed over, but in his case it was because some of us thought him too "soft" to qualify.

This political activity, starting even before my election to the academy, gave me considerable visibility in the science community. Almost as soon as I had become a member of NAS, I was invited to join its prestigious Committee on Science and Public Policy (COSPUP), chaired by Harvey Brooks, who took over that function from the committee's founder, George Kistiakowsky.

During my tenure on COSPUP (1968 71), some of its main activities were to supervise and review a survey of the biological sciences, conducted under the chairmanship of Phil Handler, and to advise Congress, at the request of a committee headed by Representative Emilio Daddario, on what public processes were needed to weigh the benefits and dangers of new technology. The Congressional Office of Technology Assessment was one of the ultimate products of the latter activity.

COSPUP was a demanding committee, as were most of my other activities in the academy. But it was also an exceedingly rewarding one. The public policy issues we examined had real importance, and sometimes we perhaps had some influence on the outcomes. Members, whatever our own expertise, received a broad education in every part of science with outstanding experts as our teachers. Most important of all was the sheer pleasure of associating with bright, wide-ranging minds and sharp wits. Even taking into account the unavoidable airplane travel, the hours I spent with COSPUP have been among my most pleasant and stimulating.

The President's Science Advisory Committee

I had hardly begun my work with COSPUP when I was invited to join the U.S. President's Science Advisory Committee (PSAC). The nomination of a social scientist to this committee was even more surprising than one's being elected to the academy. From its beginning, PSAC had been dominated by physicists. If we can talk of an Econometric Mafia, which mathematized economics in the two decades after World War II, it is even clearer that since the war there has been a Physics Mafia, which has dominated the

whole domain of science policy. The areas in which the need to advise the president on science policy was first perceived were electronics and the atom. The position of President's Science Advisor has been reserved for physicists, or physical chemists or electrical engineers nearly indistinguishable from physicists. The majority of the fifteen or so members of PSAC were physicists, with a small sprinkling of mathematicians, chemists, engineers, and biologists.

I was appointed to PSAC by President Lyndon Johnson, and joined the committee in January 1968, remaining on it until its demise at the end of the first Nixon administration. The chemist Donald Hornig was the President's Science Advisor when I joined the committee. Twenty years after the fact, I learned from Don that I was appointed to PSAC not as a social scientist (there was, in fact, at that time still strong opposition to such an appointment) but as an expert in artificial intelligence and computer science. I was personally acquainted, at the time of my appointment, with a number of the members of PSAC, and they thought they could communicate with me about the policy questions being raised by these new disciplines.

The scientific world (including myself), however, perceived the event differentlyspecifically as the enlargement of PSAC to include social science. And apparently my service on PSAC took the curse off social scientists, because James Coleman and Pat Moynihan were subsequently appointed to it.

PSAC did much of its work through panels, whose members were not all, or even mostly, PSAC members. Some years earlier, I had served on such a panel under the chairmanship of Walter Rosenblith of M.I.T. In about 1960, the USSR had created a research organization for cybernetics, directly controlled by the presidium of its Academy of Science. It was very hush-hush, and presided over by an Admiral Berg. Some staff members of our CIA, assigned to keep watch on this development, blew it up into a great Soviet plot to conquer the world with cybernetics. A thick report they drafted came to the attention of President Kennedy, who asked his Science Advisor, Jerry Wiesner, to evaluate it.

The report was full of idiocies. One that I remember was the claim that a key player in the Russian effort was "the well-known Russian economist Wassily Leontiev." At that time, Wassily Leontiev (future Nobelist in economics) was a full professor at Harvard, his family having emigrated from Russia in the 1920s.

Alas, our panel was too honest. If we had reported back to Wiesner that the Soviet cybernetics project was genuinely dangerous, American research in artificial intelligence would have had all the funding it could possibly use for years to come. Putting temptation behind us, we reported that the CIA

document was a fairy storyas events proved it to be. This was my one experience with PSAC prior to my membership on it.

The invitation to join PSAC posed a problem for me. The committee had always been deeply involved in advising on military, and especially nuclear, matters. By 1967, I favored American withdrawal from Vietnam. Could I in good conscience serve on a committee that would be active in proposing technological solutions to the military's problems in that country?

I decided that I could serve. First, if there were a way to win the war quickly, it was important that it be found, although I doubted that I could make any contribution to finding it. I was at that time not against our preventing a Communist victory in Vietnam if it could be done. I favored withdrawal because I did not think we knew how to win the war. Second, PSAC had other concerns besides the war. I could focus upon them as my main committee activity.

Appointed to PSAC, I found that it was indeed deeply engaged in trying to make technical contributions to an American victory in Vietnam, especially to finding ways to block the Ho Chi Minh Trail. The ingenuity of PSAC physicists turned out to be no match for the ingenuity and desperation of North Vietnamese peasants and soldiers, however. As we now know, no effective ways to cut the trail were found. Some groups in the Pentagon welcomed these efforts from talented physicists; others resented the interference of outsiders, Monday-morning quarterbacks.

Within the White House, Henry Kissinger and his deputy, Colonel Alexander Haig, were especially skeptical of the interference, and did everything in their power, including giving meaningless "briefings," to keep PSAC from learning anything it did not already know. I walked out in the middle of one of Haig's briefings, ostensibly to make sure I would not miss my plane.

I made essentially no personal contribution to the Vietnam activity. My technical knowledge did not qualify me to contribute in any significant way. Often, I could recognize foolish ideas for what they were, but so could the others. I had to decide what my positive contribution to PSAC would be. My first decision was that I would not be the token social scientist, although I would certainly bring social science knowledge into the conversation when it was relevant. I would find some role in which I could operate as a full committee member, indistinguishable from the others.

PSAC operated both as a committee of the whole, during its monthly meetings, and in panels (they were never called committees). There was a sentiment to revive a panel on the environment, a previous one chaired by John Tukey having produced a valuable report in 1967 laying out the whole range of environmental problems. I gladly agreed to head the new panel. While others would be more knowledgeable than I in almost any specific

area, no one would be expert over more than a few areas, so I would be at no disadvantage.

Besides, as a result of my youthful forays into entomology and ecology, I could at least drop a few names at appropriate moments. I had never lost the concern for environmental problems that was first stirred by a pamphlet I found in my father's library, "Timber, Mine or Crop?" And I was familiar with such classics as Needham's *The Life of Inland Waters* and the ecology textbooks of Clements and Weaver, which I had studied carefully during the days of Rockmarsh (see chapter 2).

There was no reason for the new panel to produce another public report. Instead, we decided to focus on specific problems of high priority and carry analysis to the point where we could recommend the next steps to take. We recruited experts for subpanels on air quality problems, water problems, chemistry (detergents were a big issue then), environmental economics, and so on.

Only two other PSAC members showed a strong interest in my panel: the physicist Murray Gell-Mann, and the vice president for research of Ford Motor Company, Mike Ference. It turned out that Murray and I were quite competitive. But he was more interested in wildlife conservation and related matters than in other environmental issues. So our paths soon diverged, and he created another PSAC panel closer to his interests.

During the last year of the Johnson administration, our panel was most active on the detergent question. Proposals were being put forth in Congress and elsewhere to ban the phosphate detergents then being used without much attention being given to alternatives. When we investigated, we found problems with the alternativesthe cure might be as bad as the disease. We put out some "go slow" recommendations, and no precipitous actions were in fact taken.

PSAC met with President Johnson only once during this year, a meeting that was somewhat unnerving. The president had a single thing on his mind: the unreasonableness of Ho Chi Minh. Why was Ho so stubborn? Why would he not accept the reasonable compromises that had been offered him? Whatever the agenda, it always came back to that. We saw a man who had been destroyed by his adversary, and we came away saddened and concerned.

After the 1968 presidential election, we submitted our pro forma resignations, but it appeared that President Nixon would continue with the committee, only replacing those whose terms had ended. Lee DuBridge, former president of Cal Tech, became the new Science Advisor. The rather chaotic meetings that had characterized PSAC under its former chairman now became quieter and more orderly.

I continued to go forward with the agenda of the panel on the environment, still retaining that as my main activity. My excellent Office of Science and Technology staff man, John Buckley, a field biologist and ecologist, pushed our activity at a good pace, providing excellent staff support.

One of the first issues that President Nixon had to face was how the federal government should organize to deal with environmental problems. Congress was pushing for a Council on Environmental Quality, and it was not then good politics to be anti-environment. As a countermeasure, Nixon created a Cabinet Committee on the Environment, asserting that the environment was so important, and the issues so interdepartmental, that it could be dealt with only at the Cabinet level.

Of course, no one knew how a Cabinet committee should operate—it was an unprecedented structure. Since it had no staff, who would prepare its agenda? President Nixon turned to his Science Advisor, and Lee DuBridge turned to his environmental panel, which accepted the responsibility for the Cabinet committee's agenda, at least until other arrangements could be made.

High on the list of pressing problems was the regulation of auto emissions of oxides of nitrogen, which had not been regulated in the initial Clean Air Act because of doubts of its being technologically feasible. Meanwhile, in briefings by people from the automobile industry, President Nixon had been told that the auto pollution problem was well in hand, and that no new regulation was needed. The charts they showed to the president, of course, had to do with the reductions in emissions of sulfur oxides and carbon monoxide, not with oxides of nitrogen. We were quick to put the latter at the head of the Cabinet Committee's agenda and to prepare a memorandum about the seriousness of the problem and the feasibility of doing something about it.

On this matter Mike Ference was very active on the PSAC panel, and by virtue of his company position, he could claim some expertise on auto emissions. He argued (against my amateur memorandum) that our chemistry was all wrong and our recommendation correspondingly incorrect. Unfortunately, he had been away from science for some years, busy with administrative concerns, and in that time he had forgotten a lot of chemistry. Besides, he was a physicist by training, not a chemist. I went home, pulled my old Schlesinger *General Chemistry* textbook off the shelf, and wrote a brief essay on nitrogen fixation in the atmosphere at high temperatures which supported the panel's memorandum. The panel bought my story, and we did not have to retreat.

I have always imagined that our action had something to do with the Nixon administration's supporting the regulation of nitrogen oxides and

with the new controls that were soon in place. One reason for believing that we had some influence is that, shortly afterward, I was invited to join the Scientific Advisory Committee at General Motors, with a not inconsiderable annual stipend. I declined, mainly because I thought there was an obvious conflict of interest as long as I was PSAC's man on the environment. By today's standards, both Ference and GM were rather insensitive to conflict of interest issues, but twenty years ago such insensitivity was more common, or at least more acceptable, than it is now.

During my PSAC years, there was much nationwide concern for education. In particular, the New Left was proposing drastic changes in the existing "bourgeois" educational system. Head Start was one of the significant (and less revolutionary) initiatives of that period. The Harvard chemist Frank Westheimer, a fellow member of PSAC, formed a panel on education, which I joined. The panel focused its attention, as appropriate to PSAC, on how research might contribute to the improvement of education and how the federal government could organize to accomplish the research.

As a member of this panel, I prepared two memoranda, one on R&D in reading, the other reflecting on a series of visits the panel made to some of the principal laboratories in the country that were then engaged in educational research. My reading memorandum took up the topics of the need to know, citizenship and the "rich life," evaluation, high-level reading skills, and motivation and cognition. On the first topic, I commented:

> There is much folklore and little data on who needs to know how to read how well in our society. Probably a large fraction of jobs do not require one to read at all; others may require mainly the reading of instructions; others (e.g., secretarial) the transcribing of written material, and so on. If we had a better set of facts about reading skill requirements for jobs, we could point improvement in the teaching of those skills at the places where improvement might mean the most.

On citizenship and the "rich life," I was similarly iconoclastic:

> Most persons who are problem readers in school are unlikely, by means of any program we would know how to design in the next decade, to be brought to a level of literacy where reading could make a major contribution either to their leisure or to their performance as citizens. There is no evidence that I know that the trash that supplies the mass reading market is a more desirable anodyne than is TV fare. Even so-called "serious" reading material, on average,

probably misinforms as much as it informsat least, I can hardly think of a tract on contemporary problems that has reached bestseller status that refutes this allegation.

The site visits (in 1968) revealed few really exciting or promising educational research projects. In my memo, I divided them into New World projects and Better World projectsrevolutionary and incremental, respectively. What little excitement we found was in the New World projects:

[O]ur initial sample included only two representatives of New Worldsone of them ludicrously incompetent, but the other very impressive. If we extend our briefings to include blacks, city school superintendents, and radical students, we will be hearing a great deal more about New Worlds.

. . . I did not come away from our meeting with Herb Kohl prepared to join his Revolution, or even liking it. I did come away from it thinking that he had succeeded in making it somewhat plausible to meas none of the other New Lefters I have encountered didthat it just might come about. Given a little time, I think all of us could write several alternative plausible scenarios: (1) no revolutionit's just a fad that will be followed by some equivalent of panty raids or goldfish swallowing; (2) the revolution will spread, will weld the alliance between disadvantaged youth and pampered, educated youth . . . and will succeed in some form in ten years; (3) wait till the GIs get back to the campusesthere'll be lively head-bashing; (4) the New Left minority will produce a major backlash and a massive shift of society to the right.

. . . The relevance of this for our committee, I think, is somewhat independent of which scenario we write. For any plausible scenario, there is an important segment of our educational systemincluding the ghetto schools . . . where the motivational problem is not going to be solved by "If you just work hard at school, you'll live comfortably forever after." Many childrenhowever they will feel at age thirtycannot get excited about the goal of living comfortably, probably because they have never lived uncomfortably. Many others believe that they will live comfortably whether they work hard or not. Many of the disadvantaged believe that they will live uncomfortably whether they work hard or not. We can dislike these attitudes; we can regard them as unrealistic; the fact that they exist puts them right in the center of all education R&D activities that pretend to deal with important issues.

The Westheimer Panel made no significant impact on American education (thereby sharing the fate of innumerable committees that have addressed these topics in recent decades). It devoted itself mainly to supporting the newly formed National Institute of Education in its efforts to strengthen educational research, with perhaps some temporary success. But in the long run, the educational establishment took over. The thoughts of our panel, stripped of their special concern for the revolutionary rhetoric of that time, could still provide a useful manifesto for educational reform, and a useful list of fundamentals to which the reform should attend.

There were at least two other occasions when I put on my social science hat for PSAC. At the time the Club of Rome* report was about to be issued, predicting the occurrence of Doomsday early in the next century, Jay Forrester, seeking publicity for the report's findings, gained permission to present it at a PSAC meeting. My reaction was one of annoyance at this brash engineer who thought he knew how to predict social phenomena. In the discussion, I pointed out a number of the naïve features of the Club of Rome model, but the matter ended, more or less, with that.

On another occasion, we learned that someone had called Alvin Toffler's book *Future Shock* to President Nixon's attention. He, or someone on his staff, wanted an evaluation by social scientists of Toffler's thesis. I wrote a memorandum pointing out that social change had surely been more rapid in my grandmother's time (from agrarian to urban society, with the advent of rapid transportation and communication) than in my own, and that she and her contemporaries showed no great signs of psychological traumano more than any other generation. I could not understand, therefore, why our generation should be subject to any special "shock."

Toward the latter part of Nixon's first term, Lee DuBridge resigned as Science Advisor and was replaced by Edward David, from Bell Labs. David tried to obtain a bigger role for his office in the budget process for science, but succeeded only in having his nose bloodied by the Office of Management and Budget. During this period, a great deal of PSAC's attention was focused on two issues: federal investment in supersonic aircraft, and the construction of anti-missile defenses for missile silos. Both of these programs were strongly supported by the president but viewed with great skepticism by the scientists on PSAC.

With respect to the SST, there was concern about environmental effects (damage to the ozone layer), a concern that was then plausible but was

*The Club of Rome is a voluntary association of businesspersons and other individuals, mostly European, who concern themselves with the future of our world. The report mentioned here was based on a computer model of population, food, energy, and environment.

some years later shown to be unfounded. The other objections were economic. A civilian SST would clearly not pay for itself. Why not let the British and French be the pioneers if they were willing to foot the bill? This argument clearly offended national pride. (In almost all of these policy decisions the technical issues, both engineering and economic, were inextricably interwoven with value questions.)

With respect to missile site defense, the opposition argued for technical infeasibility, high cost, and undesirable consequences for deterrence and the nuclear arms race. The administration, when pushed with these arguments, used the "bargaining chip" reply: the site defense program would be a valuable trade-off in bargaining with the USSR about arms control.

These were not the only issues on which PSAC members disagreed with an administration policy. A board of advisers would be ineffective indeed if its advice always coincided with the propositions before it. But in this case, one of the advisers, Dick Garwin, a clever and persuasive physicist but with perhaps less than a full understanding of political processes, saw no reason why he should not testify on Capitol Hill against the administration proposals, which he did.

Within a few months, President Nixon decided that he really didn't need a science adviser or a PSAC. Toward the end of the first term, the Committee was essentially allowed to die on the vine, to be formally abolished after the election. I guess I was officially a member to the very end, although 1 was not active beyond the end of 1972.

More Academy Committees

My involvement with the National Academy of Sciences was not limited to helping build its new Social and Behavioral Science Class, and serving on COSPUP. During the 1970s, I also chaired several committees that were appointed to deal with specific policy issues. One was charged with examining the social and behavioral science programs within the National Science Foundation. We found the programs for support of basic research of generally high quality but underfunded.

At the same time, the NSF, with congressional urging, had established a RANN (Research and National Needs) program of applied research, much of it squarely in the social sciences. Unfortunately, the administrative arrangements for RANN were quite unsatisfactory; there was too much masterminding from the top and too little initiative left with the investigators.

Our committee's generally unfavorable evaluation of RANN was a factor in the program's demise soon afterward.

A second committee I chaired was charged with advising Senator Edmund Muskie on the auto emission standards to be incorporated in the renewed Clean Air Act. The logic of the problem is quite straightforward. First evaluate the marginal cost of reducing various auto emissions. Then estimate the connection between levels of auto emissions and levels of air quality. Then estimate the connections between levels of air quality and levels of pollution-related illness and deaths. Then place a value on marginal reductions in these health effects. Finally compare the marginal cost of reducing emissions with the marginal value of the reductions and recommend optimal levels of control.

We constructed subcommittees of experts to deal with each of these phases of the problem: a subcommittee on costs of emission reduction in autos, one on the atmospheric chemistry and effects of emissions, one on the health effects of air pollution, and one on economic evaluation. Of course we had no illusion that data existed, or could be made to exist, that would produce the numbers needed to fill out our conceptual framework.

Costs of reducing emission could be estimated fairly well (at least within a factor of two or three). But atmospheric chemistry was mostly a collection of scientific mysteriescomputer modeling could give only an exceedingly rough picture of what was going on. None of the available health data measured marginal effects; biostatistics were aimed only at assessing the lowest levels of substances that produced significant health effects. And the economic analysis depended on finding credible ways of placing a monetary value on health and life.

What does a committee do under such circumstances? It behaves like a group of reasonable people. It looks at whatever order-of-magnitude information exists and sets some upper and lower bounds to the possible. It then arrives at a conclusion about whether the existing standards could have been arrived at by reasonable persons (that is, whether they are not in egregious conflict with any evidence). If they fail this test, then it suggests that the standards should be adjusted either upward or downward.

Our committee came to the conclusion that the existing standards were generally reasonable, but could stand a bit of tightening here and there. Senator Muskie, wanting even more definite recommendations to take the political heat off his committee, complained a little to the press about two-handed scientists, who on the one hand say yes, and on the other hand, no, but I think our report pretty well met his needs.

Academy committee reports, before submission, are subjected to a thor-

ough review by an independent internal committee. This always produces the problem that these reviewers, wise and intelligent people, will second-guess the committees, also wise and intelligent people; but the review is clearly essential if the academy is to produce credible reports.

The report of our air quality committee had a rough time with several reviewers, who thought that our economics experts had reached conclusions about the value of reducing pollution that were more definite than the available evidence justified. I rather agreed, but since even a sizable change in those conclusions would not have changed our recommendations, I was stubborn in resisting revisions that would have delayed the report for months and probably have made it useless to the Senate.

Participation in these kinds of committee activities soon disabuses one of any notion that the natural sciences are "exact" sciences. As soon as they have to cope with the messiness of real world problems, as contrasted with the sometimes neat and simple laboratory problems, they become at least as inexact as the social sciences (which only rarely can retreat to the laboratory). I no longer have any patience with natural scientists who imagine that they have some kind of patent on exactness which they have not licensed to their social science brethren.

If I ever believed in the myth of the "exact sciences" or "hard sciences," my belief was wholly dissipated by encounters with such topics as air quality, eutrophication of lake waters, global warming, dietary standards, effects of low-level radiation, meteorology (for example, cloud seeding), and cold fusion. All of these topics contain uncertainties about the facts and their implications at least as serious as those we are accustomed to in the social sciences. The true line is not between "hard" natural science and "soft" social sciences, but between precise science limited to highly abstract and simple phenomena in the laboratory and inexact science and technology dealing with complex problems in the real world. The mythical image of hard science still prevails in some corners of the academy but seems to be slowly losing credence.

During the latter part of the 1970s, I found myself less active in the affairs of the academy and its committees, but returned to them again a few years later, as we shall see in the fourth panel. But before crossing into that part of my life, I must say something of the foreign travels that, after 1965, occupied me (and usually Dorothea) for about a month of each year.

Chapter 20
Foreign Adventures

Ever since my memorable twenty-first birthday, I have spent an enormous amount of my time en route: attending professional meetings and meetings of policy groups, lecturing on university campuses, consulting with business firms, scuttling back and forth to RAND, to New York, to Washington. I enjoy most of it, especially when it brings me to the mountains or the sea. Cities fascinate me too, and I am familiar with most of our U.S. cities and a majority of the largest ones in the rest of the world. Conference centers and lush resort hotels? Ho-hum.

During the years when the children were growing up, Dorothea and I often managed to combine professional travel with vacations, especially on our long drives to the West Coast. In more recent years, my domestic travel has been less leisurely, with many one-night stands on campuses. I suppose I give at least two dozen public lectures each year.

Until 1960, however, I steadfastly refused all professional invitations abroad. It just seemed too time-consuming, and from what I heard about international meetings, they sounded even less rewarding than meetings at home. I was not interested in scrounging for travel grants and working out the logistics of taking children abroad or arranging for their care at home. If I were going to travel abroad, it would be with Dorothea. There were lots of reasons for staying home rather than gallivanting alone.

The Travel Theorem

In 1960 that all changed, when Dorothea and I went around the world at the behest of the Ford Foundation, to attend an international management meeting (CIOS) in Australia, to evaluate management education in India,

and to catch a glimpse of Rome. That was just after I had first announced the Travel Theorem.

People react almost violently to my Travel Theorem. I try to explain that it has nothing to do with the pleasures of travel, but only with the inefficiency of travel for learning. They don't hear my explanation; they remain outraged. They point out that I seem to be traveling all the time. Why shouldn't other people travel too? After they simmer down enough to understand the theorem, they still attack it. It takes a long time to calm their passion with reasonand usually it isn't extinguished, just temporarily subdued. Why, they think, argue with a madman?

Let me state the Travel Theorem precisely, and then say how I came to discover it. Theorem: *Anything that can be learned by a normal American adult on a trip to a foreign country (of less than one year's duration) can be learned more quickly, cheaply, and easily by visiting the San Diego Public Library*. San Diego is not essential; you can substitute any other major city. (I conceived of the theorem when I was about to go to India, where I was supposed to acquire expertise in Indian management education and a good tanin San Diego, I could have acquired both.) The theorem holds in spades if the traveler does not have a fluent knowledge of the language of the country visited.

A word about this "normal American adult." The North American continent, even within the borders of the United States, contains a wide range of subcultures. I am thinking not just of Navaho Indians or Spanish-speaking Chicanos. I am thinking also of Appalachia, of California's Marin County suburbia, of the small town of Dubois, Pennsylvania (pronounce the *s: Duboys*), of deep Mississippi cotton country, of Greeley, Colorado, of the Cajun country around Lafayette, Louisiana, of Woods Hole and Martha's Vineyardall of them except the one we call home nearly as exotic as rural Yugoslavia or the Andean Altiplano, and a great deal more exotic than the Hilton hotels and their equivalents in Mexico City, Stockholm, Athens, Tokyo, Paris, Sydney, and the other great metropolises.

So most American adults, long before they grasp their first passport, have had contact with a wide range of human cultures. If they have been at all observant (if not, they will be no more so abroad), they will have learned what a peasant is, and what are his feelings toward the possession of land; they will have heard the political views of a blue-collar worker; they will have glimpsed the life of a Midwestern born-again Christian; they will have observed the flittings of Beautiful People, and the diligent journeys of salesmen.

Even these things they probably could have learned from booksfrom Theodore Dreiser and Sinclair Lewis, from Mark Twain and F. Scott Fitz-

gerald. But I am prepared to believe that people need a certain amount of direct experience with others quite different from themselves before they can escape from their shells of ethnocentrism and truly empathize with and understand the characters from alien cultures whom they meet in books. That's really all there is to the Travel Theorem: an assertion that a little experience goes a long way.

The theorem emphatically does not claim that travel is not enjoyable. I, for one, enjoy travel a great deal and have spent no little time in it. The theorem insists only that one should not put a veneer of virtue (if learning is virtuous) on what is done for pleasure and self-gratification. The function of the theorem is to produce a well-justified guilt in those who seek to reconcile their passion for travel with the Protestant ethic. Epicureans and bon vivants should have no problem with it.

The theorem was discovered during a period when I was advising the Ford Foundation about its programs in management education. Whenever the Foundation was considering a new program in a foreign land, it would send out an American expert to survey the scene in two weeks and return with recommendations. The expert need not have any background of knowledge about the country to be visitedonly about, for example, American management education, if that was the topic. The procedure was so obviously ridiculous that the travel theorem came to me in an immediate *Aha!*

Proving the theorem took a little longer. The proof rests on the observation that, for all the diversity of cultures to be found around the world, there are only a few basically different scenarios, all of which are readily sampled in the United States. I will not go quite so far as to say that when you've known one peasant you know them all, but I will assert that having spent a couple of days with a subsistence farmer in any land (the best way is actually to work with him in the fields), you are well prepared to watch and listen to a Chinese rice planter or an Indian ryot. More important, you are well prepared to understand and learn from books that describe their lives and society.

A first encounter with one of these scenarios teaches a great deal, even by casual observation and without real social interaction. But by the time one begins one's foreign travels, all of these environments have already been experiencedand in one's native language, which permits learning in some depth. One already knows (to a first approximation) about peasants, farmers, clerks and managers, factory workers, and professorshow they talk and what they eat for breakfast. I can find examples of all of them within 100 miles of my Pittsburgh home. So there is little more to be learned about the first approximation.

The *differences* among cultures are another matter. Some are visible to

the eyes (thatched roofs, rice paddies), in situ, or in pictures one can find in books. Detecting most of the others, however, including all the important ones, requires deep knowledge of the language. We are all familiar with the obnoxious habit among old China hands (or old Persian hands, or old Spanish hands) of studding their speech with terms like *guanxi* and *danwei* to show their superior knowledge of the culture. Ignoring the oneupsmanship that produces this behavior, it makes a valid point. It doesn't matter whether you know the Chinese words *guanxi* and *danwei,* but you cannot understand Chinese culture without grasping the set of values and behaviors they connote. These fundamental subtleties are wholly opaque to the instant traveler.

But local bilinguals can tell you about them, can't they? Yes, indeed, and so can books. And with books you can exercise some quality control over the information. You can make sure that their authors are qualified as experts and interpreters of the culture. You can learn about China from John Fairbank, or about Japan from Edwin Reischauer, or perhaps even better, from native experts who have been translated. You can read the translation of *The Dream of the Red Chamber,* or *Monkey,* or *The Tale of Genji,* or that wonderful description of life in Bengal (I found it in the Santa Monica Public Library), *The Autobiography of an Unknown Indian*. If you are unsure how to make up such reading lists for efficient learning, librarians are always glad to help.

You will find the picture books in the library too. If you have not yet visited Athens, do you expect to be surprised at the appearance of the Parthenon? Or, at Agra, of the Taj Mahal? Mind you, you will enjoy both of them immenselythe Taj Mahal in the light of the full moon is a cliché, but one well worth experiencing.

Dorothea pointed out that you will not find smells in the library (other than musty-book smells). That is true. It is also true that the experience of India is inseparable from the heavy, persistent odor of burning dung. But the inexpensive way to have that experience is to buy or steal a few cow platters from your nearest American dairy farm, dry them in the sun and ignite them. It is different from the benzine odor of the American outdoor barbecue, but equally interesting.

The special case where the traveler insists on the comforts of international-style hotels is an especially easy proof of the theorem. It is well known that one can circumnavigate the globe, penetrating deserts and jungles along the way, without ever venturing outside one's own Western, industrialized, air-conditioned culture, or learning that there is anything different from it. I have had a thrilling view of Ulan Bator and the Gobi Desert (probably the only glimpses I will ever have of them) from 30,000 feet, in the business-

class comfort of a B747. My atlas had given me more information about them, but could not give me the thrill.

Anyone who espouses the Travel Theorem becomes the target of constant gibes if he has traveled or plans to travel anywhere. One way to defend against these gibes is to plan your travel itineraries in such a way as to guarantee that you will not learn anything new. Our first trip to Europe, in the summer of 1965, will provide an example. We resolved that we would not see anything on that trip that we did not know better already from books and pictures. Consider the approximately two weeks we spent circumnavigating France by car.

We took as our targets Paul Cézanne and Maurice Utrillo among painters, Marcel Proust and Henry Adams among writers. Coming out of Switzerland, we followed the Route of Napoleon (known from our history books) down to Aix-en-Provence. There we stayed at the inn at Meyrargues, situated in an old castle, with a view of a Roman aqueduct from the window and a three-star chef. We spent our days visiting every spot we could find where Cézanne had stood when he painted Mont Ste. Victoire. (We had studied the book of Loran (1943), who had photographed all of these places.)

Not only did we find the sites, but it was easy to determine within three feet exactly where Cézanne's easel had stood. And when we stood on those spots, the mountain looked exactly as it had on Cézanne's glowing canvases: the literalness of his landscapes is almost beyond belief.* We learned nothing new; we had already seen the paintings.

From Aix, we drove through Marseilles (seeing Cézanne's L'Estaque), across La Camargue (described by Anatole France, and closely resembling the coastal marshes southeast of Houston), to the Pont du Gard near Nîmes (I had seen a model of it in the Smithsonian, but I had never before swum under it). We stopped briefly at Carcassonne, too well restored to be evocative, en route to Pau, where we visited the family, old friends though we had never before met them, who had housed and parented our daughter when she spent a high school semester there. From Pau, we drove through the pass of Roncesvalle, familiar from the *Chanson de Roland,* into Pamplona (the bulls weren't running that day), and back up the coast.

The next part of our journey was devoted to Proust and Adams. We spent a night in one of the inns at Mont Saint Michel, which, pilgrims and all (and we were pilgrims too), was precisely the medieval sanctuary Adams had prepared us for. In the morning, we roused ourselves early to see the spectacular tide roll in from the horizon, over the sand flats. From there,

* Oscar Wilde put it this way: "Where were the fogs of London before Turner painted them?" Nature, as usual, imitating art.

we drove to Cabourg (Proust's Balbec), for two nights in its Grand Hotel, with the open-cage lift and the cavernous dining room and the beach that Proust had so marvelously depicted. We were sure that we spotted Mme. de Villeparisis in the almost-empty dining room. And from its windows, we could see Albertine and her friends wheeling their bikes along the beachside promenade.

Then it was a half-day's drive to Iliers (Proust's Combray), where we slept in the station hotel, under big feather quilts. The house of Proust's uncle, with its Pré Catalan, was exactly as we had visualized it from our reading of *Swann's Way,* as was the little church where the Duchesse de Guermantes cast her spell. On Sunday, we reached Chartres, where we marveled at the porches theologically interpreted by Adams, and attended high mass with the sun shining through the great Gothic windows onto the white and gold robes of the priests.

Finally we came to Paris, where we could see, even as we approached the city, the high ground of Montmartre and the Sacré Coeur, familiar landmarks welcoming us back to a city we had never before visited. Paris, where I attended a meeting organized by the Centre Nationale de Recherche Scientifique (the ostensible reason for this journey), was of course known to us from innumerable books and pictures—we did not need to limit ourselves to Utrillo and Montmartre. We returned home full of our marvelous experiences, and wholly confident that we hadn't learned a thing. The prior education we had received from Cézanne and Proust and Adams and Utrillo and others had been complete and thorough.

Wandering

I will not regale (or bore) you with an account of our wanderings abroad from that fateful European trip down to the present. Dorothea and I fell into the habit of taking one trip abroad (rarely more) a year, typically of three weeks' duration. For one week I might attend a meeting or do some other presumably useful thing, to pay the freight. The rest of the time we explored, often by car. I also took a few relatively brief trips alone, mostly filled with professional activities. After a quarter-century of this, we have become formidable competitors in oneupsmanship when the conversation at dinners or cocktail parties turns to travel.

It would be a great pleasure for me to reminisce about all of these truly wonderful experiences, but with stern self-discipline, I will limit myself to Sweden and Japan, where my contacts have been more than casual. In the

fourth panel, I will add an account of later travels to China and the Soviet Union.

Sweden

I first came to Sweden in 1968 to receive an honorary degree from Lund University on the 400th birthday of that venerable institution. Students were on the march in Sweden that year, as they were in the United States and in Tokyoto say nothing of Paris. They heckled the Lund academic procession (the American ambassador marched in it) and staged their own counter-seminar during the celebratory week. But they did join the establishment in enjoying the public fireworks.

Lund roused my interest in comparing the Swedish and American student movements. What was causing students all over the world to rise up simultaneously against their elders? Since I was fairly fluent in written Swedish (and had actually learned enough to write and deliver my lecture at the university in that language), I began to read extensively about the Swedish New Left. When it appeared that I was going to return to Sweden again in 1969 to attend a conference at Aspenäs, I decided to spend a week in Stockholm exchanging views with some of the Swedish environmental specialists (I was then chairing the PSAC environmental panel) and meeting some of the writers of the New Left.

Sven-Ivan Sundquist, a former student who had taught me Swedish and who had become a financial reporter for *Dagens Nyheter,* made the arrangements for me. I declined his proposal that I meet with the prospective Prime Minister, Olof Palme. I thought Palme had better uses for his time than satisfying my idle curiosity. (We did have dinner with him some years later, but by then he was so thoroughly immersed in his political role that his conversation was too formulaic to be very interesting.) In addition to the environmentalists, I arranged to meet with a New Left writer, Sven Fagerberg, and with Jan Myrdal, the Maoist son of Gunnar and Alva Myrdal.

To indicate the state of my political mind at that moment, I will quote just a few sentences from a long letter I wrote shortly before the trip to Sven-Ivan. (I am shocked by its conservatismbut then I was dealing with Swedish Socialists and was probably just trying to work myself up to a lively argument with them):

> I can only explain to myself the curious (in my view) social priorities that the New Leftincluding our American studentsassign by assuming that the leaders of the New Left are dominated by two unpleasant and undesir-

able human motives: envy and a lust for power. . . . It is time that we go back to Locke and Jefferson to understand what they thought democracy was all about. To them, distribution of power was not an end, but a means. And it was not a means to giving the State the task of regulating matters for the collective good, but a means of protecting individuals in their domains of private autonomy from interference by other individuals *and by the State. . . .* The malaise of the left . . . is that they have a single medicineState actionwhich is no longer relevant to the ailments we have.

Words fit for a libertarian, which I certainly was not, but guaranteed to liven up the conversation. Knowing that I would be questioned about American minority problems, I also boned up on the Swedish minority problems (Lapps and Gypsies mostly at that time). During that whole period, I found that political conversation abroad became much more coherent when I could provide comparable examples from my host country of the American problems that were in the news. By that means, one was often able to substitute intelligent conversation for America-bashing.

(At this point I must interrupt my Swedish narrative to recount a story of our 1970 visit to Cuzco. I struck up a conversation with our taxi driver, who was also a teacher [a salary he couldn't live on], and naturally a Marxist. He began to question me about the American race problem, whether we had blacks in our university. A few, I told him, but there were not many who could qualify. I was a little embarrassed to have to give that answer. Just then, I happened to glance at him and saw his perfect Inca profile. I said, "Suppose you had a brother, and your brother went to Lima to study at the university. Would he have any problems?" A gleam of understanding passed over my driver's face, and then we had a very sophisticated conversation about the problems of ethnicity and how to combat them.)

I learned little from Fagerberg that was new about the New Left. I had already read several of his books. But the meeting with Jan Myrdal, and his significant other, was fascinating. Myrdal had written an excellent book, *A Chinese Village,* about peasant life in the People's Republic of Chinawith a distinct Maoist slant but descriptively accurate. That is, it sounded just right for peasants. More recently, he had been in Albania. Without blinking, he told me, "Some of the peasants had poor land in the mountains, others much better land in the valleys. So they held a great meeting, and they all agreed voluntarily to make exchanges in their lands, so they all would be equal."

Now here I applied the Travel Theorem. I had never been in Albania, but I had encountered peasants, in the United States and Mexico, and even in Germany and France. And I had read accounts of many peasants in many

lands at many points in history. The prior probability of Myrdal's statement was far too low to be rescued by a single eyewitness account.

During our conversation, Myrdal's friend (I am sorry that I have not preserved her name) was becoming steadily angrier. Finally, she burst out, "You Americans think you have the right to visit another country and force everyone to speak English with you. You are intolerable." My verbal command of Swedish allowed me to apologize for being an ugly American, but not to carry on a complex political conversation. We did not get much farther.

Again the Travel Theorem was vindicated. I had learned much more about the Swedish New Left from my preparatory reading than from my visit. And the information in my books was probably more reliable than the oral testimony. But I had a couple of lovely weeks in Sweden, a beautiful country with most attractive people, to which I return from time to time with great pleasure.

Japan

As Sweden and Japan were the first two foreign countries to send considerable numbers of graduate students to GSIA, it is not surprising that they were among the first with which I acquired more than a passing acquaintance. In the course of my career, I have worked closely with many Japanese doctoral and postdoctoral students, with several of whom I retain close ties. Takehiko Matsuda played a leading role in introducing management science techniques into Japan, and served as president of Tokyo Institute of Technology and, more recently, Sanno University. Albert Ando, now an economics professor at the University of Pennsylvania, shyly handed me, as his parting gift, a Japanese grammar, hinting (only implicitly, you may be sure) that by mastering it I could become a civilized person. Yuji Ijiri co-authored a book with me (Ijiri and Simon 1977) and remained at GSIA to become one of this country's outstanding accounting theorists. A postdoctoral student named Yucho Anzai is today providing leadership in Japan in cognitive psychology and artificial intelligence. I have also kept in touch with two recent doctoral students, Masaru Tomita and Yumi Iwasaki.

I began to study Ando's grammar book seriously when I decided to visit Japan in 1969 to attend a business conference and to give lectures on organization theory. Today it seems ironic that I, along with many other Americans, was expounding the principles of management to the Japanese at a time when Japan was already far along in producing its economic miracle and demonstrating the competence of its managers. Fortunately, I never had exaggerated ideas about my ability to assist managers working

in cultures I did not understand, but if the Japanese were willing to listen, I was willing to tell them how Americans managed. Caveat emptor.

The 1969 visit to Japan produced an interest, a respect, and a pleasure that has taken me back there about a dozen times. Two pieces of good luck prior to the trip contributed to the positive experience. First, I discovered some Japanese primers that introduced the characters very gradually, and that followed the standard primary school textbooks in recounting the traditional stories and even songs of Japan. Second, I found and read Oliver Statler's delightful *Japanese Inn* (1961), which provided not only a manual of conduct in those beautiful, traditional establishments, *ryokan,* where one can experience much of Japanese culture but also a first course in Japanese history. Dorothea and I decided from the beginning that, wherever possible, we would stay in *ryokan*.

In Tokyo, the Fukudaya became our principal home. Only a block from the New Otani hotel in the Yatsuya district, it shut out Tokyo completely once you entered its gates and placed you in an older, gracious Japan of paper screens, gardens, and kimono-clad servants. It was, and is, a favorite place for important political negotiations. And once, as a special honor, we were shown the "Go" board on which national championships are played, a great, polished block of wood large enough to serve as an executioner's block.

Because the Japanese do not distinguish between their people and their society, they find it almost impossible to make a foreigner a genuine part of their community. The *ryokan,* perhaps because it separates its guests from one anotherthere are no public rooms, and one dines in one's own roomprovides a unique path into Japanese culture. Of course, it is almost necessary to speak at least some Japanese, else your maid, the *jochu,* could not cope with you or care for you. Most Japanese are incredulous that foreigners can survive in *ryokan,* and respond warmly to your desire to do so.

We have had marvelous times in Kyoto, with its gardens and temples; in the spa of *Kami no yama* ("mountain of the gods") in northern Honshu; in Sendai, overflowing with people, even by Far East standards, at the time of the Tanabata Festivalwhen the weaver and the plowboy, two constellations on opposite sides of the Milky Way, get to spend their one night a year together. (We created something of a sensation in a Sendai bar when we struck up the Tanabata song, learned from my primers.)

We spent a week with my former postdoc Yucho Anzai and his wife Toko on the coast of the Japan Sea, with its potters and paper makers. We learned of a theater in Tokyo that played the traditional No dramas, and participated in a *Bon* festival at a temple near Ueno Park. Not quite Japanese, we have

nevertheless been able to come close to the Japanese culture, to enjoy its beauties, as well as to see aspects of it that seem to us archaic and crude (the social roles of men and women being perhaps the most striking example of the latter).

Our entré to Japan was much aided by my professional and academic connections, especially with the members of the Organizational Science Society, led by Susume Takamiya, who introduced us to the Fukudaya on our first visit. In recent years, I have also come to know the community of Japanese computer scientists. True, we could have learned about Japan with less trouble in the San Diego Public Library, but I don't think we would have found a *ryokan* there.

THE FOURTH PANEL
RESEARCH AFTER SIXTY

Chapter 21
From Nobel to Now

More than ten years after the award of the Nobel Prize, people, on meeting me, still congratulate me, as though they have been remiss in holding silence for a decade. Frequently, they ask one or the other of two questions: "When did you first suspect that you might win it?" or "What change has it made in your life?" To the first, I usually answer, "The day I got it"; to the second, "No change." Neither answer is strictly truthful but each avoids the tedium of a lengthy reply (as rude as telling the whole truth when someone asks after your health).

The Nobel Prize in Economics*

The addition of economics to the Nobel Foundation's short list of honored disciplines was received in the natural sciences with a certain amount of shock. I was serving on the President's Science Advisory Committee in 1969 when the first economics award was made. At the next PSAC meeting, we were busy congratulating our fellow member Murray Gell-Mann, who that year had received the prize in physics and had just returned from Stockholm. Some members asked about how the economists (Ragnar Frisch and Jan Tinbergen) had been fit into the ceremony. Our chairman, Lee DuBridge, with anguish in his voice, spoke up, "You mean they sat on the *platform* with you?"

* The official name of this prize is "The Prize in Economic Sciences in memory of Alfred Nobel," but I will call it by its common name. It was endowed by the Bank of Sweden and is awarded by the Royal Academy of Sciences, following the same procedures as for the physics and chemistry prizes.

Earlier that same year, Carnegie Mellon had held a joint seminar with faculty members of business schools in Scandinavia, conducted in Aspenäs, Sweden. Walter Goldberg, a business economist of Gothenberg University, was the prime mover. One sunny afternoon during the seminar, we took a break from our discussions to gather on the lawn for academic gossip. One topic was the newly announced and not yet awarded economics prize. Who would win it? At some point in the conversation, Walter Goldberg turned to me and said, "You will receive the prize within ten years." I expressed and felt incredulityhowever much I secretly believed I merited such an award, economists did not regard me as an economist; and bounded rationality seemed to be dying a quiet death, in the United States at least. The neoclassicists clearly had won the day.

A Nomination

A year or so later, Walter asked me to send him a statement of my contributions to economics that could be used in preparing a nomination. I sent it to him promptly. Neither false nor genuine modesty inhibited me in writing it. It covered about twelve single-spaced pages. I tried to state my claims accurately but not flamboyantly. On the basis of the work Walter then did in documenting my nomination, I became a feasible candidate.

Seeds of desire, once planted, do grow, even in rather stony soil. I never lost sight of Walter's prediction, and took more than casual notice of each year's announcement of the Nobel winner for economics. The choices, year by year, seemed to me appropriate but orthodox. Quantitative and mathematical contributions were well regarded by the selection committee, which evidently was trying to distinguish economic science from economic policy or journalism.

At that time of my life, computer simulation of human thinking and associated experimental work were absorbing almost all my research time and energy. I was also heavily involved in Washington science politics. I had little time to think about economics or to try to push forward my ideas of bounded rationality or the behavioral theory of the firm. I did continue, with Yuji Ijiri, to develop models of business firm size and growth. That was nearly, but not quite, the extent of my involvement with economics.

From time to time, I received invitations to participate in meetings on topics in economics, mostly in Europe. The seed having lodged, I was not unaware that increased visibility among economists would strengthen my candidacy for the prize. That led me to accept several invitations I would otherwise have declined, the first being an invitation to participate in the celebration of the twenty-fifth anniversary of economics at the University

of Groningen, in 1973. For that occasion, I wrote the first version of "From Substantive to Procedural Rationality," but the conference proceedings were not published.

When in 1973 I was also invited to Rotterdam to receive the degree of Doctor of Economics from Erasmus University (which had absorbed the celebrated Netherlands School of Economics), I began to see, or imagine, a pattern. Was all of this attention from the Europeans a coincidence, or was an invisible hand organizing it? I observed that my sponsors in both Sweden (Lund in 1968) and the Netherlands came from the business economics sections of their faculties, not the political economy sections. Evidently I was a stalking-horse for the claim that the Nobel Committee's definition of economic science should encompass business economics.

I next accepted an invitation to attend a philosophy of science symposium in Navplion, Greece, in the summer of 1974, organized by Imre Lakatos and Spiro Latsis. There I presented (and subsequently published) a revised version of the paper on procedural and substantive rationality (Simon 1976). I also renewed my acquaintance with John and Ursula Hicks and met Lionel Robbins. Lord Robbins reminded me of all the things I disliked in the arrogance of economics and its detachment from facts. Irritated by his pomposity, I replied to one of his discussion comments in a distinctly insulting way, for which I was later scolded (privately) by John Hicks. That incident was not descriptive of the Navplion conference, which was extremely pleasantgiving Dorothea and me a chance to explore Greecebut otherwise uneventful.

From 1969 through 1973, the prize had gone to Frisch and Tinbergen, Paul Samuelson, Simon Kuznets, John Hicks and Kenneth Arrow, and Wassily Leontief. I did not see how I could have taken precedence over any of these economists. Evidently the committee did not see it either. Then Gunnar Myrdal and Friedrich von Hayek received the prize in 1974, Leonid Kantorovich and Tjalling Koopmans in 1975, and Milton Friedman in 1976. Again, no great surprises. Although Myrdal, von Hayek, and Friedman were associated in the public mind with strong policy positions, economists knew them as fellow professionals who had made important technical contributions. Eight of Walter Goldberg's predicted ten years had passed.

The year 1976 brought a more surprising event: my election as a Distinguished Fellow of the American Economic Association. In view of my inactivity in the association (in fact, I had never been even a member), I had to suspect that my selection was another step on the way to a Nobel nomination. At the AEA national meeting, where I accepted the award, Albert Ando hinted as much. Kenneth Arrow, evidently the moving spirit behind my nomination for the AEA election, had to educate the younger economists

on the selection committee on who I was and on my standing as a Fellow of the Econometric Society.

By 1977, rumors were circulating that I was on the "short list" for the prize. My colleagues at Carnegie Mellon were invited to submit statements to the Nobel Committee, and they also organized a symposium in my honor soon after I had passed my sixtieth birthday. This indication of their friendship and esteem pleased me, especially since my views were so distant from the neoclassical views that were held almost unanimously by the economists in the Graduate School of Industrial Administration.

At the same time, I experienced a slight twinge when several of the papers at the symposium provided neoclassical explanations for phenomena I thought I had explained quite satisfactorily within the framework of bounded rationality. Bob Lucas's paper on salary size distributions was the clearest example, and we got into a lively debate during his presentation. But I also felt satisfaction that my colleagues respected me without feeling the need to agree with me; I could certainly not be accused of being surrounded by disciples. Some of the symposium papers were published in a special issue of the *Bell Journal of Economics* (Prescott 1978).

In 1977, the Nobel Prize for Economics was awarded to Bertil Ohlin and James Meade. The announcement was made during the week of my Festschrift party, but I had no great difficulty concealing my disappointment; somehow, it still seemed improbable that a committee of economists could put me at the head of the list.

In the same year, Jascha Marschak, as president-elect of the AEA, was chairman of its program committee. He invited me to deliver the Ely Lecture (which I would have done for Jascha even if I were not campaigning). Jascha's death later that year was a sad event, and even a guilt-rousing one, for he had now been denied the prize that he surely deserved ahead of me.

Jascha had planned to chair the Ely Lecture session himself. Someone (perhaps with imagination and a sense of humor) decided that Milton Friedman should replace him in that capacity. My lecture, "Rationality as Process and Product of Thought" (Simon 1978b), which developed further the theme of the Navplion paper, was not at all to Milton's liking. During the discussion following the talk, unable to maintain a chairman's neutrality, he engaged in debate with me. But, still mindful of his duties to the speaker, he was not his usual freewheeling self, and I distinctly had the better of the exchangesomething that would have been problematic if Milton, famous for his debating skills, had not had one arm tied behind his back. (I don't know whether any member of the Nobel Committee was in the audience, or whether the story got back to any of them. It could not have hurt my candidacy if it had.)

The Award

October 15, 1978, was a Sunday. A week or two earlier, my name had been printed in a Swedish business magazine as one of the short-list candidates for the 1978 Nobel Prize, and I opened my newspaper each morning that week to search for the verdict there. On Sunday afternoon, I received a phone call from my friend and former student Sven-Ivan Sundquist, who had arranged my Stockholm visit in 1969. Sven reported that he had met a member of the Nobel Committee on the street that day, who told him he would not be disappointed by that year's award. After considering what that might mean, he decided that I would be the winner and called to alert me.

Needless to say, I found myself a bit tense and exhilarated during the rest of the afternoon, and made plans to arise early the next morning in case a phone call came from Stockholm after the academy meeting that was to end there about noon, Stockholm time. When the call came, at 6:00 A.M. on Monday, I was already up and dressed.

You are all familiar with the obligatory responses to the obligatory questions that reporters put on the morning of the announcement: "How do you feel?" And the invariable response: "Ebullient!" The words vary, but that's the common meaning. There is the immediate excitement and almost disbelief of the first day, interspersed with congratulatory phone calls, then followed by telegrams and letters from a thousand friends, far too many to answer individually.

These messages from the present and the past evoke the warmest glowa euphoria at being enveloped in a great shoal of humanity, all sharing the victory and the pleasure; and a nostalgia for the good times you have had with these friends. That was the best thing of all, and unexpected in its intensity.

Then there is Nobel Week in Stockholm in early December. Kathie, Peter, and Barbara joined Dorothea and me for the celebration, which is carried on in great style. The award ceremony itself, the great banquet in the City Hall, the ball at the Palace, the wake-up serenade in one's hotel room on Santa Lucia's Dayall sustain the fairy-tale quality of the week, complete with royalty.

The laureates have speaking roles on only two occasions: the Nobel Address (mine was given at the Stockholm School for Economics) and the banquet, where one awardee speaks for each prize. On the latter occasion, I could not resist showing off my Swedishit seemed somehow unfair that our hosts should be addressed in a foreign language. The theme of my brief remarks (one was allowed no more than a minute or two) was the impor-

tance of bringing the social and natural sciences to bear jointly on our great social problems. But the scene was stolen by another awardee, Isaac Bashevis Singer, who gave four reasons why he writes in Yiddish, "a dying language" (for example, because he likes to write ghost stories), and ten why he writes for children (for example, because they read books, not reviews).

A Slight Bend in the Road

As for the second frequently asked question—Did the prize change my life?—I have already answered one part of it. Anticipation of the possibility of the prize (campaigning for it, to put it bluntly) did change my life a little. I devoted perhaps 5 percent more of my total energy to economics than I otherwise would have done—not a wholly wasteful pursuit, for it forced me to rethink the case for bounded rationality. That case, at least as presented in the economics literature, had been a largely negative one, an attack on the veridicality of neoclassical theory without much more than hints about how to replace it. The distinction between procedural and substantive rationality, which I then began to develop, provided an opportunity to sketch out positively the (psychological) theory of procedural rationality.

In addition to the Navplion and Ely papers, I wrote another in the same vein for my Festschrift celebration: "On How to Decide What to Do" (1978a). In all of these papers, I tried to show that economics has to be concerned with computation—with the processes people actually use to make decisions—and has to describe the nature of these processes.

By the time the prize was awarded, therefore, I was already engaged in renewed polemics with the economics profession. A Nobel award is, as Teddy Roosevelt found the U.S. presidency to be, a "bully pulpit" for presenting one's ideas. I used the occasion of my Nobel address in Stockholm to publish an extended review of the theory of bounded rationality. Then, in "On Parsimonious Explanations of Production Relations," published in the *Scandinavian Journal of Economics* (Simon 1979b), I attacked the claims that neoclassical theory was supported by extensive empirical evidence, proposing alternative explanations for some of the more important phenomena that neoclassical theory had claimed to account for.

It has been said that young people who receive the Nobel Prize may become unproductive, being unable to resist the new demands made on their time. Since I had been notorious for some years before the prize came, I had long ago learned many ways to decline requests, and hence that was not a major problem for me. I simply had a bigger menu to choose from, and had to decide how much of my effort should be devoted to economics.

Certainly more has been allocated to it than would have been if I had not won the prize. I have not undertaken new empirical research in economics, but have focused upon methodological issues. In addition to developing the distinction between procedural and substantive rationality, I have been following through the implications of two other ideas.

One idea, first developed for a symposium in Sweden in the summer of 1983, is that most of the conclusions that neoclassical economists draw do not depend on the assumption of perfect rationality but derive from auxiliary institutional assumptions that are required in order to reach any conclusions at all. Much the same conclusions can be reached from these auxiliary assumptions, with fewer mathematical pyrotechnics, by assuming the actors are satisficing rather than optimizing. This observation shifts attention from the unrealistic and superfluous postulates of optimality to the auxiliary assumptions that are doing the real workand that need empirical testing.

The second idea, which emerged during the summer of 1988 while I was attending a symposium in Siena, Italy, on the new institutional economics, is that we should try to reconstruct economic theory around the concept of organizations rather than the concept of markets, as the former play a much larger role in a modern economy than do the latter. Development of this idea revealed that the new institutional economics relied much more than it should upon neoclassical reasoning.

It would be hard to say how much of my new activity in economics was motivated by the prize and the opportunities it provided to be heard, and how much by the many current signs of restiveness in the profession and widespread dissatisfaction with the orthodoxy of neoclassicism. Whatever the motivation, I now spend perhaps 10 percent of my research time thinking about the application of bounded rationality to economic theory. To that extent, the prize has affected my scientific life.

Econometrics and the Prize

My being chosen for the prize occasioned some astonishment. Many economists and most media folk thought I was an outsider, an unknown who had been selected by some fluke. Those who held that view were simply ignorant of the sociology of the economics profession. They were evidently also unaware that, according to a study published a few years previously, I had been the fifth most frequently cited economist (in leading economics journals) during the 1950sthe period when I was devoting substantial time to economic research.

The most salient fact in the postwar history of economics was its sudden

conquest by mathematics and statistics. In 1950, it was still difficult to get a paper published in the *American Economic Review* if it contained equations (diagrams were more acceptable). The Econometric Society had been founded a quarter-century earlier as a meeting ground for mathematically inclined economists, and *Econometrica* as an outlet for their work. I think it could be said that by 1970 mathematics had taken over economics (for better or worse); the simplest theory had to be clothed in mathematical garb before it could receive any serious attention, and "empirical work" was synonymous with "econometrics."

It is perhaps not too disrespectful to label the people who brought about this revolution the Econometric Mafia. Who were they? If you examine the list of Fellows of the Econometric Society in 1954, fifteen years before the first Nobel Prize in economics was awarded, you will find the names of 20 of the first 27 prizewinners. Three others (Bob Solow, George Stigler, and Leonid Kantorovich) became Fellows later, but well before they won the prize, leaving only Ted Schultz, Sir Arthur Lewis, James Meade, and James Buchanan off the magic list. (I was elected a Fellow in 1954.) Since the list of Fellows in 1954 contained only about 120 names, of whom not more than 80 were still living in 1969, the first year of the prize, a historian of science might take this record as evidence for an invisible college that had a major influence on the Nobel nominations and selections.

So, far from being an outsider, I was a duly certified member of the Econometric Mafia, much better known to my fellow members (many of whom had meanwhile received their awards) than to the rank and file of the profession. I knew personally about half of the 1954 Fellows, and among these friends and acquaintances were nearly all the previous prizewinners, in addition to such figures as Marschak, Trygve Haavelmo, Gerard Debreu, Larry Klein, Franco Modigliani, Oskar Morgenstern, Jim Tobin, Stigler, and Solow, most of whom I saw frequently in the 1950s and 60s, and all but two of whom also subsequently won the prize. All of them were well acquainted with my work in economics.

Then I must add two additional names. One member of the Nobel Committee in 1978 was Herman Wold, who had done work on causality closely related to my own. (We had even disputed politely about it.) A second member, Sune Carlson, had written his doctoral dissertation at the University of Chicago on a topic in management (on how executives use their time), hence was well qualified to evaluate my record. It was he who wrote my citation and read it at the Nobel ceremony.

If I was an outsider to the economics profession as a whole, I was an insider to its elite. Without that accreditation, I suspect that I would not have won the prize.

Research Themes into the 1980s

Most of my research in the decade after receiving the Nobel Prize continued to be directed toward cognitive science, often pushing forward along the paths marked out earlier. In 1989, I published a second volume of *Models of Thought,* a collection of papers in psychology mostly completed subsequent to the appearance of volume 1 in 1979. It was almost exactly the same length as the previous volume (500 pages), and the research had followed sufficiently similar lines that I could arrange it within the same categories that were used in that earlier volume. Some familiar programs (especially EPAM and GPS) reappeared, as did some familiar problem-solving tasks, especially the Tower of Hanoi.

The Tower of Hanoi is a puzzle of Chinese origin involving a pyramid of disks impaled on one of three vertical pegs. The task is to move the pyramid to one of the other pegs, moving only one disk at a time and never placing a disk atop another that is smaller than it. For many years the Tower of Hanoi has served as a laboratory task for research in problem solving, and Dick Hayes, Ken Kotovsky, and I have used it extensively. If chess plays the role in cognitive research that Drosophila does in genetics, the Tower of Hanoi is the analogue of *E. coli,* providing another standardized setting around which knowledge can accumulate.

Using these well-tried tools is fitting. Old dogs should not be learning new tricks after their sixty-second birthdays. But my research during the decade did, in fact, take some new directions, although these are only partially represented in the new *Models of Thought*. One of the principal novelties was experimental work with my Chinese colleagues on short-term memory and the magical number seven. Here we had some success in reconciling apparently conflicting memory models that had been proposed by George Miller and Allan Baddeley, respectively.

There is a new emphasis in volume 2 upon learning processes, especially on how students can learn from worked-out examples and how this process can be modeled by adaptive production systems. Some of that research was carried out with my Chinese colleague Xinming Zhu. There is increased emphasis, too, on the role of visual imagery in thinking, and the mechanisms underlying it. Finally, there is attention to scientific discovery, as represented by my joint work with Deepak Kulkarni on KEKADA, a program that designs sequences of experiments, adapting each new experiment to the findings of the previous one (Kulkarni and Simon 1988).

The research strategy was the one we have followed through the whole history of A.I. and cognitive science: As we gain an understanding of simpler processes and task domains, we tackle more complex ones. So, in the past

ten years, I have moved on to learning processes; to problem representation, including imagery; and to the processes of scientific discovery.

In addition, Anders Ericsson and I reviewed what was known about thinking-aloud protocols and developed a theory of what can and cannot be learned from them about thought processes. Our work on thinking-aloud protocols became a book, *Protocol Analysis,* published in 1984. Also not reported in *Models of Thought,* research in collaboration with Pat Langley, Gary Bradshaw, and Jan Zytkow over the whole decade produced *Scientific Discovery* in 1987 (Langley et al. 1987).

New Architecture for Old

In cognitive science there is currently a preoccupation with questions of general cognitive architecture, which I do not share. There are great debates about whether the human mind is to be modeled by SOAR (Allen Newell), Act* (John Anderson), connectionist nets (Jay McClelland), or something else. I have been more interested in what Bob Merton has called "theories of the middle range" programs such as GPS, EPAM, the sequence extrapolator, and BACON, which simulate human behavior over a significant range of tasks but do not pretend to model the whole mind and its control structure.

It is not that I regard the broader architectural issues as unimportant; but, even when solved, they do not explain how very general schemes are adapted to perform particular classes of cognitive tasks. The architectures have almost more the flavor of programming languages than of programs. Hence, I do not believe that the more specific programs for particular task domains or ranges of such domains will be displaced when the "right" general architecture is discovered. They will simply become essential components in the larger system.

Parallel or Serial?

Another new development in cognitive science that I have watched with interest, but from a distance, is the vigorous activity in constructing simulations of intelligence that employ "neural networks" or other highly parallel architectures, instead of the serial symbolic systems that we have used in our work. My view, which I won't defend here, has been that these "connectionist" architectures have a role to play (for instance, in simulating visual and auditory sensory processes), but that they will not replace physical symbol systems as models of higher mental processes.

This challenge to our basic approach from the devotees of the new parallel systems has placed me in the unprecedented position of being a conservative of the Old Guard instead of a rebel. I am now for the first time learning how it feels to be a target of the attacks of Young Turks, to have one's cherished beliefs challenged and the permanence of one's life work threatened. So far, I have not felt any painful anxiety, perhaps because I am not convinced that the ramparts will crumble.

On several occasions, however, I have examined the connectionist claims by comparing the behavior of the new parallel systems with the theories, and especially EPAM, that Ed Feigenbaum and I had earlier proposed to explain the same phenomena. In 1984, we published a general defense of EPAM as a plausible theory of recognition processes. And in 1989, Howard Richman and I showed that EPAM could account for a range of phenomena that had been proposed as critical evidence for the necessity of a parallel architecture. I expect that there will be more work of this kind to do. And it is useful work, for it is only through this kind of detailed comparison that we will gradually understand how these various mechanisms complement one another.

Theories of Learning

The new parallel systems have capacities for learning, but there are alternative ideas, within the paradigm of symbol systems, about how learning takes place. The idea of using adaptive production systems (production systems that reprogram themselves) as a means for learning from examples was developed by David Neves in the 1978 thesis he wrote under my supervision. Yucho Anzai and I carried on other explorations of these ideas. I was very pleased when Xinming Zhu, in the Chinese Academy of Sciences, began testing their applicability to secondary school instruction in algebra and geometry.

By 1987, Zhu and his Chinese colleagues had carried a class, in two years, through the entire standard three-year math curriculum in the Chinese middle schools, substituting studying examples and working problems for lectures by the teacher. The students scored slightly higher, both at the end of the course and a year later, than students in a control group.

When Professor Zhu spent a year with me in Pittsburgh, we tried to get comparable experiments started here, but succeeded on only a small scale. It turns out to be easier to arrange educational experiments in China than in the United States. In China, you need only the permission of the Minister of Education. In the United States, you need the permission of teachers,

students, parents, principals, superintendents, and our university committee for the protection of human subjects. (I am not complaining, just observing.)

For a long time I have thought that the presence of large numbers of intelligent mathematical illiterates (college graduates who say, "I never could do mathematics") in a high-tech society creates a whole class of alienated intellectuals and poses a serious long-term problem for democracy. (I am merely echoing C. P. Snow's argument in *The Two Cultures and the Scientific Revolution*.) Understanding this problem has been a major hidden objective in my research on learning.

How we should deal with the problem hinges on the answer to an empirical question. To what extent is the mathematical illiteracy of otherwise highly intelligent people a matter of genes, and to what extent is it remediable by appropriate education (and reform of attitudes)? I want to understand the cognitive roots and mechanisms of mathematical competence and incompetence. There is no question I would more like to answer than this one before my research career ends.

Scientific Discovery

The major focus of the new work of the 1980s has been the simulation of scientific discovery. "Creativity" is always the last refuge of the skeptics of artificial intelligence. In simulating creativity, it was important to use tasks that could not be dismissed as trivial or humdrum. Therefore, our research team (Langley, Bradshaw, Zytkow, Kulkarni, and 1) took our experimental tasks from great moments in the history of science: Kepler's discovery of his Third Law of planetary motion; Ohm's law of electrical conduction; Dalton's theories of chemical reactions; the discovery of atomic and molecular weights; the conflict between the phlogisten and oxygen theories of combustion; Krebs's explication of the synthesis of urea in living organisms.

By using these actual (and important) historical cases of scientific discovery, we removed the difficulty often attending research on creativity, that what is studied is not creativity in any important sense (for example, measuring creativity by how many uses a subject can find for a brick). The research reported in *Scientific Discovery* (Langley et al. 1987) by no means completes the job of explaining the processes of science, but it takes several long steps toward that goal and sharpens the questions that remain. It supports strongly the proposition that scientific discovery is achieved by the normal problem-solving processes that have been observed in less formidable problem domains.

Mental Imagery

During the past decade I also made progress, especially in collaboration with Jill Larkin, toward understanding how mental images (and drawings on paper) are used in thinking and problem solving. That question has been merging more and more with the question of how problem solvers form their representations of problems, and these combined questions provide a central focus for much of my current research. Learning, imagery, and problem representation have flowed together into a single system of highly interrelated processes.

Labyrinths or Logic?

The study of representation inevitably leads back to another question that has divided the artificial intelligence (and cognitive science) community from the beginning. I'll put it in terms of competing metaphors. Is thinking best viewed as a process of reasoning from premises, using the metaphor of logic, or as a process of selective search through a maze? Early A.I. research relied largely on the maze metaphor, but cognitive science research stemming from linguistics and logic preferred the reasoning metaphor. The maze, of course, has been the metaphor of my own research.

In recent years, the logic metaphor has been attracting increasing numbers of adherents, and I have become involved in the dispute. It is another important item on the current research agenda that is leading me into interesting new regions, including literary criticism and perhaps hermeneutics (if I can ever find out just what that word means).

So many paths to explore. So little time.

Developments at Carnegie Mellon University

Under Dick Cyert's leadership, Carnegie Mellon achieved a steadily growing distinction among America's research universities in the 1980s. I have already described many of the ventures that provided the foundations for that progress. The greatest asset of the university has been its capacity for innovation. That capacity, in turn, rests partly on its traditions of small size, weak interdepartmental boundaries, and solid adminstrative support (or at least hunting licenses) for entrepreneurial undertakings. We measure our success not only by the quality of teaching and research on our own campus,

but by our influence on intellectual and educational trends in the nation and internationally.

We believe (and the world agrees) that we have had major, broad impact on education and science through the Carnegie Plan, our conceptions of management education in GSIA, our School of Urban and Public Affairs, cognitive psychology, computer science, the rhetoric and writing programs in the English Department, the renaissance of engineering design, and other programs that I have not had occasion to talk about in my narrative. We believe that the future health and success of the university depends on retaining that innovating role.

The great new venture of the decade was a leap forward to bring the whole university firmly into the computer era. The goal was to provide a university network tying together all the local computing resources, accessed by students and faculty through powerful individual work stations. This is not the simple battle cry of "Every student with a computer"; the key to the educational power of the scheme is not PCs for students, but general access to the pool of common resources and full communication among the members of the community.

Carnegie Mellon, with its strong Computer Science Department and sophistication in computers, decided that it should try to lead the way nationally in these developments. I had little to do with the decision (and might have been afraid to make it had I been president). Some of the principal actors have been Dick Cyert himself, Allen Newell, and Pat Crecine, when the latter was Vice President for Academic Affairs.

We are still far from the end of the experiment. The hardware companies have not yet provided the low-cost work stations that will eventually be needed. We are only beginning to develop the requisite educational software and an adequate institutional structure within which that software can be created. We have had modest success in studying the social changes that are occurring in the university community under the influence of the computers. Great and exciting tasks lie ahead as we push forward on all these fronts.

I have played the role of elder statesman during this decade, somewhat reluctant to take a strong position on decisions whose final outcome will not be known until long after I have left the campus. The next generation should make those decisions. I do not say that I am uninvolved in campus affairs. In fact, the retirement of Dick Cyert from the presidency of CMU in July 1990 and the search for a new president has drawn me somewhat back into university affairs for a time. But I am not prepared to take the strong measures or issue the ultimatums that have sometimes been part of my style in the past. Some of my younger colleagues have not even seen

displays of my anger or whatever it is and imagine that I'm kind of a nice guy.

The Politics of Science

During the 1980s, I remained active in the affairs of the National Academy of Sciences, serving two three-year terms on its Council. The Committee on Science and Public Policy (COSPUP), of which I had been a member in the late 1960s, was revived, but with the inclusion of engineers, so that it was now COSEPUP: the Committee on Science, Engineering, and Public Policy. During most of the 1980s, I served on the reconstituted committee.

At the time of the reorganization of NAS to include social and behavioral sciences, there had also been created, under the aegis of its congressional charter, a National Academy of Engineering and an Institute of Medicine, to provide for greater participation of engineers and physicians, respectively. Under the same threat of separation that the behavioral and social scientists used to broaden their participation, the engineers and medical scientists had worked toward essential equality within what is now called the Academy Complex, a label that makes me shudder. There is today a troika, comprised of the presidents of the three component bodies, in which the president of the NAS is *primus inter pares*.

As part of the arrangement, the Engineering Academy (NAE) has essentially equal representation with NAS on COSEPUP. What problem do I see in that? The NAE represents the engineering community, which is very closely associated with the industrial community; whereas NAS is much more closely allied with the academic community. Advice from one of these groups is likely to reflect somewhat different values than advice from the other. Moreover, in terms of the sciences, NAE is drawn almost wholly from the physical sciences, and largely from physics and its applications. Hence, the representation of the various fields becomes correspondingly tilted in COSEPUP.

Since, to the general public, and to Congress as well, the Academy Complex means the National Academy of Sciences, there can well be misconceptions about the nature and societal affiliations of the advisers who produce the academy's reports. Anyone, for example, who regards the academic community as a partial counterweight in our society to the so-called military-industrial complex might well be surprised and concerned to learn that the Academy Complex is an amalgam of the two communities.

Because I think it important that the academic community have a separate and significant voice in science policy in our nation, I have opposed, as a

member of the Academy Council and in other capacities, and with no success whatsoever, the steps that have been taken over the years to create the present merging of academic, industrial, and medical communities into the Academy Complex. I do not agree that it is always desirable, as those favoring the changes think, that science and technology speak to Congress with one voice.

It does not seem to me at all harmful that different recommendations, each properly labeled, will sometimes come from the academic, the engineering, and the medical communities. If Congress has to weigh the advice of three academies that do not always agree, so be it. It is a proper function of democratically elected bodies in our society to choose among the experts in the light of what and whom these experts represent.

Since my preferred political style seems to be to work from within institutions rather than to fight them from outside, I have continued my activities, including my COSEPUP membership, in NAS. Perhaps the most useful function of our committee is to identify policy problems at an early stage, call attention to them, and see that we or someone else deals with them.

For example, COSEPUP issued an early warning about the problems arising from federal security controls over the export of high-tech products and knowledge. It has sponsored two committees, the so-called Corson and Allen Committees, to examine the academic and industrial components of the problem; and the reports of these committees, while not wholly enacted into policy, have played an important role in curbing excessive controls over international communication.

There has been a considerable continuity of my activities, both on the Carnegie Mellon campus and in Washington, from the decades before I received the Nobel award through the 1980s. If there was any considerable shift in the allocation of my time, it had to do with my increasing involvement with China and then the USSR. I am perhaps experiencing, however, a gradually increasing reluctance to engage in the arduous travels that are associated with my domestic and foreign political activities (as well as my numerous visits to university campuses), but no diminution at all in the excitement of my research. With a little more ability to say no, and with the aid and forbearance of my graduate students, I will be inclined to move more and more of my efforts into research.

Chapter 22
The Amateur Diplomat in China and the Soviet Union

My very first trip abroad with Dorothea, the visit to India in 1959 at the request of the Ford Foundation, was a "diplomatic" mission, to survey business education in India and make recommendations to the Foundation for a program to strengthen it. Subsequently, my foreign travels were all tourism and science. I attended a number of international meetings but took no active part in international scientific organizations, and did not otherwise engage in international science politicsuntil my love affair with China changed all that. It began in 1972 with a trip that had mainly touristic and scientific intentions, but most of my China travel took place in the 1980s. In this chapter I will discuss these trips as well as my more recent, post-glasnost, involvement with Soviet social science. These are my two most important ventures into amateur diplomacy.

China

I have visited China ten times and have spent more time therenearly a year in allthan in any other foreign country. I can best tell about my encounters there with the help of excerpts from some documents, and a little introduction and explanation for each one.

Chinamy Chinabegan in July 1972 when, carrying our bags, a little band of computer scientists and their wives stepped down from the train at the border between Hong Kong and the People's Republic of China and walked slowly across the bridge under the unsmiling stares of the soldiers, rifles at the ready, of the People's Liberation Army. Seated in a waiting room, complete with stuffed armchairs and racks of propaganda magazines in many languages, we waited for our train to Canton, were greeted by our

scientific and political hosts from the local science academy, and began our immersion in the blue-clad austerity of Maoist China and the nonstop loudspeakers of the Cultural Revolution.

I reported the story of our trip in *Items* (Simon 1973), a newsletter of the Social Science Research Council. Although we were in China for less than three weeks, nothing in the report was seriously misleadinga claim not all visitors to China of that time can make. The following account draws on that document.

Mao's China in 1972

What can one report about China on the basis of a nineteen-day visit? Very little from the travel itself, but a great deal from the library trips stimulated by the upcoming journey. I testify here as a China Expert Twice Removedonce removed because my expertness derives from reading the works of China Experts; once again removed because most of those experts are themselves Experts Once Removed, veterans of painstaking China watching from Hong Kong, Taiwan, Tokyo, or Ann Arbor.

But I am exaggerating my remoteness from the facts. Mao's *Little Red Book* is as much a fact of China as is a stone of the Great Wall. It is available at your local library, as are other relevant documents, as well as eyewitness accounts of recent China by perceptive observers. I am much better off than an archaeologist, who has only bones and physical artifacts to go by in reconstructing a civilization. I am as well off as a historian, with whom I share those most important artifacts of all: the words that members of a society use to communicate with each other.

I have even further qualifications as a China Expert. Observations, to produce facts, must be skilled observations by qualified observers. The description of a moon rock by a layman produces very little, if anything, in the way of fact. Only a geologist can extract a fact from a rock. Only a social scientist can extract a fact from a social artifact or a social communication. Hence, I am a Qualified Observer of facts about China. What I see and, more important, what I read will be sifted through the mesh of theory that I hold in my mind, will be winnowed.

The circumstances of that first trip were as follows. In 1971 some computer scientists of my acquaintance agreedstirred by the table tennis match in Pekingto try to arrange a scientific interchange between American computer scientists and their counterparts in the People's Republic of China. Two of them volunteered to visit the Chinese Embassy in Ottawa, where the proposal was received politely but coolly. Nothing more was heard through the subsequent period of the Kissinger and Nixon trips. In the

middle of April 1972, a cordial invitation was received for six of us (of our own selection) and our wives to visit China as guests of the People's Republic. The lucky six were Severo Ornstein of Bolt, Beranek and Newman, Inc. (who had made the initial contacts and served as head of the delegation), Thomas Cheatham of Harvard, Wesley Clark of Washington University, Anatol Holt of Massachusetts Computer Associates, Alan Perlis of Yale, and myself. All six, and two of the five wives who completed the party, were experts in computer science; only my wife and I also had social science training.

The trip gave us some nineteen days in China, spent mainly in Canton, Shanghai, and Peking, with plane transit between those cities. We saw rural China from the ground only on the train from the border to Canton, and in brief excursions by automobileamong them, trips to a commune, to the Great Wall, and to a temple in a city near Canton. We were free to wander unescorted in the cities, and two of us who had enough rudiments of spoken and written Chinese to read, ask directions, and shop made extensive use of that opportunity. About half the time was spent in working sessions with Chinese computer scientists, in lectures (by both sides, but mainly by us), and in smaller discussion groups. Only two of the scientists we encountered (and I think none of our other hosts) spoke usable amounts of English, so our communication (except in our urban wanderings) was mediated entirely by the half-dozen interpreters who traveled with our group.

The hospitality was overwhelming. In spite of, or because of, our unofficial and apolitical status, our accommodations and travel arrangements were excellent, red tape was absent, and the food was exceptionally good. (Some of our compatriots soon backslid to eating Western breakfasts, but I enjoyed my morning soup and dumplings through the final day.) We were dually hosted by the Chinese Agency for Travel and Tourism (CATT) and the Chinese Academy of Science.

Evidently, CATT and the academy reached no agreements before our arrival on how we would spend our time. This was the subject of much negotiation with the CATT representatives, who (we thought) wished to take us to the usually visited monuments of the revolution (such as Yennan and Mao's birthplace), with only token and ceremonial meetings with scientists, while we opted for a heavy work schedule.

The CATT official doctrine was: "In China, we believe we must meet several times before we become friends," which we countered with, "We Americans are queer, hasty folk. We say that people become friends by working together." In the end the work ethic triumphed, but at the sacrifice of the visits to Yennan and Shaoshan. We believe our stance was appreciated

by the Chinese scientists and came to be respected by the CATT administrators.

The interchanges with our colleagues in computer science were extensive and meaningful. We found an impressive computer technology in China, lagging, perhaps, some four to six years behind ours, but roughly comparable in quality to the Russian technology. We saw both the computers and the factories in which they were made. The computer scientists were well read in Western literature, but no foreign technicians or imported hardware were in evidence. Although its factories were small, China appeared to be producing several hundred medium-large, modern solid-state computers per year.

The discussions, arranged with the scientists in Shanghai and Peking, were lively and attended by dozens of researchers, teachers, and students. (The formal lectures were attended by a hundred or more people in each city.) Our Chinese colleagues wanted to learn about recent developments in the West; and they were eager to tell us about the design of their computers, and of the ALGOL compiler they had produced for one of them.

They responded warmlywith more than polite warmthto our expressed desire to continue a two-way exchange of information and visits. They were very open in answering our technical questions (obviously, we did not ask them about military applications of computers). The fact that Guo Mojo, president of the Chinese Academy, entertained us at a dinner before we left Peking suggests that our hosts were not displeased with the interchange. A toast I offered to closer relations between the academies of science of the two countries received a cordial but thoroughly noncommittal response from our host.

What were the facts about Mao's China in 1972? The first fact, supported by all the eyewitness evidence from travelers, is that the China experts, who watched China from outside, with almost no direct access, had been essentially accurate in their analyses. I made no observation that contradicted anything the experts had led me to expect. Now I want to qualify that. The experts not only report facts; they also theorize about them. Richard Solomon's conclusions about the psychological and sociological roots of Maoism appeared to me generally sound, not because of anything I observed, or could possibly have observed, in China, but because the facts he adduced (and those available through translation of source documents) fit well our general knowledge about human behavior. Franz Schurmann's theories about organization, on the other hand, seemed to me less sound, not because of anything I observed or of any facts he adduced, but because of his general disregard of modern organization theory and organizational practice in the West.

When I say that the China experts were accurate, I refer especially to their inferences about economics and politics. Of course, they did not know just how much grain, for instance, China was producing. No traveler could find that out, nor was it obvious that the Chinese government knew. The Chinese experience suggests that it is possible to run an economy, even a planned economy of sorts, with only rudimentary statistics. On the other hand, perhaps the Chinese government *did* know, but succeeded in keeping that knowledge from anyone, Chinese or foreign, who did not demonstrate a need to know.

The China experts were right when they told us that the Chinese were not impoverished and that they were not affluent; they had been making steady, but not spectacular, economic gains. The China experts had been equally right in telling us that Maoism was both prescriptive and descriptive of large aspects of Chinese behavior. There was a total absence of public evidence (in the theater, in bookstores, in conversation, for example) of even the most elementary freedoms of political, intellectual, and artistic expression. Social control of personal dress seemed slightly less complete than at the height of the Cultural Revolution some years earlier; occasional colored blouses and shirts could be seen on the streets of Peking, but not often enough to allow you to mistake where you were.

As for economic equality, the picture was less simpleas the China experts had also concluded. The contrast between urban commuters on bicycles (one in every two with a wristwatch) and peasants on a showpiece commune near Peking (austerely neat) was clear. Thus the picture fit together. There were no major pieces of discrepant evidence. What the casual traveler, the eyewitness, saw is what others had been seeing, and what the China experts, watching from outside, told him he would see.

But why should we expect otherwise? A society is fundamentally a simple system, not a complex one. It is more akin to a mass of colonial algae than to a highly synchronized machine. Its main regularities are statistical, and its parameters, statistical aggregates. The rough magnitudes of these aggregates cannot be hidden either from the members of the society or from visiting observers. The general level of life is revealed by the buildings, tools, means of transport that dot the landscapeand by the visible physical condition of people.

The most sophisticated component of social structure is its system of symbol flows, its communications. But these, too, are extremely difficult to disguise. The *Renmin Ribao* is published as a major medium for official communication of public policy to the Chinese masses. For certain purposes, and within severe limits, it can lie, but it cannot tell different stories at home and abroad.

The channels of communication used to organize and manage the Cultural Revolution, until the army was called in toward the end, were primarily newspapers, television, and the big-character wall postersthe mass media. Hence, while interpreting what was happening at any time in China could be difficult (substitute "the United States" for "China" and the statement is still true), and prediction impossible, the difficulty had little to do with the inaccessibility of data. The populace of China, of any society, finds many things about its own society difficult to understand and predict. The Chinese citizen, too, is a China watcher.

What about the power struggles in the inner circle: the fall of Liu Shao Chi, and subsequently of Lin Piao? Here, the relevant communications were largely private and restricted. But an observer, citizen or foreigner, stationed in Tiananmen Square was in no better position to intercept these communications than one reading translations of the Chinese press in the local public library. I can only conclude that China watching from abroad is as good a way of doing social science on a societal scale as any I know; and by and large, the China watchers had done it well. Except for some specifics about computer technology, they had already described for me almost everything I saw on this nineteen-day trip to China, and more (which didn't spoil my enjoyment of the trip one bit).

And so let me turn to the summing up; for tourism to a part of the world where important events are occurring always calls for a summing up. The Chinese people, except for a very few of them, were better off in 1972 than they had ever been in modern timesand by no small margin. (I guess this is what John Kenneth Galbraith, a ten-day expert, meant when he wrote in the *New York Times* that the system "works.") Yet China in 1972 was a land of fear. What I interpreted then as lack of curiosity about the world and self-satisfaction at being residents of the Middle Kingdom was fear of expressing a forbidden interest that some suspicious, eavesdropping Maoist could interpret as evidence of unhealthy Western ideas. The Chinese people were almost totally deprived of every kind of freedom we hold importantfreedom of political and artistic expression, of choice of occupation or residenceand had no visible prospect of attaining these freedoms under a government that did not regard them as social goods. Is the contrast of relative economic well-being with political repression a contradictiona Hegelian, Marxist, Maoist, or just plain garden-variety contradiction? It cannot be a contradiction, because it is a fact about the world; and the world is as it is, and cannot contradict itself.

Nothing I learned about China simplified for me the political decisions facing the United States in the years ahead. My admiration for genuine economic progress did not blind me to the fact that achievement of the

messianic Maoist missionnot our version of it, but Mao's versionwould destroy the human values I place highest in my scale. My genuine concern for that prospector for the damage that could be done in trying to realize itdid not blind me to the undesirability of changes in Chinese society that would destroy its economic and social gains. I did not wish for counterrevolution, although I was subsequently gratified by that middle-left deviationism that the far-left Maoists *branded* counterrevolution.

I returned home with the slim, or perhaps not so slim, hope that in the long run human beings in China would find the same things valuable that were valued by human beings in the United States: the hope that we could avert apocalyptic confrontations between two messianic visionsfor ours was that toountil those visions were moderated by a third vision, the vision of tolerance for human diversity.

Before leaving our trip of 1972, I must recount one revealing anecdote. From the moment we arrived at Canton, the Chinese were very curious about our ages. No matter how often we told them, they would ask again. Then we began to realize that, when our names were arranged in alphabetical order (and how else would Americans arrange them?), our ages coincidentally ran strictly from youngest to oldest (myself). But it was in that order that we were seated at banquetsfrom highest table to lowest. And it was in that order that our cars proceeded in autocade. (Foreign visitors being rare at that time, each of us had a car, a driver, and an interpreter.) How, wondered the Chinese, could the head of party be the youngest, and the oldest be the last?

When I moved forward from the sixth table at the banquet to the head table, to offer the abovementioned toast to Guo Mojo for closer relations beween our science academies, the Chinese were astonished. My interpreter, pale with fright, refused to go up with me, and I had to ask one of the Chinese scientists at the head table to translate for me. After the banquet, the Chinese must have held some hurried conferences to determine why the toast had been offered by the most junior (but oldest) member of the party.

At our next and final banquet, in Canton, we were seated at our usual tables. With our head of party was seated the senior Chinese scientist who was present. At my, sixth, table was seated the senior Chinese political officer who was present. They had finally figured out that I was the political commissar of our delegation!

China in the 1980s

I returned to China a second time in 1980 with a delegation of the American Psychological Association. The ages were in the right order this time: Neal Miller, the oldest, was chief of delegation, and I was second, my status padded somewhat by my Nobel Prize. The revolution that had removed the Gang of Four was four years old, and everyone was hopeful but nervous. Blue was still the color to wear.

China in 1980 was a land of tentative and tremulous hope, its fingers still stiffly crossed, just experiencing the first faint stirrings of economic reform. In conversations with former Red Guards during my stay at Tianjin University, I found that they felt as thoroughly betrayed by the Cultural Revolution as did those whom that revolution had victimized.

The newly rehabilitated intellectuals were still fearful of return to the recent past, but at the same time eager to use their new freedom to soak up the knowledge of Western science. I recall, from all the scenes of that year's trip, a bleak hall in Nanjing Normal University, the faculty, still clad in the standard blue uniforms, trying to grasp the new ideas of cognitive psychology and computer simulationa new kind of magic to replace Marxism and acupuncture.

I struck up a friendship with Professor Jing Qicheng, our principal host from the Institute of Psychology of the Chinese Academy of Sciences, and before the end of the trip we had agreed that I would return to lecture on cognitive psychology and to do research in the institute.

When the psychology delegation left, I remained an additional week to lecture at Tianjin University, where I was known as an economist and an organization theorist. I recall the shock of entering Tianjin by train on a rainy, gray November morning, the streets bordered with huge, disheveled mountains of brick, the ruins of buildings still remaining from the devastating Tangshan earthquake of 1976. I recall my first meeting with Professor Xiao, and his frank despair at my terrible Chinese accent.

Already, the ideological atmosphere was sufficiently clear that I could devote one of my lectures in Tianjin to Western (Adam Smithian) economics, and these lectures could then be translated and published. On the occasion of this visit, I kept a journal for a few dayssomething I am seldom able to doand I quote from it to convey something of the atmosphere around the faculty guesthouse at Tianjin:

November 1, 1980. *Yesterday morning I suddenly found the radiators warm.* [*There had been an unseasonable cold spell, and I was not dressed for it.*] *Government regulations, I am told, allow heating of government*

buildings only from November 15 to March 15. So I am left with three hypotheses: (1.) A dormitory is not a government building . . . (2.) The administration decided to use its hot water coal for heating instead . . . (3.) The administration decided it could not afford having its distinguished foreign visitor catch pneumonia.

Last night at dinner I was able to make myself understood in Chinese to say: (a) that henceforth I wanted Chinese food instead of pseudo-Western fare; (b) I needed a knife to pare my apple. The first sentence was generated in consultation with my table mates (the English teachers) whose Chinese language skills, surprisingly, are almost non-existent.

On the wall of a house facing my bedroom window, someone has scribbled in English "The people is the greatest. Learn from Lei Feng." [*Lei Feng was one of the popular martyrs of the Revolution.*]

November 4, 1980. *At 6:30 the bugle wakes us up—except that I have already wakened earlier. A flicker of the lights indicates that the hot water has been turned on—usually quite hot in the morning if only lukewarm in the evening. This morning, I notice that bathroom and bedroom radiators are warm, but not study radiators. Can they be turning on the heat only for me?*

After the bugle, lively martial music, with a voice calling out commands for the calisthenics—I haven't taken part. I don't see anyone in the village behind us exercising, but perhaps the students are out on the campus in front. The martial music lasts twenty minutes, then the cocks are crowing again. (They start about 5:30.)

Yesterday afternoon, I "escaped," walked out of the campus and took the #8 bus downtown. Giggles and stares, but no problems. Then I needed a bathroom, badly. Decided there must be one in the department store, headed there and asked for it in my impeccable accent. Blank stares. Finally, in desperation, I pulled out my dictionary and showed them the characters. Two nice young men led me to the location. (The bathroom was a single room with both facilities entirely public. Fortunately I didn't have to use the second half, which was filthy.)

Inspected the department store, and bought some little padlocks (lien so) *for gifts. Took the #8 bus at end of line near river (got a seat!). . . . On return, walked over to Nankai University campus; saw a substantial free market between the two campuses.*

Evening: visited by three delegations: (1) the American language teacher, having some problems with his team, (2) a professor here who is trying to translate one of my articles (full of slang and metaphors!), (3) two young men, one of whom seemed to know much math and operations research but no English, the other as interpreter, who asked about satisficing.

Seven o'clock, and the morning news on the public address system outside. Time to go to breakfast.

My third China visit, in 1983, lasted three months. I gave a full course at Beijing University on cognitive psychology (translated by Professors Jing Qicheng and Zhang Houcan *), and worked in the Institute of Psychology of the Academy of Sciences on short-term memory research. I had brought along a graduate student, Gary Bradshaw, to help put LISP on a minicomputer. Dorothea taught English to some of the workers at the institute.

China in 1983 was notable for the free markets selling a great variety of fruits and vegetables everywhere along the main streets. Overt fear was gone and optimism strong. My friends talked with me in public places about economic reform, without looking over their shoulders (a test of political freedom that I learned to apply in the United States during the McCarthy era). Everything could be discussed.

During our stay, we took several wonderful trips, which brought us to Chengde (once called Jehol, it was the capital of Manchukuo during the Japanese occupation); the site of the diggings for Peking Man; Xian, with its ceramic figures; Qifu, the birthplace of Confucius; and West Lake, near Hangzhou, and many other places.

Now I began to have a more serious diplomatic role. The American National Academy of Sciences, in cooperation with the Social Science Research Council, and the American Council of Learned Societies, had created, some years before China reopened to the West, a Committee for Scholarly Communication with the People's Republic of China (CSCPRC.). I had been invited to become a member shortly after my 1980 trip, and became chairman in the summer of 1983, serving for four years in that position.

In the early days of the reopening of China, CSCPRC played a critical role, and the Chinese attached much importance to it. Sometimes they got angry at us when we pressed too hard for access of American scientists to Chinese sites, but they valued greatly our ability to provide funds for mutual visits of scholars and scientific delegations.

An incident indicates how seriously they took it. During my 1983 trip, I was about to go to Tianjin for a one-week visit and expected to stay in the faculty guest house, as I had in 1980. When an Assistant Minister of Education learned that I had just become CSCPRC chairman, he arranged (without consulting me, of course) for me to stay instead at the government's VIP guest house in Tianjanan establishment where Prince Sihanouk had frequently sojourned.

The guest house was in a beautiful large park, hundreds of acres in area, with a number of small villas in which guests were accommodated. It was

also more than a mile from the Tianjin campus and patrolled by guards who would certainly not let any Chinese student or faculty member enter without advance permission. I protested, but it did no good. I was chauffeured back and forth to the campus.

My most important task as chairman of CSCPRC was to get Chinese permission for establishing a one-man permanent office in Beijing. It took about a year to accomplish that, with the indispensable assistance of Yan Dongsheng, the vice president of the Chinese Academy of Science. On December 17, 1985, at a seminar celebrating the opening of the office (housed at the Friendship Hotel in the university district of Haidian), I gave a talk offering my interpretation of the meaning of the Chinese opening to the West—its potential and its problems.

The title of my talk (which I did not select) was "The Intellectual Open Door." The so-called open door policy was an American invention, aimed at equality in rights for all nations in their commerce with China. China was not consulted about the policy, nor, of course, about the scope of these rights and privileges.

The phrase "open door" no doubt reminded most of the Chinese in my audience of the traditional proverb, "If you open the door, the robbers enter." I apologized for it, and emphasized that my topic was not the commercial but the intellectual open door. I argued that a two-way open door for scientists was as advantageous for China as it was for the United States. I paid my respects to Chinese sovereignty, acknowledging that it was the Chinese who must decide how far they would open their door to the researches of Western geologists, biologists, and social scientists who wanted to study at Chinese sites.

In fact, during the ascendancy of Premier Zhao—that is, until shortly before the Tiananmen affair—the door continued to open, and the opportunities for research by American scholars in China, particularly cooperative research with Chinese scientists, continued to broaden. The Tiananmen affair does not seem to have closed the door very much in most disciplines, but it has created a tension that was not there in the preceding years.

I returned to China annually from 1984 through 1987 for three-week stints. Dorothea and I had studied Chinese seriously before the 1983 trip, and I could get around without difficulty and travel alone by train. On most of these occasions I came alone, because I had a heavy work schedule and there was little for her to do. (I did manage to visit the Buddhist caves in Datong, on the border of Inner Mongolia, and to take the boat through the Yangtze gorge, with my friend Jing, from Zhongqing to Wuhan, and to reach the top of Mount Taibut by an aerial tram and not on foot, accruing little merit in the eyes of the gods.)

The mood of optimism in China continued in 1984, but by 1985 the difficulties of transferring economic reform from agriculture to industry were becoming apparent, as were the first traces of inflation. The years 1986 and 1987 represented a plateau, with new questions and struggles about direction, growing pessimism about the economy, and increasing concern with corruption. Tall clusters of gray apartment houses continued to rise everywhere in Beijing, but the confidence in steady, continuing development was no longer there.

In addition to my activity in cognitive psychology, I was called on fairly frequently to lecture on management and economics, and on artificial intelligence. In the autumn of 1987 I was invited to join a World Bank workshop in Beijing with members of Zhao Ziyang's think tank, a group that played a leading role in planning the economic reforms. The other "foreign experts" who were invited were mainly managers of large public corporations in both socialist and capitalist countries: the head of Petrobras in Brazil, the head of the West German railways, an economist with an Indian bank, the head of a trading company in Slovenia, and a Bulgarian electronics entrepreneur. The second American was the well-known management consultant Peter Drucker, an old acquaintance.

The Chinese economy at that time was already suffering from serious inflation, and talk about corruption was becoming steadily louder. The first attempts at establishing unregulated prices (at the urging of Western economists) had had meager success and were one of the causes for the inflation. The government was looking desperately for alternatives.

All the experts in our partyWestern, Eastern, Socialist, Capitalisthad remarkably similar outlooks. A central problem for government enterprises was to enforce some kind of real responsibility for efficiency, for standing on their own bottoms. When they had economic problems, they sought state subsidies. And they were never allowed to manage themselves free from Party interference.

We prepared a set of recommendations as the product of our week's labor, and had an audience with Premier Zhao to present themthe week before he left that post to become chairman of the Communist Party. I was designated chief of delegation. There is no point in detailing our recommendations here. As far as I can tell, they had little or no influence on subsequent events.

What was most remarkable about our meeting with Zhao (I had met with him once before in Washington, several years previously) was his obvious intelligence and his political sophistication. Each time we presented a recommendation, he was instantly aware of its political implications, and we could see clearly some of the heavy constraints within which he was op-

erating. I did not return from the 1987 trip with great optimism for the immediate future of China's economy, however much I admired the valiant efforts that Zhao and his brain trust were making.

In the summer of 1988, I was unable to make the usual trip to China, but Jing and I made plans for my return in 1989. I was to arrive in Beijing on June 5. I will tell the story of that trip in a report, most of which I wrote in Beijing or on the trip home.

BEIJING, JUNE 1989

And now it was June 1989, and I saw China again, at least Beijing. Saw it for 48 hours, just long enough to have my laundry done. The scene was exactly as I had imagined a revolution to bea society dissolved into a gas of unstructured molecules. People without the solid information, without the firm expectations that human bounded rationality depends upon.

The Journey

Jing Qicheng was in the United States in the spring of 1989, and as the student demonstrations in Beijing heated up, we discussed whether it would be wise to postpone my summer trip to China. We agreed that before I left Istanbul for Frankfurt (I was returning from a trip to Turkey) I would make a final check with him, for Jing had direct phone contact with Beijing. On Friday, June 2, all seemed dear and peaceful; the demonstrations were winding down. I took the Lufthansa flight from Istanbul to Frankfurt on Sunday morning, where I was to board the connecting plane to Beijing. When I went down to the gate, fifteen minutes before flight time, I saw for the first time the Sunday newspapers with stories of bloodshed at Tiananmen.

From the newspaper accounts, the situation seemed serious enough, but I long ago learned that news becomes more alarming the farther you are from its source. Why not go to Beijing and see the actual state of affairs? A quick choice at a branch in the maze. I boarded with the other passengersmostly Chinese, few tourists. The plane was half full.

When I woke at dawn, after a reasonably good sleep, it was mostly clear below. We passed over a huge tundra-like swamp, then a Siberian industrial city and into clouds just before I judged we would touch the corner of Lake Baikal. Clear again over barren mountains, and Ulan Bator was beneath us. Then the Gobi Desert, and we began to descend as we reached the mountains of Hebei, where segments of the Great Wall snaked along the ridges.

We arrived at the Beijing airport at about 10 A.M. on Monday morning

after the Tiananmen tragedy. There was nothing remarkable about our descent or landing, nothing unusual to be seen below, just the mountains, rivers, and villages of China. About a half-dozen soldiers or policemen were visible alongside the landing strip, no more.

I was through customs almost instantly, since I had no checked baggage. No Zhu Xinming. (He was to meet me.) I waited an hour, chatting with some Westerners who had come to collect other passengers and learning the current rumors from them. Could the Friendship Hotel (near the universities, a block from People's University and a mile from Beijing University) be reached? They weren't sure whether cab drivers would consent to go there or whether the road was open. They told stories (mostly at second hand) about the bloody events of Saturday and Sunday, providing a wide range of estimates of casualties.

I searched out the currency exchange office on the second floor of the airport. A long line (mostly departing passengers who hadn't known about the airport tax, and needed Chinese money to pay it). Peering down on the departure lobby, I saw that it was flowing over with other passengers seeking to check in for departing flights.

Downstairs again with some Chinese money, I was accosted by two or three enterprising taxi drivers, who appeared willing to head for the Friendship Hotel. I chose one, and we settled for $65 FEC (about $20 US), much more than the metered fare, but quite reasonable under the circumstances.

We conversed in gestures and (his) fluent and (my) broken Chinese. He said (and gestured) something about not being able to eat. I laughed, and told him that everyone thought taxi drivers were rich and ate all they wanted. From then on we were good friends. Such doubts as I had that he really intended to take me to the Friendship Hotel vanished.

My driver was very angry about Saturday's shootings. He thought President Bush should get into the action, should do something. I replied that only Chinese could solve Chinese problems, that China had spent a century getting foreigners out of its hair, and should not invite foreigners back into its affairs. He seemed to find that argument somewhat convincing, but still wasn't sure America could not do something for Chinese freedom.

On the long access road from the airport we passed a convoy of soldiersabout five trucksparked by the side of the road, surrounded by a large number of local people. The soldiers were huddled inside their trucks, but the atmosphere seemed friendly and the conversation brisk. These were the only soldiers I saw on the whole route.

There was almost no motor traffic, just an occasional car or truck, but there was no mistaking we were in China. The bicyclesthe bicycles that

appear in all my dreams of China, whether they be of 1972, 1980, 1985flowed steadily along both sides of the road, the pedals moving neither slowly nor quickly, just steadily, legs moving up and down, up and down. The riders were mostly youngish, more wearing white shirts than work clothes. Coming and going in an endless stream. Here and there, small groups of people stood on the berm of the roadnot waiting for buses, for there evidently were nonejust talking earnestly.

Now we passed the burned-out carcass of a small car (a military jeep?). As we came closer to the city and approached the first main east-west cross street, the intersection was partially blocked by two larger burnt-out cars, small crowds of people around each of them, staring. The driver maneuvered through the crowd carefully, attracting absolutely no attention.

As we reached the ring road, turned, and passed along it, we found more wrecks of cars in our pathmilitary vehicles of some kindand a bus or two straddled across the lanes. Here and there, road separators and other debris had been pulled out into the road, partially blocking it. The stream of bicycles was now less dense, a few cars and trucks. Except for an occasional street-side market, the stores were closed. There were few pedestrians.

A big-character sign hung across the portal of Beijing Normal University, and the gate area was filled with students. I had no time to spell out the symbols, but could guess their general content.

As we reached the gate of the Friendship Hotel, where I hoped a room had been reserved for me, I was at home again. But I noticed a few changes from my last visit in 1987. The renovation of Building Number 5, in which I had lived for so many months during previous visits, was complete.

I left the cab at a new administration building opposite Building 3. When I inquired whether I had a reservation, there was the usual confused and unsuccessful search of the records until I mentioned my association with the Academy, whereupon they quickly found it in another file. I was expected in Building 3.

The renovation of Building 3 had cut all the rooms in half, but my room was attractive and comfortable. Predictably, the lamps were already a bit battered and wobbly, and the bathroom floor had been scrubbed black (how do they do it? Dorothea thinks it is easy enough, with the dirty mops they always use). The local representative of the Academy promised to telephone the Psychology Institute (or Professor Xu) about my arrival. He suggested that the streets weren't safe and that I stay inside the compound. That seemed reasonable advice.

In a couple of hours Lao Xu phoned. He said he wanted to come to see

me, but it wasn't safe for the car (perhaps because they couldn't have returned home before the curfew time, perhaps because things were actually more dangerous than my ride from the airport suggested). They would come in the morning.

Meanwhile, I had strolled around the compound. The Westerners I encountered were eager to exchange news and rumors (mostly rumors). What was going on? The shootings of Saturday night, of coursewith widely varying estimates of the casualties. Rumors of sporadic new shootings. Rumors of clashes between two Chinese armies in Beijing. Rumors of three new armies closing in on Beijing. The Chinese government had disappeared completely from sight. There was no news on Chinese TV. No one showed special excitement, although there was talk about the prospects of leaving.

I went over to Building 4 to see if the CSCPRC office was still there. Someone told me that I could find it on the fifth floor. I took the elevator, walked the length of the corridor, knocked on the door. Our staff man, Perry Link, was not there. Instead, I was greeted by a Princeton historian, Andrew Placks, who had just moved in for sleeping purposes, his apartment elsewhere in Beijing being now potentially unsafe. He was torn between remaining for the last two weeks of his intended stay in Beijing (to finish his reading of some Ming-Qing texts in the library!) and pulling out now. We talked political science. But modern China wasn't really his thing, he said. He was mostly into Ming-Qing, and even older things.

It was not yet quite time for dinner. I felt rather suspended in space and time, the complete absence of reliable information depriving me of any coordinates or of any particular inclination to make plans or projections. I did not think about what path to choose in the maze. There was no maze.

Beginning another stroll through the compound, I was startled to encounter Liang shifu, who had chauffeured me so loyally for the several years prior to his retirement. He was looking young and healthy, still wearing a crew cut that made him look very like a heavyweight boxer. Clearly retirement had agreed with him. We exchanged warm greetings as old friends, lao pengyou. He asked alter Lao Pan (Dorothea) and I after his grandchildren. (The next afternoon, I had evidence of their well-being when I met him again with a very plump young boy of ten in tow. "He eats and eats," said Liang proudly.)

Dinner in the Northeast dining room at 6:30. The room was less than half full. There were almost no tourists. I sat at the table of foreign experts. A chemist from Princeton was accompanied by his young, very quiet (and probably scared) daughter. He had been lecturing at Lanzhou and they had

returned to Beijing as the situation became stickier. There was an electrical engineer from Brown, I think, and a family whose girl, about 10, told us proudly that she had made a flag to wave during the student parades. Everything had been festive until the night of Saturday.

The talk was all on what had happened, who had been where when the violence took place, and what to do. The consensus was that the situation must get worsebut no facts, and no government. It did not take long to acquire the feeling that the situation was completely undefined and that one was wholly at the disposal of mindless forces.

The airport was the escape hatch, the one path out of chaos. But rumor had it that the road to the airport was closed. (I was able to scotch that one, at least for the early afternoon. Who knew what had happened since?) But even if the airport were reachable, the airlines sold no tickets there. The ticket offices were downtown, near Tiananmen, probably inaccessible. No tickets, no airport; no airport, no departure.

What about the railroads? The Central Station was in the war zone, occupied by huge crowds of soldiers and people. How would one buy a ticket? Were trains running? Would there be a railroad strike? Were other cities safer? Clearly, on Monday, June 5, it was easier to enter Beijing than to leave it.

At about 8 P.M., I put in a long distance call to Dorothea. I had to dial a busy line a few dozen times before I reached the operator, but she then put through the call in a few minutes. I described the situation and promised to send word from time to time of my whereabouts. I decided to see what my friends would say when they came to call in the morning, and went to bed at 9:00. Surprisingly, I slept well. Evidently, the situation was too vague to provide material for dreams.

In the morning, I delivered my laundry to the desk. It was unlikely that I would leave Beijing that day, and they were always prompt in returning laundry by evening. In any event, I had not decided whether to stay or leave. That would depend on how my colleagues at the Institute viewed the situation when we met later that morning.

At breakfast, more refugees and more tales. A CMU engineering colleague, Arthur Milnes, appeared. He was a guest of the Semiconductor Institute, scheduled to leave China via Shanghai at the end of the week. Could he get to Shanghai? Could he leave from Shanghai?

I called Perry Link at the CSCPRC office. He was cordial, but had little information that I didn't already have. He said he might be able to arrange a car in the afternoon to go to the ticket office; he would call me.

The delegation from the Institute of Psychology arrived at about 10:00: Xu Liacang, the former director, Zhang Jiatong of the Foreign Bureau, a

new assistant to Zhang named Leng, and Mrs. Kuang Peizi, the Institute Director. They would send out the car to fetch my research collaborator, Zhu Xinming, and his wife, Sun Changhua, who were at home, in the southwest part of the city. Almost no one, they said, was at the Institute. The streets were unsafe.

They did not think that I could do any useful work at the Institute, and urged me to leave Beijing as soon as possible. It was not clear how much of their concern was for my physical safety and how much for the embarrassment of having a foreigner on their hands if policy should turn to the right. Nothing was to be gained by putting that question.

I did not see how I could ask them to take a responsibility for me that they seemed reluctant to accept. Besides, the worst-case scenarios were not attractivea Beijing besieged by the (possibly) converging (hypothetical) armies, food supplies dwindling, the airport closed, trains not running. I was not especially looking for that level of adventure.

There was much discussion about how to accomplish my departure, but few ideas. We tried to get Zhou, the President of the Academy, into the act, telephoning him at the Academy. His office did not know where he was. Conferring with the rest of the Politburo? Sacked? We never found out. The next idea was to call the American Embassy. The press line, whose number Link had given me, was dead. Another number rang for long minutes, but no one answered and the ring changed to a busy signal.

By that time I had heard several stories about the American Embassy's responses to the stranded, and did not think the Embassy would be of any great help. Typical story: "I asked them what to do. They said I should stay off the streets. Then I said that I might try to get to the airport. Reply: 'That would be a good idea.'" Embassies of other nations were securing tickets for citizens and arranging transport to the airportthe American Embassy, nothing. These were rumors, of course, as believable as the other rumors.

Meanwhile Zhu and Sun Changhua had arrived. We were able to talk, with some interruptions, about our research, and we exchanged several papers. Zhu had started a new series of high school experiments (teaching algebra by methods based on our theoretical models), but they had been interrupted about a month previously by the political demonstrations. [Zhu was able to resume them before the end of 1989.]

Soon, discussion returned to my plans. At about this time, Xu, Zhang, and Mrs. Kuang disappeared (back to the Institute?), never to appear again. I didn't realize they were leaving and am not even sure I said a proper goodbye to them. To Leng I observed that I could go to any destination outside China: although my return reservation was from Hong Kong, that

was irrelevant. I could go to Tokyo, or Singapore, or Hong Kong, or for that matter, to Frankfurt. It took a while for the idea of irrelevance of destination to sink in, but after a bit, that was agreed upon.

But tickets had to be obtained at the downtown CAAC office. If we sent Leng to the ticket office, how would he pay for the tickets? We didn't know the price, especially since we didn't know the destination. Up to this point, Leng had not shown any particular talent, and was an unknown quantity. Should I countersign $1,500 or more of my traveler's checks, leaving myself almost penniless if anything should happen to Leng? (I wasn't worried about theft, but that he might have trouble, or be intercepted, or be arrested, or who knows what.)

Meanwhile, Arthur Milnes had appeared with his Institute host, who thought he had two tickets to Shanghai. Would I like to accompany them there? Shanghai did not sound very promising. The Semiconductor Institute could not put me up at their guest housethat was unthinkable under the Chinese bureaucratic rules, for my unit, my danwei, *was the Psychology Institute, not the Semiconductor Institute. Wandering around China at this moment without connections did not seem a good idea; better to stick with my* danwei.

I resolved to go downtown with Leng, while Milnes opted to continue to Shanghai. On the way out, we stopped at the room of the Academy representative, who informed us with great certainty that the whole downtown section had been sealed off by the military and couldn't be reached. How did he know? He had heard it. I mentioned my ride from the airport (also rumored to be cut off), and repeated my decision to go.

Since Zhu and Sun would need the Institute car to get home, they would go with us too. Counting the driver and Leng, we were live. The drive down to the CAAC offices was much like the drive from the airport: the same steady flow of bicycles, almost no cars or trucks, no buses at all. Again, we passed the big-character posters at Beijing Normal, but now we continued south toward the center of Beijing.

No soldiers. No police, not even traffic police. As on the previous day, the citizens of Beijing seemed to be policing themselves. The carcasses of burned-out cars were encountered with increasing frequency. At one point, a wrecked bus almost blocked the street. We reached the CAAC office without difficulty, and without any attention being paid to us. We were less than a mile from Tiananmen.

The CAAC office was less crowded than any Chinese ticket office I had ever seen. We immediately got attention. Tickets to Hong Kong were not available before the 12th of the month, but I could leave for Tokyo on the next day. But I would need FEC ("funny money") to pay for the ticket,

about $550. I endorsed six traveler's checks, and sent off Leng (who was performing admirablyintelligently and energetically) to cash them at the nearest bank. While we waited, Sun Changhua also set off, returning with Cokes, peanuts, and crackers (we had not had any lunch, and it was now 2:00 P.M.).

Soon Leng returned empty-handed; all banks were closed. But the man at the counter assured us the tickets could be held until 5:00, and we knew that money was available at the exchange desk of the Friendship Hotel.

We piled into the car again. Driving along the back of the Forbidden City, below Coal Hill, and parallel to Chang An, about a mile away, we skirted the back of the VIP compound, Zhong Nan Hai, past the old city library. At all points, we were within about a mile of the area of the weekend's main violence. Still no soldiers, but an occasional traffic cop at an intersection, directing the nearly nonexistent traffic.

On the way, we had to take the Zhus to their home, passing close to the intersection near the Military Museum where the 27th and 38th armies were alleged to have clashed on the previous day. Seeing no soldiers there either, we arrived at the Zhus' corner without incident. Zhu, bless his heart, wanted to discuss how he could come with me to the airport the next day to give me a proper Chinese farewell. Since that made no sense, we said our farewells there.

Leng, the driver, and I headed back toward the hotel, passing a wrecked tank in the middle of the street. However, when we encountered a new hotel along the way, we stopped to cash the checks there. Leng then took me home, and went back to the CAAC office alone to pick up the tickets.

In my room again at about 3:30 P.M., I read a Chinese book until I fell asleep. Leng's knock on the door awakened me at 5:00. He had the ticket, and we agreed he would pick me up at 6:30 in the morning for the 9:00 plane.

At dinner, Milnes told me he had his Shanghai ticket. The Princeton chemist and his daughter seemed now also to have definite travel plans, and the daughter was more cheerful. The new rumors at dinner were not very different from the old rumors. The converging armies again, and clashes between armies. The army was coming the next day to occupy the university campuses (which would take the troops right past our hotel, on Bai Shi Xiao Street).

But also that Deng had died suddenly (of cancer!), and Li Peng had been shot in the leg. (Later I was told that someone interviewed on radio claimed to have started this rumor in order to force Li Peng to surface!) Apart from these rumors, no hint of what the government was doing, or who was the government.

Once my path was set for departure, my mood changed. Yesterday, there had been no plan, and it didn't matter very much. It would be time to make my decision on the next day. I observed the world around me as though from outside. Now I had a plan, and I could see all the possible obstacles that might abort it. There was a maze, but could I find a path through it?

It was harder than the previous night to take my mind off the subject, but I was delighted to find that my laundry had returned on time. I went to bed early again, and slept reasonably well, but more fitfully than the previous night, for I was concerned that I not oversleep the alarm.

At 10:30 P.M., I was awakened by a phone call from my old friend Lao Li, now retired from the Institute, whom I wouldn't be able to see. We exchanged warm greetings. Then back to bed. (Another American who slept in the Friendship Hotel that night reports that he was kept awake by intermittent cannon fire. I heard not a single shot. But then, I do sleep well.)

On Wednesday morning Leng and the driver arrived exactly at 6:30. I had been up early and packed. To while away the time before they came, I sat down and began this account of my journey. The trip to the airport was hardly different from the trip in the opposite direction two days before. Now, near the beginning of the airport road, a sizable army unit—perhaps a division, certainly a regiment—was camped in a large field next to the right-of-way. They kept to their encampment, a few hundred local residents watching them from nearby.

The airport was not nearly as crowded as on Monday. I was soon checked in to my flight, which loaded late, and then sat at the gate until noon. (The person who mentioned cannon fire at our hotel the previous night left for Hong Kong as I was leaving for Tokyo. He reported loud cannon fire at the airport. I heard none during the live hours I was there. In fact, I heard no shots of any kind during the entire time I was in Beijing. My hearing gone bad? Irrelevant, in any event—enough shots had been fired for anyone's taste.)

During this wait, security guards entered the plane several times to check the boarding passes of the passengers. The man sitting next to me, fortunately not Chinese, had misplaced his, but they forgot about him after hassling him mildly for a while. The plane ultimately left the ground and we were all on our way to Tokyo.

If the Travel Theorem needs further confirmation (it doesn't), this journey provided it. Anything I learned in the information vacuum that was Beijing from June 5 to June 7 I could have learned easier, quicker, and cheaper in the San Diego Public Library. But then I would not have shared that tragic moment with the Chinese people, and, somehow, I am glad that I did share it. Glad and almost unbearably sad.

The Soviet Union

My political principles ruled totalitarian states out of bounds for our travels. Principles, however, are easier to proclaim than to apply. Dorothea and I had (as I have confessed elsewhere) dipped down from France through Roncesvalle for one afternoon in Franco's Spain, but since we got no farther than Pamplona, that hardly counted. Again, we visited Prague in 1976, but here the issue was not clear, since the Czechs were at least as much sinned against as sinning. I had had some previous contacts with "good" Czechs, who were living under very difficult conditions, harrassed by the Party. I hoped a visit would give them more aid and comfort than it would give the establishment.

No qualms were felt when we visited Yugoslavia; it seemed clearly desirable to encourage their departures from the Stalinist party line, and our trip there in 1971 was most enjoyable. East Germany should clearly have been out of bounds, but somehow I finally agreed to attend an international meeting there in 1985. There is no easy way to rationalize that departure from policy.

Over the years, I had consistently declined invitations from the USSR. Russian literature has always fascinated me, although my Russian reading skills are not quite up to it. Nevertheless, I had read most of the great and nearly great Russian novels in translation, had read a good deal of Russian history, and had watched with sympathy the anguish of the Russian people in World War II. But the USSR was the quintessence of totalitarianism. Besides, I was told by all returning visitors that the food was terrible and the Intourist surveillance oppressive. Since I travel for pleasure, I could see no point in going to Russia, even apart from political principles.

At the time of the Afghanistan invasion and the exile of Sakharov to Gorki, I even stopped talking to Russian scientists and urged, as a member of the council of the National Academy of Science, an interruption of all scientific exchanges. (We stopped a little short of thatthere was no consensus on a policy.) At about that time, Boris Lomov, head of the Psychology Institute of the Russian Academy of Sciences, invited me to contribute a paper to a new journal he was starting. I wrote him a letter (which I also published in the March 24, 1980, *Chronicle of Higher Education*) declining, and stating, in effect, that I would start cooperating with Russian scientists again when the USSR started behaving in a civilized manner. He was understandably angry, and that is where we left it.

Meanwhile, social science members of our academy were asking ourselves whether we could do anything useful in the interest of nuclear peace. The physicists had long since established a channel for periodic informal meet-

ings with Russian physicists for discussion of nuclear disarmament policies. There was general agreement that these meetings had been useful and even important. We had no intention of duplicating that effort, but sought out other possibilities of useful activity.

We ultimately formed a Committee for the Application of Behavioral and Social Science to the Prevention of Nuclear War—a ludicrous title, which nonetheless conveyed our intent. Our initial agenda included preparing a series of volumes that would survey social and behavioral science literature for research relevant to the goal of our committee. It also included activities aimed at identifying genuine social scientists in the USSR and attempting to begin a dialogue with them—perhaps through workshops on topics relevant to nuclear conflict and its avoidance. Bill Estes of Harvard agreed to chair the committee, and I agreed to co-chair it after I had disengaged from a couple of other activities. In spite of my general reluctance to communicate with Soviet scientists at this time, I *was* willing to discuss with them matters relating to nuclear war.

In the spring of 1987, I found myself a member of a small delegation that went to Moscow for one week with the goal of identifying counterparts—Russians who could reasonably be described as social scientists—and of reaching an agreement with the Russian Academy of Sciences for a program of collaboration on topics relevant to nuclear conflict. By that time, of course, perestroika and glasnost had broken out, and it was time to be civil to Russians again. I did have some doubts about what the food would be like, and it was, shall we say, uninspired.

Our trip was wholly successful. We visited nearly a dozen institutes of the Russian Academy of Sciences, identifying a number of people who were doing or striving to do serious social science research. Toward the end of the week, Estes and I met with Scriabin, the Secretary of the Academy, a hardliner (who fortunately was on the point of retirement, interested mostly in talking about the dacha and automobile that would then be his). Upon agreement that a cooperative program should be organized, we went back to the Hotel Russiya to draft a formal document—handwritten, since we could find no typewriter in the hotel, and of course no duplicating machine. The next morning, the document was accepted by our Russian colleagues with a few modifications (we had to get the authorization of the Director of the Institute in which we were meeting before photocopies could be made), and signed by both sides. Within the framework laid down, we have since had several productive workshops with our Russian colleagues, one in Washington, one in Tallinn (in Estonia), and one in California

While in Moscow, we had time for a bit of sightseeing, including visits to several Russian Orthodox churches that were being restored. I also made

a point of visiting the Jewish synagogue, where, early one morning, I saw a dozen old men intoning passages from the Torah (I assume). It was moving to see this evidence of human indomitability, but hard to identify with it when it was expressed in this archaic form.

With the aid of someone at the American Embassy, I made contact with the refusenik group led by Alexander Lehrner, who arranged a special evening gathering. Meeting Lehrner's son near a subway stop, Bob Axelrod and I were driven to Lehrner's apartment (he had bought into a co-op before he became persona non grata) in a shabbily maintained building but comfortable. There was no evidence that we were followed, and no one paid any attention to us as we entered the building and went to the apartment.

Since the meeting had been called hurriedly, it was attended by only half a dozen persons, who were desperately eager to tell us about the work they were doing in mathematical economicscompletely isolated from what was going on in the outside world. Almost all the refuseniks present had been waiting for more than a decade for exit permits. (Most of them have by now been allowed to depart from the Soviet Union.) It was most unclear how they were eking out a living. There was no harassment during our return trip to the hotel. With a certain degree of nervousness while passing through Soviet customs, I carried back to the United States a thick samizdat that had been entrusted to me, describing their theories.

The trip had one other by-product. I did not see Lomov while I was in Moscow, but another member of our delegation did, and conveyed my greetings to him. His reply was, "Well, maybe Herb will write that article for me now." There was no longer any reason why I shouldn't, but what should be the topic?

Over the years, I had discovered that dialectical materialism did not necessarily exclude mysticism. Research on extrasensory perception was supported by the military in Russia (as it was in the United States and in China). Marxist versions of Gestalt concepts and phenomenological viewpoints were used to argue against the possibility of computers thinking, just as they were in the West. In other words, all the folk beliefs survived; they just were repackaged and relabeled in Marxist language.

For example, Tichomirov and Posnyanskaya had "demonstrated" that the eye movements of an expert chess player could be explained only on the assumption that he took in the "meaning" of a position at a glancea holistic, Gestalt view of the process (see page 222). To refute this argument, Barenfeld and I had written a computer program that showed that these eye movements could be produced using only local information. But Communist party philosophers were still publishing "proofs" of the impossibility of artificial intelligence, à la Dreyfus and Searle. (And Dreyfus's book *What*

Computers Can't Do [1972] had been translated into Russian and published in Moscow.) You had to look at the dateline to know whether you were being attacked by a Marxist or a phenomenologist.

It occurred to me that the best thing to publish in the Soviet Union would be a paper showing the consistency of information-processing psychology with dialectical materialism. I was able to get the collaboration in this task of Qicheng Jing, who was well versed in Marx, Engels, and Lenin, and the paper was written and published in Lomov's journal (Simon and Jing 1989). Translating information-processing concepts into "correct" Marxist language proved not at all difficult. The paper has now appeared in Russian, Chinese, and English versions. Whether this will have any effect on Marxist attitudes toward cognitive science remains to be seen. Whether there will be any Marxists left to concern themselves with this exotic question is also in doubt.

One last matter: In the light of all this talk of political principles, how do I explain my willingness to visit China, a state every bit as totalitarian as the USSR, as early as 1972? A frivolous answer would be, "The food promised to be (and was) much better." I am not sure I know the true answer. A spirit of adventure that overcame principle? The nature of the Soviet Union was well known, but the China of the Cultural Revolution seemed a great mystery. A romantic attachment, shared with many Americans, to the idea of China?

By 1980, of course, China was very different from the Soviet Union. Speech on most subjects was quite free, and most foreigners did not sense any close surveillance. (For the Chinese, it was different.) Things got progressively betteruntil June 1989. We will have to wait to see how comfortable travel in China will be in the near future, and whether there is still a role there for an amateur diplomat.*

* I returned to China for three weeks in August 1990, finding the country calm, and speech surprisingly open and free.

Chapter 23
Guides for Choice

A favorite question to pose to gurus is, "What is your philosophy of life?" On rare occasions, I have been incautious enough to answer the question. A philosophy of life surely involves a set of principles. But principles for what purpose? Principles can provide a book of heuristics to guide choice at life's branch points, a thread to keep one on the right path in the maze. Principles can also rationalize, explain, or provide excuses for choices one has already made. It's not easy to distinguish between these two uses of principle; perhaps it is not even necessary. A philosophy of life can contain both.

In either case, it would appear to be a lot easier to have a life philosophy at age sixty-eight than at age eighteen. Or is it easier? Perhaps the process of living confuses as often as it clarifies. Perhaps one should write down a life philosophy at eighteen, before the complexities have emerged, so that one can produce it, on request, when one is sixty-eight. But that is dangerous too. One's readers would be tempted to compare the philosophy with the life. Safer to write it at sixty-eight, or even later.

The phrase "life philosophy" sounds solemn. We must distinguish between its two different meanings. In one sense, a life philosophy is a statement of your raison d'être in the midst of your cosmic and human environment. In a second sense, your life philosophy is your picture of this cosmos, including, in center foreground, your picture of the human condition.

As to the first sense, the creature of bounded rationality that I am has no illusions of attaining a wholly correct and objective understanding of my world. But I cannot ignore that world. I must understand it as best I can, with the help of my scientific and philosophical fellows, and then must adopt a personal stance that is not outrageously incompatible with its ap-

parent conditions and constraints. I must eschew personal goals that require gravity shields or the perfection of humankind for their success.

I am an adaptive system, whose survival and success, whatever my goals, depend on maintaining a reasonably veridical picture of my environment of things and people. Since my world picture approximates reality only crudely, I cannot aspire to optimize anything; at most, I can aim at satisficing. Searching for the best can only dissipate scarce cognitive resources; the best is enemy of the good.

Already, you have learned something about my life philosophy, both the cosmological and the personal one. Let me now describe the former a little more systematically. I am a creature of the twentieth century, thoroughly immersed in its science and its empiricism. My cosmos began (probably) with a Big Bang, and has been evolving inexorably ever since through astronomical, geological, biological, and anthropological ages, the timeline magnifying gradually, perhaps exponentially, as we approach the present, and shrinking again, perhaps exponentially, as we peer into the future. Parts of the picture change from time to time, especially the parts most distant fore and aft, but not (at least in the past quarter-century) in ways that are important for a personal life philosophy.

This cosmological machine has laws, but I cannot detect in it any purpose. In this respect, also, I am a creature of my century, needing, like logical positivists and existentialists, to postulate my own goals because I cannot see that they have been given to me by any external donor. The world is vast, beautiful, and fascinating, even awe-inspiringbut impersonal. It demands nothing of me, and allows me to demand nothing of it, a little like some people's conception, today, of a house-sharing or bed-sharing "relationship."

But if the cosmos is indifferent to me, I need not be indifferent to the cosmos. I can seek to live in peace with it. Nor need I put the matter so negatively. The cosmos can be the source of some of my deepest pleasures. Gazing at it, outdoors at night or in a forest or through a microscope, I find inconceivable variety, pattern, and beauty, beyond the competence of human artists.

Some of the beauty of the cosmos is hidden, to be revealed only by the code-breaking activity we call science. Catching glimpses of new patterns, never before seen by the human eye, bringing them into the open, provides the scientist with his or her most moving experiences. And though we can have such experiences directly only a few times in a lifetime, we can have them vicariously as often as we wish by studying the work of our fellow scientists, present and past.

I suppose that is why I am a scientist. But why a social scientist? How

did I choose that path? To explain why (if, indeed, I know the reasons), I must return to the cosmological stage, this time the part occupied by human beings. Neither Aristotle's "featherless biped" nor "rational animal" seems to capture it all, though the latter is closer if we place equal emphasis on both noun and adjective. We humans are minds (and consciousnesses) in bodies that move in a physical world. We are subject, without exemption, to physical and biological laws. If we fall, our bones break; if we cannot find food, we starve.

We have become the species we are through a long process of evolution. As a result, we come into the world equipped with at least some of the requisites of survival (including supporting adults who nurture us). The newborn child is ready to breathe, to suck, to defecate. It doesn't need a life philosophy to do those things or to want to do them. It is ready, also, to learn. And whether through learning or because of the equipment it brings with it into the world, it is soon able to empathize with other members of its species: to feel their hurts as its hurts and, later, their poverty as its poverty. I don't need to recite a full list of human traits, inborn or acquired; I have mentioned some of the more positive ones. I could equally well have mentioned human propensities for predation against our own species, and the deeply ingrained selfishness that was surely one of the prime conditions for our survival.

The human condition is often described as absurd. Surely the term is appropriate: a body shackled to a self-conscious mindor is it the mind that is shackled to the body? The wants and needs of the two parts are absurdly disparate. Can the body regard as anything but absurd the mind while it is gazing at the stars or, worse, wrapped up in its own thought? Can the mind regard the act of sex or the savoring of food as anything but absurd?

Of course only the mind, not the body, can make judgments about absurdity. And so, given the range of human needs and wants, the mind creates myths reconciling it with the body, thus turning absurdity into pleasure, beauty, and tenderness. The mind sometimes even tries to find a common denominator for all the claims of the body, of itself and of the surrounding environment. It gives this common denominator impressive names like the Good, or utility. But the notion of a single, overarching goal is an illusion. We, the mind and the body, have many needs, many desires, fortunately not all clamoring at once (I refer you back to my story, "The Apple," and its crude picture of this symbiosis of needs and wants [see pages 180 88]). Stamping them all with the label of "utility" would be futile. The plurality is real; there is no monolithic goal.

In this committee of urges, wants, and needs, housed in body and mind,

there is no consensus about *the* purpose of life. Mark Twain told a story of Siamese twins who agreed upon alternating time slots during which one or the other would be in full charge. The story did not end well: Both twins had reason to regret the murder one committed while he had control. But the absurdity of the story is the human absurdity. Each of us "time-shares," alternating our many selves. Some parts of life are spent in the enjoyment of music, others in the enjoyment of sex, yet others in the enjoyment of food, leaving lots of time for the enjoyment of mountains, the enjoyment of friends, and, for some fortunate ones of us, especially the enjoyment of science.

Of course this list is not complete; I mean it only to be illustrative. Moreover, I have left out everything except the time spent in consuming. There is work, too, and obligation and duty; a great deal of them in most of our lives. And there are sorrow and grief, which we do not count among life's blessings, but which deepen our other experiences and give them meaning and sometimes a poignancy they might not otherwise have.

SoI am describing a human life with many goals but without a goal. Who would want it otherwise? Who would want to be free from the hundred desires that are always making exigent demands on a day that will not stretch beyond twenty-four hours? And who is capable of fashioning that master plan, that comprehensive utility function that allocates to each want precisely its proper slice of time?

Homo Rationalis

In this chapter, I have been describing my life, and also my personal life philosophy, but I have also been describing the life of Everyperson. My interest in Everyperson began in 1935 as an interest in human decision making, especially in people coping with the complexities, the uncertainties, and the goal conflicts and incommensurabilities of everyday personal and professional life.

You have seen me following that interest in these pages over more than fifty years, an interest that has never left me. I no longer feel as ignorant of the answer as I did in 1935; I and others have made considerable progress toward understanding the conflict and providing solutions. But the allocation of individual or organizational resourceshow it is done and how it ought to be doneremains a central problem of the human condition.

Pursuing the answer has led me on a long but pleasurable search through the maze of possibilities. To understand budget decisions I had to study decision making and, more generally, the processes of human thinking. To

study thinking, I had to abandon my home disciplines of political science and economics for the alien shores of psychology and, a little later, of computer science and artificial intelligence. There I have remained, except for occasional brief visits to the home islands.

At least that is one version of the story: a single-minded search that has persisted for a half-century. Perhaps it is even the true version. Another possibility is that excitement lit the path: first the excitement, after World War II, of game theory, linear programming, and the use of mathematics in economics and operation research; then the excitement of the computer, the machine that taught us how a mind could be housed in a material body.

What significance should one attach to coincidences? The demands of the problem and the excitement of the new tools lured me down the very same path of the maze. And so I was able to spend my scientific life pursuing a problem I thought central to understanding the human condition, while indulging myself in the mathematics and computer formalisms that gave me so much pleasure just in the doing. Nor was I denied the pleasures of friendship, even in professional life, for we have seen that most of my work has involved warm partnerships.

The pictures of *Homo economicus* and *Homo cogitans* that emerged from this quest have already been sketched. When, abandoning the *a priorism* of neoclassical economics, I looked at actual decision making and problem solving, I saw a creature of bounded rationality using heuristic search to find satisficing"good enough"courses of action. And with the help of computer simulation, my colleagues and I were able to account for the facts of human problem solving in a range of both simple and complex situations.

Economists did not flock to the banner of satisficing with its bounded rationality. These ideas still remain well outside the mainstream of economicsbut not indefinitely. For they provide a realistic picture of human choice, a picture that may instruct us about some of the puzzling problems of economics today: decision making under uncertainty, business cycles with their accompanying natural or unnatural unemployment, the role of entrepreneurship in investment, and others. But there is backbreaking empirical work ahead, for the theory of bounded rationality does not permit all one's theorems to flow from a few *a priori* truths. Fixing the postulates of such a theory requires close, almost microscopic, study of how people actually behave.

Science, viewed as competition among theories, has an unmatched advantage over all other forms of intellectual competition. In the long run (no more than centuries), the winner succeeds not by superior rhetoric, not by the ability to convince or dazzle a lay audience, not by political influence, but by the support of data, facts as they are gradually and cumulatively

revealed. As long as its factual veridicality is unchallenged, one can remain calm about the future of a theory. The future of bounded rationality is wholly secure.

Homo Socialis

How do you put duty in a utility function? For a satisficing theory it's quite easy: Simply place it among the constraints. Of course, we may also view duty as a cost we pay for society's willingness to cooperate with us. This implies that every person has a price. Possibly so, but I prefer the satisficing view.

What duties would I impose? Starting at the weak end of the spectrum, there is general acceptance of the duty not to harm othersthe negative version of the Golden Rule. A higher, and not unreasonable, standard is the obligation to leave the world no worse off than it would have been without us. Since most people, even people in rather humble circumstances, can meet that requirement, perhaps it is proper to insist on it.

A still heavier obligation, not always acknowledged, is to leave to future generations as wide and interesting a range of options as our generation inherited from our forebears. To do so, we must accept collective responsibility for securing sustainable energy sources, preserving the environment, stabilizing world population, and somehow removing or dulling the threat of the Bomb. We have no obligation to solve all the world's problems (there is no prospect that we could); we do have an obligation to avert irreversible catastrophe and to oppose implacably every step toward it.

When we turn to obligations to do positive good, the road seems steeper and stonier. The social scientists of my generation are Depression children, and although the Depression probably had little to do with bringing me to social science, I share the values and feelings of my generation. Given the productivity of which human societies today are technically capable, I regard the elimination of poverty (at least poverty measured against basic physiological and psychological needs) as one of the Big Goods that is actually attainable, perhaps within a couple of generations.

Distributive justice? That's more elusive. My cosmology shows clearly that the distribution of the world's goods owes little to virtue and a great deal to the lottery that distributes families, genes, places of birth, material resources, and other forms of access by the throw of cosmic dice. Does that call for a norm of full equality? Only if you believe that people's aspirations must be guided by comparison with the well-being of others. That belief seems highly unproductive, as it turns the whole life of society into a zero-

sum game in which some can win only if others lose. There must be better games. If I were to select a research problem without regard to scientific feasibility, it would be that of finding out how to persuade human beings to design and play games that all can win. Clearly neither the USSR nor China has succeeded in inventing such a game; nor have we, although perhaps we have come closer.

Homo Scientificus

Does a life philosophy, in addition to the cosmological and personal, include a third element, a philosophy of science? If so, you have already been exposed to most of mine, and can learn even more about it in the Afterword that follows this chapter. If the quality of a research problem rests on the importance of the questions it addresses and the availability of ideas and techniques that hold out a promise of progress, then the study of mind is a most promising research domain. The questions it addresses have puzzled humankind since the earliest times, and underlie the most fundamental questions of epistemology, including the much-discussed mind/body problem. Moreover, understanding the nature of mind is fundamental to building viable theories of social institutions and behavior, of economics and political science. Economics dodged the problem for two centuries with its *a priori* assumptions of human rationality. But those assumptions are no longer fruitful; they must be replaced by a more veridical theory of the human mind.

Since the 1950s we have had the tools to study the mind. We now have a third of a century's accumulation of evidence that the digital computer is the crucial tool we had been lacking. The computer as applied in cognitive science both provides a language for stating theories of human behavior without placing them in the Procrustean bed of real numbers and, by simulation, spins out the implications of the theories. The computer allows us to plumb mind to the level of symbols; we still wait for powerful biological tools to plumb to the neural level.

My life shows that my tribal loyalties are weak. I am a social scientist before I am an economist or a psychologistand, I hope, a human being before anything else. I believe (my third creation myth) that what brought me to the social sciences was the urge to supply rigor to a body of phenomena that sorely needed it. Physics was already too far along (I thought) for genuine adventure. The social sciences offered a field of virgin snow on which one could imprint a fresh form.

Disciplines, like nations, are a necessary evil that enable human beings of bounded rationality to simplify their goals and reduce their choices to calculable limits. But parochialism is everywhere, and the world badly needs international and interdisciplinary travelers to carry new knowledge from one enclave to another. Having spent much of my scientific life in such travel, I can offer one piece of advice to others who wish to try an itinerant existence: It is fatal to be regarded as a good economist by psychologists, and a good psychologist by political scientists.

Immediately upon landing on alien shores, you must begin to acquire the local culture, not to deny your origins but to gain the full respect of the natives. When in economics, there is no substitute for talking the language of marginal analysis and regressionseven (or especially) when your purpose is to demonstrate their limitations. When in psychology, you must be able to understand references to short-term memory and latencies and spreading activation.

The task is not onerous; after all, we acculturize new graduate students in a couple of years. Besides, it may lead you to write papers on fascinating topics that you would otherwise never have encountered. For one of the nice features of the utility function (or the committee of goals I would substitute for it) is that it can acquire ever new dimensions. Learning a new language every decade or so is a great immunizer against incipient boredom.

In describing my life, I have situated it in a labyrinth of paths that branch, in a castle of innumerable rooms. The life is in the moving through that garden or castle, experiencing surprises along the path you follow, wondering (but not too solemnly) where the other paths would have led: a heuristic search for the solution of an ill-structured problem. If there are goals, they do not so much guide the search as emerge from it. It needs no summing up beyond the living of it.

AFTERWORD
THE SCIENTIST AS PROBLEM SOLVER

Many pages of this book have been devoted to describing my scientific work and its impact. Only in a few places have I said anything about my personal style in doing science. In a recent book (Langley et al. 1987), my co-authors and 1 have said a great deal about discovery. Deepak Kulkarni and I have added yet another chapter on this theme in a paper in *Cognitive Science* (Kulkarni and Simon 1988), in which we concluded that the scientist is a problem solver, searching through a maze, and that the theory of discovery is a gloss on the theory of problem solving.

Scientists set themselves many different kinds of tasks: formulating significant problems, discovering interesting phenomena, finding the laws that are hidden in data, inventing new representations for phenomena and their accompanying theories, inferring the logical consequences of theories and testing them, designing experiments, finding explanatory mechanisms to account for empirical generalizations, and inventing new instruments for observation and measurement. Undoubtedly there are others.

All of these tasks use the same general kinds of problem-solving processes as do chess players in choosing moves, subjects in the laboratory in solving the Tower of Hanoi problem (see page 327), physicians in making diagnoses, computer salesmen in configuring systems for clients, architects in designing houses, and organic chemists in synthesizing new molecules.

Moreover, the "insight" required for such work as discovery turns out to be synonymous with the familiar process of recognition. Other terms commonly used in the discussion of creative work—*judgment, creativity,* even *genius*—appear either to be wholly dispensable or to be definable, as insight is, in terms of mundane and well-understood concepts.

Much of the published work about scientific discovery has until recently consisted of anecdotes, frequently autobiographical, about specific discov-

eries and their finders. If discovery requires creativity, or even genius, it would be immodest for anyone to claim that he or she had made a discovery, and futile to try to describe how it had been done.

But if discovery is plain, garden-variety problem solving, then there is no immodesty, and perhaps not even futility, in adding to the anecdotal evidence. In the next pages I will think aloud, albeit retrospectively, about some of my own scientific work, and see whether it, too, fits the problem-solving mold. In particular, I will see if I can find examples to fit each of the components of discovery that I have mentioned.

My predictions will face backward, for backward predictions are really the only ones we can wholly trust in this realm. After all, forward predictions may be influenced by the very theories we are trying to test: The theory may fit our behavior only because we have read about it and think we will do better if we follow it.

Formulating Problems

It is usually thought that before you answer a question you must state it. Or, to change the metaphor, for something to be found, something must have been lost. But is that always true? When one finds a vein of gold, was it nature who lost it? If we can find gold that we haven't lost, perhaps we can answer questions that we haven't asked.

We may find gold (even gold we haven't lost) by searching for it. But that means that we have already asked, "Where can we find some gold?" But what about the gold we find when we are not looking for gold; when we are engaged in some different activity (gathering wildflowers on the mountain, for example)? At the very least, we must *notice* the gold; it must attract our attention, distracting us from the flowers. Do we account for this by postulating a need for gold? Or will an attention-attracting propensity of shiny yellow objects do the job? And how does the attraction of these yellow objects distract us from the flower-gathering task?

Now let's return from gold seeking to problem seeking. Our metaphor suggests that one way to find a problem, and perhaps even its solution, is to try to solve some other problem. That doesn't tell us where the other problem came from—but one problem at a time! We are dealing with the phenomenon of surprise. Searching for wildflowers, we are surprised to see something shining and golden in the rocks. To be surprised, we must attend to the surprising phenomenon. Hence the dictum of Pasteur: "Accidents happen to the prepared mind."

And now we have a new problem: How does a mind become prepared?

If I am to follow the time-honored tradition of using autobiographical anecdotes as the evidence for my theory of discovery, perhaps it is time for an anecdote. My first piece of scientific work was a study of public recreation in Milwaukee (Simon 1935). A standard topic in studies of organizations is the budget process, which in this case involved the division of funds between playground maintenance, administered by one organization, and playground activity leadership, administered by another. How was this division (which was a frequent subject of dispute) arrived at?

My previous study of economics provided me with a ready hypothesis: Divide the funds so that the next dollar spent for maintenance will produce the same return as the next dollar spent for leaders' salaries. I saw no evidence that anyone was viewing the decision in this way. Was I surprised at their ignoring accepted economic theory? Perhaps, initially, but on reflection, I didn't see how it could be done. How were the values of better activity leadership to be weighed against the values of more attractive and better-maintained neighborhood playgrounds?

Now I had a new research problem: How do human beings reason when the conditions for rationality postulated by neoclassical economics are not met? Investigating further, I thought I could see a rather simple pattern. Those who were organizationally responsible for playground supervision wanted more money spent for leadership; those who were responsible for the physical condition of the playgrounds wanted more spent for maintenance. Generalizing, people bring decisions within reasonable bounds by identifying with the partial goals for which their own organizational units are responsible (Simon 1947, chap. 10).

Of course this is only a partial answer. It defined and labeled the phenomenon of organizational identification, a concept that has proved valuable in administrative theory. The broader questionHow do people make decisions when the conditions for the economists' global rationality are not met (or even when they are)?remains an active frontier of research today. The central concept is bounded rationality, a label for the computational constraints on human thinking. When people don't know how to optimize, they may very well be able to satisfice, to find good enough solutions. And good enough solutions can often be found by heuristic search through the maze of possibilities (Simon 1955a, 1982a).

Now what does this anecdote say about finding problems as a part of scientific discovery? One thing it says is that a research problem I found in 1935 has lasted me for a half century. I have never had to find another. The broad problem of accounting for human rationality has generated an endless series of subproblems: How do people solve the Tower of Hanoi problem; How do they choose chess moves; How do they make scientific

discoveries? (Newell and Simon 1972; Simon 1979a, sec. 4, 7; Langley et al. 1987).

Another lesson is that scientific discovery is incremental. An explanation for a particular act of discovery must take everything that has gone before as initial conditions. What we seek to explain is how these initial conditions lead to the next step—in this case, how my knowledge of price theory, and my boss's desire to know how two organizations cooperated to provide recreation services in Milwaukee, led me to observe a phenomenon that initially surprised me; and how that surprise led to the concepts of identification and bounded rationality. Steps taken fifteen years later led from bounded rationality to satisficing, and from satisficing to heuristic search.

Third, the anecdote adds another to the long list of examples of surprise striking the prepared mind as a key event in discovery. What was "prepared" about this particular mind? My training in economics, evoked in the context of a budget situation, disclosed a contradiction between what theory taught me ought to be happening and what my eyes and ears showed me was actually happening. Without the training in economics, the observed behavior would have appeared entirely "natural." Without the observations, I could have continued in the happy illusion that the neoclassical theory of utility maximization explains human behavior in the domain of budgeting. And since my exposure to the economics profession was still rather minimal, I had not acquired the habit, so common in that profession, of ignoring the real world when it contradicts the theory.

Nothing mystical. Nothing magical. Can we simulate it? The heuristics resemble quite closely those of KEKADA, the program that Deepak Kulkarni and I used to simulate the research strategy of Hans Krebs, who found the chemical path for the *in vivo* synthesis of urea (Kulkarni and Simon 1988). The program experiences surprise when its expectations are not met, and reacts to its surprise by seeking explanations for the surprising phenomena. We have not yet expanded KEKADA to simulate the discovery of bounded rationality. But I might have saved myself a lot of work in 1935 had I then had KEKADA to advise me.

Laws from Data

In *Scientific Discovery* (Langley et al. 1987), my colleagues and I gave primary attention to inducing laws, quantitative and qualitative, from data, using programs we call BACON and DALTON, among others, to simulate the process.

Data are not the only possible starting points for inducing new laws;

theories can also be used, in conjunction with data or independently. At the limit, it may be possible to find a descriptive law directly, by deriving it from a more fundamental explanatory law. For example, Newton showed that Kepler's Third Law of planetary motion could be derived mathematically from the law of gravitation. (But note that Newton was working backward from the law that Kepler had already discovered by data-driven search.)

Before one can find laws that fit empirical data, one must have appropriate data that look as though a smooth mathematical function could generate them. It's the recipe for rabbit stew all over again: First catch the rabbit.

Only once in my life have I run across such data, and I cannot recall exactly when I first encountered them—possibly as early as 1936 when I read Lotka's *Elements of Physical Biology* (1924). Lotka's data show that when the number of species belonging to each genus in some order of plants or animals is counted, and the genera are then arranged according to the number of their species, the genus with the nth largest number of species will have about $1/n$ as many species as the genus with the largest number.

Similarly, when the frequencies with which different words appear in a book are counted, and words are then arranged in order of their frequency, the nth most frequent word will occur about $1/n$ times as frequently as the most frequent word. Moreover, about half of all the words that occur in a book will occur exactly once, about one-sixth exactly twice, one-twelfth three times, and so on. These relations hold for books in any alphabetic language, and the departures from regularity are small. The same regularity is seen in the populations of cities in the United States: The nth largest city is about $1/n$ times as large as New York. If the data are replaced by their logarithms they fall on a straight line with a slope of minus one.

What does one do with regularities like this—regularities that, at first blush, can only be described as astonishing? Sir Francis Bacon advised us to induce general laws from them, to find formulas that fit the data; and our computer program BACON (Langley et al. 1987) shows us how to do just that. Then, like the great chemist John Dalton, we should see if we can postulate a mechanism whose operation would produce the regularity described by the formula. Our program DALTON (Langley et al. 1987) simulates that process too. With BACON and DALTON we would know what to do.

I wish I could say that this was my immediate response to the Lotka data. Memory fails me. I recall my fascination with them, but not whether I pondered over them or, if I did, for how long. I do recall that when I returned to Chicago after 1942, I thought about them again. I have a clear picture of sitting in the biology library in the University of Chicago, reading

a paper referenced in Lotka's book. I also recall talking about the matter to Allen Newell while visiting him and his wife, Noël, in their Santa Monica apartment between 1952 and 1954. But I was doing many other things during these years. The startling data on word frequencies and city sizes were not a constant preoccupation, but were more like a recurring itch that needed to be scratched occasionally.

Sometime during 1954 I found the answer. Only a few aspects of the discovery are recoverable now from memory. First, I looked for a function to fit the data. I was especially impressed by the regularity of the word frequencies at the low end of the frequency range. The simple fractions seemed to point to a formula involving ratios of integers. In fact, the simple formula, $f(i) = 1/[i(i + 1)]$, gives the required numbers, 1/2, 1/6, 1/12, and so on. For large i, we have approximately, $f(i) = 1/i2$. The rank, which is simply the integral of the frequency, will then give $F(i) = 1/i$, so that on a logarithmic scale the relation between rank and frequency will be linear with a slope of minus one.

Finding an equation that fits these magic numbers sets a new problem: finding an explanation for the equation, a plausible rationale for the phenomena. My recollections of how I did this are even sketchier than my recollections about the previous stage. The ratios of integers were again the key. Where can you get ratios of integers? Ratios of factorials are one possible source: 1/6 can be written as the product of 1/2 and 1/3, and 1/12 is the product of 1/2, 1/3, and 2/4. In general, the formula $(i-2)!/i!$ produces the required numbers.

The next step is likely to occur only to someone who has a little mathematical knowledge, and sees in these ratios of factorials something like the Beta function, or at least sees the kinds of expressions one encounters in problems on combinations and probabilities. (In fact, I discovered that the Beta function was what I wanted by searching through my copy of Peirce's *A Short Table of Integrals,* where I vaguely remembered having seen some ratios of factorials.)

Are there any other reasons for thinking of a probability model? Indeed there are. What do word frequency distributions and city size distributions (as well as the other quite different phenomena where this same law applies) have in common? Nothing very obvious, unless they can be viewed as instantiations of the same probabilistic scheme for drawing balls of different colors from an urn. So let us see whether we can interpret the formula as representing the steady state of some sampling process.

Here I recall being aided by a metaphor. We think of a book as being created word by word. If a word is added that has already occured k times, the number of words occurring $k + 1$ times each will be increased by one,

and the number of words occurring k times each decreased by one. For equilibrium, the words that had previously occurred k times must be created as rapidly as words that had previously occurred k - 1 times. In this way, the k bin will be replenished as rapidly as it is depleted.

At some point I began to visualize a cascade, with successive pools of water each maintained at a constant level by flow in from the pool above and flow out to the next pool below. Working back from our answerthe distribution that we know describes the phenomenait is not hard to show that the equilibrium condition requires that the probability of creating a word that has already occurred k times must be proportional to k.

We are ready for the final step: to interpret the probability assumption. For word distributions, it can mean that the chance of a word's being chosen as the next word in a text is proportional, because of association, to how often it has been used already, and also proportional, because of long-term associations stored in memory, to how often it is used in the language. In the case of city sizes, it can mean that birth and death rates are approximately independent of city size, while cities will be visible and attractive to migrants in proportion to their current sizes (Simon 1955b).

I won't defend these interpretations here. My purpose is to understand the process that reached them. If my account, through the filter of thirty to fifty years of forgetting, has any relation to reality, then we see a process for arriving at the initial formula that looks very BACON-like, followed by working-backward search processes driven by the evocation of prestored mathematical and real-world knowledgeBACON as the front end to an expert system.

Again, my hands are waving wildly. You will not have failed to notice that I have not accounted at all for the cascade metaphor, yet at some time it was evoked and helped me to formulate the steady-state relations. So there is still work to be done on the theory of discovery; still theses to be written and papers published. But I see in this little history, or imagined history, no magic, no mystery. Each step appears to proceed, if not inexorably at least plausibly, from the preceding one.

If the data cried out so loudly for explanation, and if the discovery process proceeded so plausibly, why did not others discover this law and its explanation? Indeed they did. The first was G. Udny Yule, the English statistician, who in 1924 constructed a model very similar to the one I have just described to explain the data on the distribution of species among genera. (I could have been led to this paper by a footnote in Lotka, but I wasn't.) A second was the English economist D. G. Champernowne, who published "A Model of Income Distribution" in 1953, describing a quite similar process. A third was B. Mandelbrot, who, in 1953, published "An Informational Theory of

the Statistical Structure of Language." I learned about all of these partial anticipations when I searched the literature and inquired among my friends prior to publishing, in 1955, my own paper on the topic (Simon 1955b).

That still isn't quite the end of the story, for again, the solution of one scientific problem created a host of new problems. In the book I co-authored with Yuji Ijiri, *Skew Distributions and the Sizes of Business Firms* (Ijiri and Simon 1977), you can find a series of essays applying generalized versions of the same mechanism to understanding the size distributions of business firms and the economic implications of these distributions.

Representations

Mention of the cascade metaphor that I used in finding the law underlying skew distributions raises the question of representations. What kinds of representations do scientists use in thinking about their research problems, and where do these representations come from? One hallowed form of the question is whether scientists (and others) think in words, or whether thoughts take some quite different shapewhether they employ "mental pictures," for example.

The French mathematician Jacques Hadamard, in his delightful book *The Psychology of Invention in the Mathematical Field* (1945), comes down heavily on the side of images and against words. Among the many distinguished mathematicians and scientists testifying for him is Albert Einstein, who, in a letter to Hadamard (1945, pp. 142 43), stated that "the words or the language, as they are written or spoken, do not seem to play any role in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images which can be 'voluntarily' reproduced and combined."

What is good enough for Hadamard and Einstein is good enough for me. I, too, have difficulty in finding any presence of words when I am thinking about difficult matters, especially mathematical ones. Even as I sit here at the keyboard, composing this chapter, I cannot really detect the words in my thoughts (or much of anything else, for that matter) until they come out the ends of my fingers. But perhaps I am not thinking, but just recording previously composed ideas that reside somewhere in my subconscious mind.

Even if we do think in images, neither Hadamard nor Einstein have had much success in describing just what these images are or how they are represented in a biological structure like the brain. Nor have I. I believe, however, that Jill Larkin and I have recently made substantial progress in explaining these matters (see "Why a Diagram Is (Sometimes) Worth 10,000

Words," [Larkin and Simon 1987]). The basic ideas, which I will not elaborate upon here, are that (1) in the course of transforming verbal propositions into images, many things are made explicit that were previously implicit and hidden; and (2) (learned) inference operators facilitate making additional inferences from the images in computationally efficient ways.

We also show, as a by-product of our analysis, that diagrams are representable as list structures, hence are programmable in standard list-processing languages, hence are readily representable in systems of neuron-like structures. Since the surface structures and the semantics of natural languages can also be represented as list structures, it follows that propositions and pictures (or at least diagrams) can use common representational machinery.

Now just as there has long been a debate over whether we use words or images in our thoughts, so has there been a debate (perhaps the same debate) over whether our internal representations of problems look like collections of propositions or like models of the problem situations. Each of these views has been held by an important segment of the cognitive science community, and the two segments do not often communicate with each other, except sometimes to quarrel.

One segment, under the banner "Let language lead the way," takes verbal reasoning as its metaphor for the problem-solving process, and thinks of reasoning as some kind of theorem-proving structure. The second segment of the cognitive science community uses heuristic search through a problem space (a mental model of the task domain) as its metaphor for problem solving. *Human Problem Solving* (Newell and Simon 1972) adheres strictly to this viewpoint.

Let me return to my main topic of providing anecdotal evidence about the problem-solving processes used in scientific discovery with an example that I will present rather sketchily, to avoid technical detail. Economists frequently use what they call "partial equilibrium analysis," to avoid talking about everything at once by making a host of *ceteris paribus* assumptions. They examine the impact of a disturbance upon a small sector of the economy while assuming no interaction with the rest of the economy.

If challenged on the legitimacy of this procedure, economists may defend themselves by saying that, of course, interactions are not completely absent but they are small, hence unimportant. We hear that argument not only in economics, but throughout all of science. But is it a satisfactory argument? Small effects, persisting over a long period of time, may accumulate into large effects.

Thoughts of these kinds (represented as words or as images?) went

through my mind while I read, in the early 1950s, a paper by Richard Goodwin, "Dynamical Coupling with Especial References to Markets Having Production Lags," published in *Econometrica* in 1947. Without claiming any clear recollection of the precise steps I took to formulate and solve the problem that his paper evoked, I recall conceiving of a large dynamic system divided into sectors, with strong interactions among the components in each sector, and weak interactions among sectors. I remember also that I worked very hard for several months to see how such a system would behave, and that I worked without paper and pencil while taking long walks.

I held a vague mental image of the matrix of coefficients of the dynamic system—hardly surprising, since this is the way dynamic systems are normally represented in mathematics books. At some point, I saw that the rows and columns of the matrix could be arranged in a number of diagonal blocks with large coefficients in them, and only small coefficients outside the diagonal blocks. The matrix was "nearly block diagonal." The number of blocks and their sizes were not seen in detail. If forced to give numbers, I might say that there could have been three blocks, each three rows by three columns in size—but the supposed recollection is surely a fabrication.

At some later point, I acquired a metaphor. I visualized a building divided into rooms, each room divided, in turn, into cubicles. (You can find a diagram of my metaphor on page 212 of *The Sciences of the Artificial* [1981], 2nd ed.) We start out with an extreme disequilibrium of temperature, each cubic foot of each cubicle being at a different temperature from its neighbors.

Several things now seemed obvious. Throughout each cubicle, a constant temperature would be established very rapidly by the exchange of heat between adjoining spaces. At some later time, each room would attain a constant temperature by heat diffusion through the walls of the cubicles. Still later, the entire building would reach a constant temperature by exchange of heat between the thicker walls of the rooms.

Moreover, because of the differences in the durations involved, each of these processes of equilibration can be studied independently of the others. In studying the equilibration of each cubicle, we can ignore the other cubicles. In studying the equilibration of rooms, we can represent each cubicle by its average temperature, and ignore the other rooms. In studying the equilibration of the building, we can represent each room by its average temperature. As a result, the mathematics of the problem can be drastically simplified.

There still were some difficult mathematical steps from this picture to rigorous proofs of the (approximate) validity of the simplification, but the result to be attained was clear. The reasoning I have described was carried

out mainly in the summer of 1956, and incorporated, together with the mathematics, in a paper written with Albert Ando later that year, but not published until 1961 (Simon and Ando 1961).

I can throw no further light on the source of the heat exchange metaphor, or on how, if at all, I drew inferences from the image of the nearly-block-diagonal matrix. Block-diagonal matrices were not unfamiliar to me, for they had played an important role in my work on causal ordering in 1952 and 1953 (Simon 1952, 1953). The mathematics, which was fairly standard, would have been evoked, I think, in the mind of any mathematician who had put the problem in the form we did.

Our theorems and methods (which may be used to invert matrices that are nearly block diagonal) have attracted the attention of numerical analysts, and of natural scientists who are concerned with hierarchically organized systems. The aggregation method we introduced has also now been recognized to be closely related to the so-called renormalization procedures that play an important role in several parts of physics, and that were invented quite independently of ours.

Even with this sketchy account, the discovery process appears quite unremarkable. The problem was found in the literature (Goodwin's paper), and was represented in a standard way by matrices having a certain special structure. The metaphor, by showing how such a system would behave, made clear the nature of the theorems to be proved. Although nothing is revealed about the source of the metaphor, it is not at all esoteric. The proofs, while intricate, would not pose any great difficulty for a professional mathematiciana case of normal problem solving, we would have to conclude.

Finding an Explanatory Model

The last two sections provided two examples of the process of finding an explanatory modela model for the rank-frequency relation and a model of nearly decomposable dynamic systems. How could one discover an explanatory model of human problem solving? One way might be by observing some problem-solving behavior closely and inducing the model directly from those observations.

There is a good deal of merit in that answer, and something like that happened when the General Problem Solver was invented. But even in that case, the empirical observations were not the sole source of information that guided the discovery. The inventors also had some notions of the shape of the thing they were looking for.

Explanatory theories take a variety of forms. For example, the behavior of gases is commonly explained by supposing that they consist of a cloud of energetic particles, interacting with one another in accordance with the laws of mechanics. Magnetic attraction between two bodies is explained by a field of magnetic force in the space between them.

One common form of explanation, in both natural and social science, employs systems of differential equations, or difference equations. At any given time, the system is supposed to be in a specified "state," and the differential equations then determine to what state it will move a "moment" later. Thus, in mechanics, the state is defined by positions and velocities, and the differential equations show how forces produce accelerations that bring about a continuing change in state through time.

Building an explanatory model involves a choice among these or other representations of the phenomena. Will it be a particle model or a continuum model? Will it represent static equilibrium, a steady state, or dynamic change? The representation has to be chosen prior to, or simultaneously with, the induction of the model from the data.

When Allen Newell, Cliff Shaw, and I began to construct a theory of problem solving, around 1955, we were already committed to a representation. In fact, it was our recognition that such a representation had become available with the invention of the digital computer that motivated us to undertake the study of human thinking. We observed that the program of a computer is formally equivalent to a set of difference equations. At each operation cycle, the program determines the new state of the machine as a function of its previous state (the contents of all its memories) together with any new input it has received. Moreover, these difference equations were not limited to manipulating numbers but could process symbols of any kind.

The explanatory task, then, was to describe the processes of problem solving in the form of a computer program. The data we could muster on the behavior of human problem solvers had to be examined for clues to the nature of that program. This requirement provided strong guidelines both for the kinds of data that would be valuable (preferably data that followed the course of problem solution as closely and minutely as possible) and for the best ways of examining the data (searching out the succession of "actions" the problem solver executed and the cues that motivated each action).

Of course, there was more to the representation than simply that it be a computer program. It had to contain symbol structures that could represent the structures in human memory, known to be, in some sense, associative. There was a continuing two-way interaction between the gradual construction of the representation and the construction of the theory that used it.

Sometimes programming convenience (or necessity) dictated choices;

sometimes psychological requirements did. Some aspects of the representation that were initially conceived mainly to meet programming needs (for example, the list-processing languages and data structures in the form of lists and description lists) were later seen to have psychological meaning as networks of associations.

Once some experience had been gained with information-processing models in the form of computer programs, they became a readily available tool for building theories of other aspects of human thinking, as has been detailed in previous parts of this book. No alternative representations were even considered.

In the past few years, with a whole new menu of variants availableproduction systems, models of memory with spreading activation, connectionist models, SOAR, the PROLOG languagechoices of representation have again become an important and difficult part of the model-building process.

Designing Good Experiments

Experiments are supposed to test hypotheses or, better yet, to choose among contending hypotheses ("critical" experiments). That an experiment meet one or both of these aims is neither necessary nor sufficient for its being a good experiment. It is not sufficient because testing weak-tea hypotheses of the form, "variable X affects variable Y," or its negation is not usually very interesting, and does not often contribute much to our understanding of the world. Testing stronger quantitative hypotheses (for example, the periods of the planets are as the 3/2 power of their distances from the sun) is much more interesting, and very interesting indeed if the hypotheses are closely connected with broad explanatory theories (for example, with the inverse square law of gravitation).

But when we test these stronger quantitative models, we must remember to throw away the whole standard apparatus of statistical significance tests, which is no longer applicable.* We must also remember that models are multicomponent creatures, and when our data do not fit a model, we are faced with the difficult diagnostic task of determining what to changeor whether to discard the entire model.

So much for sufficiency; what about necessity? Is model testing the only

* I cannot pause here to defend this dictum. It will sound like heresy to psychologists but is nearly unanimously accepted by mathematical statisticians. My reasons, and pointers to the literature, can be found in Gregg and Simon (1967).

reason for experimenting? Surely not. One good reason for running an experimentor for spending one's time just observing phenomena closelyis that you may be surprised. The best things that come out of experiments are things that we didn't expectespecially those that we would never have imagined, in advance, as possibilities. Of such stuff are many Nobel Prizes made.

Lest I be accused of planning experiments by casting dice, let me suggest that there are heuristics for planning both kinds of experiments, experiments to test models and experiments to generate surprise. (Of course, an experiment designed to test a model may also produce a surprise.) I offer some examples, beginning with a model-testing experiment.

A few years ago, I began to study the Chinese language. I did it just for fun and because I planned to visit China but, to put a more solemn face on things, I called it "exposing myself to new phenomena." That allowed me to do some of it on company time with a good conscience. Once in China, the Chinese psychologists I worked with and I decided to replicate with Chinese language materials some standard short-term memory experiments. The motive was to test a model. Does Chinese have a magical number (Miller 1956)? And is it the number seven? The answer to both questions was yesno great surprise.

Meanwhile, I had learned a striking fact about the Chinese language (no surprise to my Chinese colleagues, but a surprise to me). A Chinese college graduate can recognize about 7,000 Chinese characters (*hanzi*). Each character is pronounced with a single syllable. But in the Chinese language there are only about 1,200 distinct, pronounceable syllables (even taking account of tone distinctions). Hence, on average, there are about six homophones for each character.

Somehow (intuition or recognition at work), I remembered that short-term memory (STM) was generally thought to be acoustical in modality, but only because of Conrad's rather indirect evidence that errors in recall generally involved similarity in sound rather than similarity in appearance. In Chinese, we could put the acoustical hypothesis to direct test. After establishing that the STM span is about six or seven unrelated and nonhomophonic visually displayed characters, we presented the same subjects with strings of visually distinct homophonic characters. The result was dramatic: The STM span dropped to about two or three, confirming Conrad's result (Zhang and Simon 1985; Yu, Zhang, et al. 1985).

A similar disposition to test models underlies the experiments that Bill Chase and I did on memory for chess positions, building on the earlier work of de Groot and others (Chase and Simon 1973a, 1973b). Could the difference in chess memory between experts and novices be accounted for by

differences in their vocabularies of "chunked" chess patterns? Our experiments demonstrated differences, but not of the right magnitudean answer that, if slightly disappointing, was much sharper than if we had simply asked whether experts' chunks were larger than novices'.

The experiments on chess memory, like those with Chinese characters, were designed by asking what quantitative predictions a current model made and what measurements would test these predictions. The problem-solving search took place in a task domain, and was facilitated by looking for "surprising" or "interesting" features of the domain. In the Chinese language case, the surprising feature was found first, and the model to which it was relevant was found second. In the chess case, the order was reversed.

These experiments all have an experimental and a control condition, just as any well-designed experiment is supposed to have. In the Chinese language experiments, we compared homophonic with nonhomophonic strings of characters. In the chess experiments, we compared the performance of experts with the performance of novices, and chess positions from well-played games with random positions. The expert/novice dichotomy has also served me in good stead in some more recent experiments in problem-solving in physics (D. P. Simon and Simon 1978; Larkin et al. 1980). An incidental benefit of using this paradigm is that clear-cut experimental and control conditions seem to soothe the savage breast of referee and editor.

Problem Isomorphs

One other experimental manipulation has provided us with almost unlimited mileage: the idea of problem isomorphs. I think I invented the idea of problem isomorphs about 1969, or a little earlier; I do not have any evidence of earlier mention by myself or anyone else. I have a conjecture about its antecedents. (It is a reconstruction, not a recollection, although my Dutch colleague, John Michon, without prompting, corroborated it.)

Saul Amarel was one of the first researchers in artificial intelligence to point out that changing the representation of a problem could sometimes greatly facilitate its solution. Amarel, Newell, and I participated in a semester-long seminar at CMU in 1966 on the topic of problem representation. Now it is only a small step (at least by hindsight) from the idea that a subject can solve a problem easily by finding the right representation to the idea that an experimenter can make a problem harder or easier for a subject by presenting it in one guise or another.

So much for the antecedents. Soon, problem isomorphsproblems with identical task domains and legal-move operators, but described by different

sets of wordswere a topic of discussion in the Understand Seminar (alias the Cognitive Science Seminar), which has run weekly in the Psychology Department at Carnegie Mellon University for twenty years. The first example was number scrabble, an isomorph of tic-tac-toe; and John Michon then added another member to this set. John R. Hayes rapidly became the most prolific and ingenious designer of problem isomorphs, providing us with somewhere between a dozen and two dozen isomorphs of the Tower of Hanoi puzzle, most of which have been used in one or more experiments (Hayes and Simon 1974, 1977; Simon and Hayes 1976).

We have used isomorphs to discover what characteristics of a problem, other than the size of the task domain, account for its difficulty. Early work in problem solving, our own included, had focused on the combinatorial explosion of search as the main source of problem difficulty. Yet we had found that the Tower of Hanoi, with a relatively small and easily exhaustible domain, and the Missionaries and Cannibals puzzle, another much-studied laboratory task, with a tiny one, could occupy human adults for fifteen minutes or a half hour before they found a solution.

The idea that only the size of the task domain could affect problem difficulty sometimes died hard. One referee for a funding agency gave our project proposal low marks, assuming that our experiments could have only negative results, as all isomorphs must be of the same difficulty. (At the time we were told of this objection, we had already demonstrated experimentally differences in the ratio of 16 to 1.)

Experimenting without an Independent Variable

The experiments described up to this point all compare performance under two or more different conditions, by manipulating an independent variable. When I examine my other experimental research, I find to my embarrassment that this fundamental condition for sound experimentation is seldom met. What have I been up to? What can I possibly have learned from ill-designed experiments? The answer (it surprised me) is that you can test theoretical models without contrasting an experimental with a control condition. And apart from testing models, you can often make surprising observations that give you ideas for new or improved models.

Let me start with an example of the latter kind that I have already mentioned briefly. Many summers ago Jeffrey Paige and I took thinking-aloud protocols from high school students solving algebra word problems, in order

to discover what processes they used and to compare their behavior with Bobrow's STUDENT program, which solved such problems.

Jeff conceived of a fine idea. We constructed some "impossible" problemsproblems that could not be given a real physical interpretation because their solutions involved boards of negative length or nickels that were worth more than dimes. We then asked our subjects to set up the equations corresponding to the problem statements, but not to solve them.

The outcome was wholly unanticipated. Our subjects fell into two groups, rather consistently over a set of three problems. Some set up the equations that corresponded literally with the verbal statements of the problems. Some translated the problems inaccurately, always ending up with equations that described a realizable physical situation. (A few said, "Isn't there a contradiction?"meaning, "I draw inferences from the problem statements that conflict with my knowledge of the real world.")

Because we were trying to get as dense a set of data as we could, we had asked the subjects both to think aloud and to draw diagrams of the problem situations. The diagrams drawn by subjects in the first group were generally incomplete and unintegrated, and did not reveal the contradiction. The diagrams drawn by subjects in the second group misrepresented the situations in just the way their equations didso as to make them physically realizable. The direction of the casual arrow is not clear, but one can use these results to conjecture that subjects in the second group used imagery to represent the problem situations before translating into the language of algebra. Subjects in the first group translated directly to equations using only syntax to guide them.

With this kind of information in hand, one can begin to construct models for these sorts of behavior, and to make additional predictions. The ISAAC program, written by Gordon Novak to solve physics problems presented in natural language, uses an internal diagram of the problem situation to mediate between the verbal stimulus and the equations it finally constructs (Novak 1976). The UNDERSTAND program that John R. Hayes and I constructed, around 1972, to show how verbal problem instructions could be converted into inputs for a GPS-like problem solver, borrowed this same insight from the algebra experiments (Hayes and Simon 1974). All of this work was antecedent to the current investigations, mentioned earlier, of representation and imagery.

But the most massive set of examples of the experimental strategy of "just looking" is to be found in *Human Problem Solving*. Density of data was the name of the game, and protocol analysis the way of playing it. Both Al Newell and I agree that the core of GPS was extracted directly from a particular protocol that we can identify. We also agree in what week in the

summer of 1957 it was done. On the details, the evidence is not wholly concordant, but the main lesson is clear: The GPS theory was extracted by direct induction from the thinking-aloud protocol of a laboratory subject, without benefit of an experimental and a control condition.

What, in addition to luck, entered into the result? First, we already knew that we wished to represent our model as a computer program in a list-processing language. Second, a data-gathering method was used that obtained the densest record of the subject's behavior we knew how to get. Third, some care had been taken in selecting the task. Application of these criteria to the selection of problem-solving tasks accounts for a substantial fraction of the knowledge that has been collected about problem-solving processes during the past thirty years, and a substantial part of the theoretical efforts that have succeeded in accounting for behavior in many kinds of tasks.

Do these experiments really lack independent variables? Can't we consider the task domain or the subject to be just that? Or course we can, but why should we? The principal knowledge gained from these experiments did not come out of comparing between tasks or subjects. It came out of painstakingly analyzing individual protocols and inducing from them the processes that problem solvers employed. Once this had been done, we could test the generality of our results by comparing over tasks and over subjects. But detailed longitudinal analysis of the behavior of single subjects was the foundation stone for the theories we have built.

If the methodology troubles us, it may be comforting to recall that detailed longitudinal analysis of the behavior of a single solar system was the foundation stone for Kepler's laws, and ultimately for Newton's. Perhaps it is not our methodology that needs revising so much as the standard textbooks on methodology, which perversely warn us against running an experiment until precise hypotheses have been formulated and experimental and control conditions defined. Perhaps we need to add to the textbooks a chapter, or several chapters, describing how basic scientific discoveries can be made by observing the world intently, in the laboratory or outside it, with controls or without them, heavy with hypotheses or innocent of them.

The Scientist As Satisficer

My economist friends have long since given up on me, consigning me to psychology or some other distant wasteland. If I cannot accept the true faith of expected utility maximization, it is not the fault of my excellent education in economics.

Alas, it did not take. My traumatic exposure in 1935 to the budgeting process in the Milwaukee recreation department had made of me an incorrigible satisficer. I have sketched the theory of scientific discovery to which my study of these problems has led me. It is not a theory of global rationality but one of human limited computation in the face of complexity. It views discovery as problem solving; problem solving as heuristic search through a maze; and heuristic search as the only fit activity for a creature of bounded rationality.

Some scientists believe that theories should be judged by their ability to make correct predictions. I have provided here some tests of the predictive power of this problem-solving theory of discovery. The anecdotes from my own scientific life are instances where it gives a pretty good account of the processes visible in my research.

The problem-solving theory describes me, like KEKADA, formulating a new problem in response to surprise at encountering an unexpected phenomenon. It traces my BACON-like progress toward discerning a lawful regularity in data, and the evocation of knowledge, in expert-system style, to explain the regularity. It accounts for my use of diagrams to gain a grasp of complex phenomena in a dynamic system. It illuminates how the availability of representations and the invention of new ones has influenced my efforts to construct explanations. It characterizes a number of my strategies for designing experiments, and perhaps even explains why I am frequently unconcerned about such things as experimental controls or even independent variables.

Of course, I am exercising poetic license in talking of predictions. A comprehensive Simple Simon has not been programmed; only pieces of him exist. It would be more defensible to talk of explanatory accounts than of predictions. But you will not be misled by the metaphor, which is as useful as one can expect a metaphor to be.

The information-processing theory of discovery that I have been describing has one other virtue. It is not only a descriptive theory but a normative one as well. Not only does it predict (explain) my behavior successfully, but, unbeknownst to me, it has provided me for fifty-three years with a reliable set of heuristics for conducting research. Quite unwittingly, I have been following the instructions of BACON, of STAHL, of GLAUBER, of DALTON, and of KEKADA. I couldn't have had better guidance.

Even combined together, these heuristics fall far short of a master plan guiding my research career. In any given year, I have seldom known what next year's experiments or next year's problems would be. But the heuristics introduced a slight bias into my decision making. Each time I came to a point of choice, they nudged me along one path rather than anothera

little more reliably than if I had tossed a coin. In reviewing my research and my life, that is as much plan as I can find.

One heuristic that has been of first importance to my work is missing, however, from the programs I have described in this chapter: To make interesting scientific discoveries, you should acquire as many good friends as possible, who are as energetic, intelligent, and knowledgeable as they can be. Form partnerships with them whenever you can. Then sit back and relax. You will find that all the programs you need are stored in your friends, and will execute productively and creatively as long as you don't interfere too much. The work I have done with my more than eighty collaborators will testify to the power of that heuristic.

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PHOTO SECTION

Great-grandfather Alexander Goldsmith
(with Civil War medals), 1900.

Great-grandmother Anna Dahl Goldsmith, 1900.

High jinks of German Student Verein members, Darmstadt Technisches Hochschule. Arthur Simon is at the right, unarmed, 1902.

Grandmother Rosalie Herf Simon, 1905.

Grandfather Joseph Simon, 1905.

The Merkels, 1907. First row: Grandma Ida and
Edna; second row: Grandpa Charles and Uncle Harold.

Young Herbert with his brother Clarence, 1918.
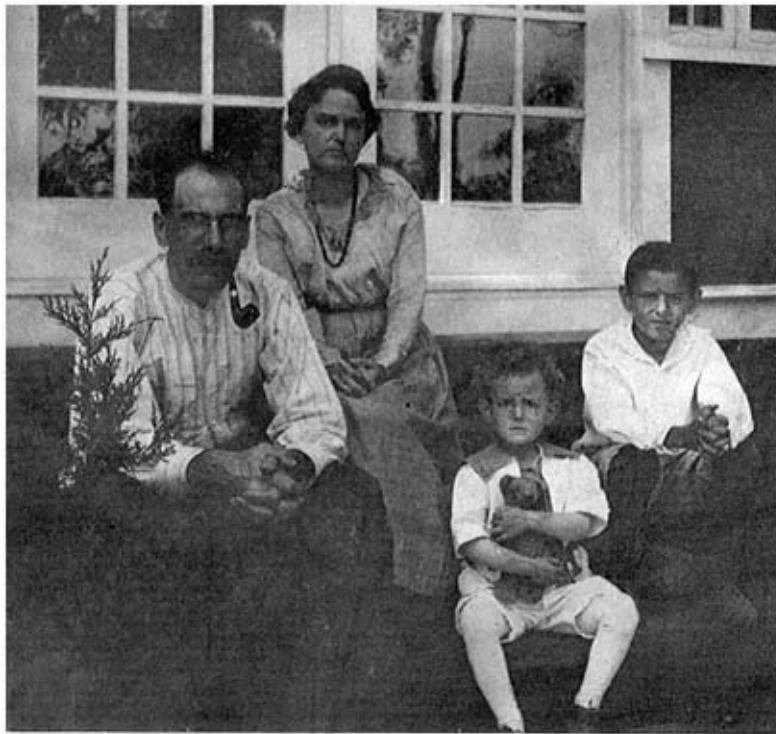
Arthur, Edna, myself, and Clarence at the Cedar Lake
cottage of the Werwaths, about 1918. (Otto Werwath, a
classmate of Arthur's at Darmstadt, was proprietor of the
Milwaukee School of Engineering.)

Entrance hall to 3220 Juneau Avenue, early 1920s.
The living room is to the right, the dining room (with canary)
beyond. An ancient radio, probably of my father's construction, is
beyond the door at center. The two framed pictures are reproductions
of Boecklins: the Toteninsel (extreme left, on stairs) and the Swamp
Maiden, popular examples of German Romanticism.

I approach the age of six, New Year's, 1922 (the year I began first grade at the 27th Street School).

Outside Burton Hall dormitory, University
of Chicago, 1934. Rear to front: myself, Leo
Shields, Winston Ashley, Ellis Kohs, Ralph
Niemeyer, "Eeyore" (Milton Wolford). Harold
Guetzkow took the photo.

Professor Charles E. Merriam, "The Chief,"
about 1935, during his tenure as chairman of
the University of Chicago Political
Science Department.

Rockmarsh, spring 1935. I walk out
to the Gravel Island.

Dorothea and I pose after our
wedding at my home in
Milwaukee, Christmas Day, 1937.

I take a rest at Evolution Basin, on the trail toward Muir Pass,
during out Sierra Nevada hiking trip with the Arnons, summer 1941.

Dorothea, looking very healthy, at the door
of our Virginia Street aerie, on return from the
Sierra trip, 1941.

Peter, Barbara, and Kathie (ages 3, 1, and 5, respectively)
at the schoolyard across from our 57th Street apartment
in Chicago, spring 1947.

The first year at GSIA, 1950. From the right: Lee Bach, Elliott Dunlap Smith, myself (standing), and Bill Cooper, with the six graduate students in the first class in Industrial Administration.

With Jim March in 1958, at the time we were writing *Organizations*.

Research on problem solving, using the Tower of Hanoi puzzle as the laboratory task, 1969. (The Tower of Hanoi was to cognitive science what the bacteria, E. coli, were to modern geneticsan invaluable standard research setting.)

Bill Chase and I discuss expert memory in chess, while doctoral student Neil Charness prepares to film an experiment, 1973. (If the Tower of Hanoi is the E. coli of cognitive research, chess is the Drosophila.)

Programming the first Psychology Department computer for an experiment, 1973.

The Simon family in the best of humor, on our way to the ball at the royal palace during Nobel week, December 1978: myself, Dorothea, Barbara, Katherine, Peter. I acquired the traditional top hat after being awarded a doctorate by the University of Lund in 1968. (Photo by Kjel-Ake Andersson, *People Weekly,* © 1979 Time Inc.

Science politics. A meeting of a committee of the National Research Council, about 1982. In the rear row, I am flanked by the economists Gerard Debreu and Bob Solow; political scientist Gabriel Almond stands at the extreme right. In the front row, left to right, are geographer Julian Wolpert and psychologists Bill Estes and Mark Rosenzweig.
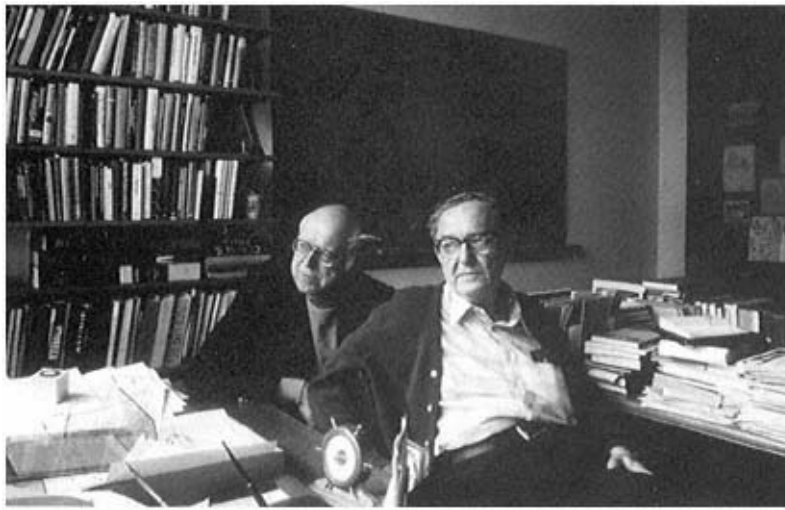
Allen Newell and I looking backward, 1985. (There do not seem
to be any photos of us in the period around 1955 1956. Clearly,
we had no sense of history. Or perhaps we were too busy
with our research to be photographed.)
(Photo by Duane Michals, © Time Inc.)
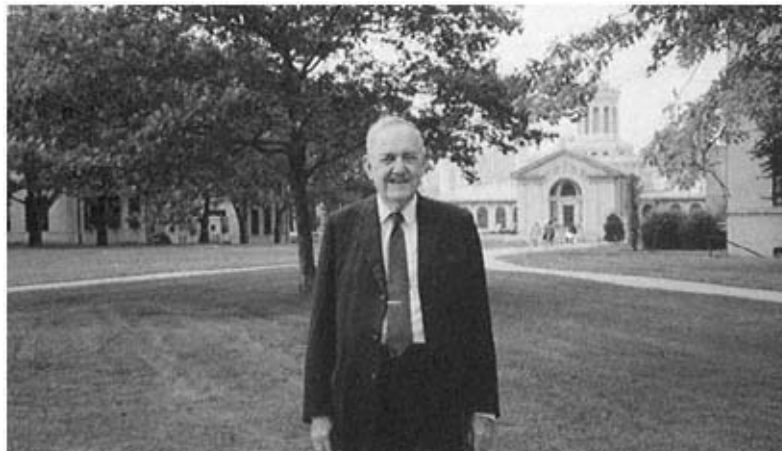
Carnegie Mellon's Palladian campus (circa 1992), designed by
Henry Hornbostel. My office is in Baker Hall (long building under
trees on the left). Behind me is Hamerschlag Hall, whose façade
was inspired by Alberti's Sant' Andrea in Mantova. (We verified this
by visiting Mantova in 1993.)

Unwrapping the stocking gift, Christmas morning, 1993. A
corner of the living room at 5818 Northumberland Street,
our home since 1949.

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