My Life

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Preface

In 1998, while on sabbatical in New York City following my years as Dean of Engineering and Provost at MIT, freed from the time restraints of teaching, committee work, and all that goes with a career in academia, I reflected on the transformations of society during my own lifetime. In my own academic fields they have been unprecedented, but in the wider world political changes have also been extraordinarily significant. As this is being written, developments in Israel, where I was born in 1941, are impossible to predict, but one need only compare today's situation to the terrible threats that obtained then, especially in the Arab-Israel war of 1948, to hope that in our own time peace between the two peoples will become a reality. As I narrowed my focus to my own career, I saw that the developments in Artificial Intelligence, Computer Science, Electrical Engineering and Engineering Systems have also transformed the respective fields, as well as society. Finally, I reflected on my relationship to my academic home, the Massachusetts Institute of Technology, and on the influences that led me to pursue AI, CS and Engineering Systems and academic administration as a life work. It occurred to me that, although there would certainly be enough material to fill a book calling attention to these topics, organizing them in a readable manner might be difficult.

One of the first problems was how to do justice to those who went before me, whose values and histories shaped my own life. I believe strongly in the adage, "As the twig is bent, so grows the tree," and spent a lot of my time while writing this memoir simply calling to mind my family history as it was passed down to me and as I remember it from my own childhood. These stories are vital to my own history, and I have tried to honor my parents and grandparents by beginning this book with them. Chapter 1 carries the family from origins in Germany and Romania, through my parents' escape from Nazi Germany in 1939 after the beginning of World War II; their life in Palestine, their eventual immigration from Israel to the United States in 1954, and my own early childhood are described in Chapter 2. Our arrival in the US in the mid 50's was fortuitous, since the US was going through an amazing period of expansion and optimism.

In the course of writing the next two chapters, I began to realize how strongly certain influences inherited from my father have set me on a course different from that of my American-born peers, beginning as early as junior high school. My father's classic German attitudes, fortified by our Jewish traditions and modulated by my mother's Romanian background, would influence the direction of my studies of organizations and structures; as important, they would shape my outlook as I took on the responsibilities of a husband and father. I can see now how the philosophy of Kant, himself greatly influenced by Plato and who had much influence on much of modern German culture, conditioned my studies of mathematics. It also set me apart from my American colleagues, whose thinking largely reflects that of the British Empiricists, such as Hume, and explains some of my frustration with their views of the organization of men, engineered systems, and the mind.

My own life became inextricably entwined with MIT when I arrived from Columbia University as a graduate student. This becomes evident in the following chapters, where the emphasis shifts from the personal to the academic. Gradually my work in computer science and artificial intelligence began to come under pressure from my "day job" in the Institute's administration, from early laboratory and departmental posts to those of Department Head, Dean of Engineering, and Provost. It is hard to gain perspective, even from this distance, on these years, and it may be that others will recall events differently from me. I have made every effort to be honest in my descriptions of events, and to give credit where it is deserved. MIT has, like most American universities, undergone broad and deep changes in the past three or four decades. MIT had been under significant budgetary pressures since the Vietnam era. Fortunately, the growth of its endowment in the late 1990s was largely translated to a significant decrease in the budgetary pressure just as I was leaving the post of provost. While MIT's endowment has declined somewhat since and has grown again since, its prior growth has been mostly maintained and will help stand the Institute in good stead in the coming decades as competition with other major universities intensifies.

When I was Dean of Engineering I was able to play a role in the transformation of engineering education to include an emphasis on engineering systems, an approach that includes awareness of environmental issues, manufacturing, design, and management of engineering. I am optimistic that the emergence of Engineering Systems as a field will have a strong and positive effect on all of engineering.

In retrospect there is a consistent intellectual trend of thought in much of this background. My father's German heritage, my undergraduate major in pure mathematics, and my interest in political philosophy, especial Plato's, all emphasize abstractions. My research related to the system MACSYMA, my research on the knowledge based approach to Artificial Intelligence and my approach to engineering systems all rely on this background and interest in abstractions. On the other hand, not everything in my background is consistent with this emphasis on abstractions. My mother was born in Romania, and although she lived in Berlin for twenty years, she still had a tendency to get around rules, whereas most Germans tend to obey rules. I think some of that rubbed off on me too.

I would like to express my appreciation to the many people who have helped me to put my reflections into a form that I can share. During my sabbatical I was encouraged by Columbia's Dean Zvi Galil, who was my host. Friends and associates read and commented on the preliminary drafts, and made many helpful suggestions; most notable among them were Jacob Katzenelson of the Technion and Arthur Steinberg of MIT. My editor, Lois Malone, added readability to some passages in which I was so close to the material that I was unable to focus on it clearly, and checked the final work for errors. Most of all, I would like to thank my wife Peggy for her many years of love and encouragement, and my

sons Jesse and David for their patience when MIT seemed to stand between me and them.

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Chapter 1: Swept Along by History

Romania, 1914–1918

Each side of my family came from a part of Europe with a long, tempestuous history, my father's from Pomerania, a state in Germany at the time, but now largely in Poland. My mother's family came from Romania. Pomerania has been shifted from one major power to another since the thirteenth century, but by the time my grandfather, Julius Moses, was born in Pomerania, it had long been culturally and politically German. Romania, with its wild mountains and forests and its harsh climate, bore the brunt of the invasions by Goths, Huns, and others after the Roman Empire withdrew; this may have something to do with the fiercely independent nature of its people, among whom my mother grew up.

Abraham and Betty Losner, my mother's parents, lived in the Romanian town of Bohush located in the region of Moldavia, now partly in the Ukraine. Betty's family, the Steins, owned a seltzer manufacturing plant, and were moderately well-off; I don't know what Abraham's family background was, but he was a Talmudic scholar who also knew several languages and at one point made his living as a journalist. At the time World War I began, the couple had three young children, Samuel aged 4, Frieda aged 3, and one-year-old Golda, who would become my mother. Both Abraham and Betty were only too familiar with the anti-Semitism that was endemic to the country. Since the Kingdom of Romania was established in 1881, anti-Semitism, encouraged by corrupt officials and petty demagogues, had focused on the Jewish estate managers of absentee landlords. They had experienced the peasant revolt of 1907, which singled out not only these landlords, but Jews as well. As they began their family, they must have been relieved when Romania opted for neutrality during the first Balkan war; but in the year my mother was born, 1913, Romania was fully involved in the second one. These disputes would be among a tangle of events that swept Europe into World War I; their unsatisfactory resolution still haunts the world almost a hundred years later.

Abraham, conscripted into the Romanian Army, was on maneuvers when the war broke out in August 1914 and, even though Romania had declared its neutrality, Germany attacked, and he found himself in a German internment camp. He spent the rest of the war there, and made the best he could of his situation. The Germans relied on him for translations, since he was fluent in both German and Romanian. Although he was only a private, his captors placed him in charge of doling out the prisoners' rations, a distinction he would come to regret.

In August 1914, with Abraham at the front, Betty went daily to the notice board where, in those early days of the war, the list of Romanian dead was posted. One day, there was his name: Abraham Losner. She was overcome with grief, and could barely walk home, where she gave her family the tragic news. The family began the mourning period, and it was several days before someone told them that a man from a nearby town, also named Abraham Losner, was the one who had died.

With Abraham interned, Betty struggled to support herself and her three children. Her family's fortunes had suffered, and everyone was short of food. Remembering an old Jewish tradition, she fasted on market days (Mondays and Thursdays) so that the children would have enough to eat. In one of the complex twists of the later years of the war, the Tsarist Army captured the village. Russian officers took over her home and pressed her into service cooking for them. In return they provided her and her children with extra food, which made things easier for them, at least until the Russians learned, in February 1917, of the overthrow of the Tsar. Told that in the new Russian Army everyone would be equal, much to Betty's surprise they hastily removed their epaulets and all other distinctions of rank. Extra rations for the officers ended, which also meant that Betty and the children had less to eat.

The war ended on November 11, 1918, only two days after Romania entered it on the side of the Allies. Abraham telegraphed Betty from Berlin that he would be home soon; she immediately replied, telling him to stay put and she and the children would join him there. During his long internment some of the returning Romanian prisoners had accused him of favoring Jews when he distributed camp rations; he had been tried in absentia and sentenced to a long jail term in Romania. I don't know if the charges were true, but in any case, given his low rank and his being Jewish, it is likely some would have blamed him whatever he did.

Pomerania, 1900–1935

While my mother's family was experiencing the war first-hand, my father's parents, Julius and Zippora Moses, were relatively undisturbed. Julius owned a wholesale scrap-metal firm, and wartimes favor scrap-metal dealers. He had married well: Zippora was a Tarnowsky, from a very large, well-to-do merchant family in the eastern Germany town of Posen. She brought an ample dowry to the marriage, but it appears that Julius was not an adroit businessman and allowed it to dribble away. They had two children, Bernhard, born in 1905, and a sister. All I recall being told about my father's sister is that she had two sons, and that both she and they perished in the Holocaust. Bernhard, bright, industrious, and well-behaved, excelled in academics. In school, where the boys were seated according to academic rank, he would have been seated first except for the pervading anti-Semitism, which kept him in the second seat. He dreamed of going on to university after high school, and of becoming a lawyer; Julius had other ideas. He wanted his son to follow him in the business, and Bernhard began training in the traditional method, apprenticing himself to one of his father's business competitors for a period of several years.

He was eighteen, still an apprentice in the scrap-metal business, when he had a first-hand experience of the financial chaos that had overtaken Germany. This was in 1923, only four years after the Treaty of Versailles, which imposed heavy reparations on Germany, and had led to the establishment of a new German government, now known as the Weimar Republic. Auspicious beginnings, in which a middle-of-the-road administration suppressed excesses on both the Left and the Right, gave way to mass unemployment and unprecedented currency inflation that wiped out much of the middle class. Clumsy efforts to stem the devaluation included over-stamping notes with values ten times or more their denomination: a stamp converted a five-million-mark note to a fifty-million-mark note. It became difficult to carry enough paper money to complete the simplest transaction. During this time those workers who were still employed adopted the tactic of going directly from the paymaster to the grocer's, before the cash could be devalued again. One day Bernhard's boss gave him a suitcase full of cash in paper currency and sent him to Berlin to buy some scrap metal. He had opened the suitcase and was preparing to count out the money when the seller, impatiently saying it would take too much time, put the money on a scale. He found it more convenient, and evidently just as effective, to weigh paper money.

Added to this business environment, with failures on every hand, was the creeping threat of nationalist terrorism, exemplified by a series of political assassinations in 1921–22. The reparations that crippled Germany's attempts to recover were eased beginning in 1923, and in 1925 Hindenburg became the second president of the Weimar Republic, heading a coalition government that restored some measure of economic stability and even prosperity. In 1926, with full recovery and respectability apparently within reach, Germany joined the League of Nations. At about this time, when things were looking a bit more hopeful, Julius took Bernhard into his firm. His competitors continued to buy scrap metal during the good economic times of the late 1920s, while Julius and Bernhard accumulated cash. As the Great Depression began, in 1929–30, price deflation enabled them to buy out their competitors and await better days.

Hitler provided these "better days." Early in 1933, and despite some misgivings, Hindenburg appointed the National Socialist leader, Adolf Hitler, chancellor. Things moved swiftly thereafter. In an act that inflamed sentiment all over the country, the Reichstag was burned, and within a month the parties of the extreme Right, the National Socialists and the Nationalists, won a bare parliamentary majority. In October, Hitler withdrew from both the League of Nations and the Geneva Disarmament Conference, and began rearming.

The nature of the new government became clear, as Hitler organized his Third Reich with himself at its head. Rearmament was, of course, good for the Moses scrap-metal business, and the family became wealthy, millionaires several times over, but it was clear that evil times lay ahead. In 1935 they arrived: the Nuremburg Laws took away Jews' citizenship, outlawed intermarriage between Jews and non-Jews, and barred them from professions such as teaching and the practice of law. Of more importance to the Moses family, the Nuremburg Laws also forbade Jewish ownership of a business. Julius and Bernhard sold out at a considerable loss, and the family moved to Berlin. They invested the capital from their previous prosperity in two apartment houses and settled in to live on their rents. That the apartment houses were located in the eastern part of Berlin was not significant at the time; it would become so a couple of decades later.

Berlin, 1935–1939

As life in Germany became more restricted and dangerous, especially for Jews, Bernhard saw clearly that it would be necessary to leave. Many German Jews were emigrating to the United States and other countries, but there was a more tempting option: Palestine. Bernhard had a cousin, Bernhard Zondek, Zippora's nephew, who emigrated there in 1935. The two Bernhards had been named for the same uncle in the Tarnowsky family. Bernhard Zondek was a doctor, robbed of his practice by the Nuremburg Laws. Tens of thousands of Jews were fleeing to Palestine, which had been taken from the defunct Ottoman Empire, now represented solely by Turkey, and placed under British mandate following World War I; Britain responded to their persecution in Germany by increasing the allowable number of immigrants. The 1917 Balfour Declaration had assured them of British help in establishing a new Jewish homeland, and despite growing Arab opposition and vague territorial agreements, they poured in.

In 1936 Bernhard Moses went to visit his cousin, who lived in Jerusalem. Bernhard found life in Palestine fairly primitive, some of its practices still carried on as under the Ottoman Empire. An example was the system of weights and measures, which differed among Tel Aviv, Jerusalem, and Haifa. But restaurant meals were cheap, less than a Palestine shilling, wine included, and life seemed to hold promise there. The two Bernhards became reacquainted as they discussed the future.

The Zondeks were a famous family of doctors: Bernhard, his father, and Bernhard's two brothers were mentioned prominently in the Jewish Encyclopedia for their medical achievements. Bernhard was one of the inventors of the Ascheim-Zondek test, the so-called "rabbit test" for pregnancy. He worked in the Hadassah Hospital in Jerusalem, which was part of Hebrew University. The hospital was so far ahead of anything in the Near East that even Arab rulers sent their wives there for treatment. At that time of great hopes for the new nation, Hebrew was being reestablished as the standard language, even though this required inventing new words to name the multitude of things that had not existed in the early days, when Hebrew evolved naturally. There was a considerable opposition to establishing Hebrew, from people who felt German would be more workable. Zondek said a decade earlier the hospital had decided by voting, which language would prevail, and Hebrew had won by one vote; I have also heard this story in reference to the Technion, Israel's equivalent to MIT.

Zondek had prospered, but in the wake of the new influx of refugees, many doctors were driving taxis. "When you go back, tell them to please not send more doctors," Bernhard said. "Tell them to send more patients."

During his visit, Bernhard Moses attended the first performance of the Palestine Symphony Orchestra—now the Israel Symphony Orchestra, on Mount Scopus in Jerusalem. The orchestra's conductor was William Steinberg, whose son, Arthur, I would come to know very well as an MIT professor; the guest conductor that evening was the redoubtable Arturo Toscanini. Arthur Steinberg is named after Arturo Toscanini who helped his father and the rest of the family come to the US.

On his return to Berlin, Bernhard learned that his father had been diagnosed with cancer. There would be no question of emigration in the immediate future. And so he and Zippora waited for the inevitable, while life around them grew ever more difficult. Small indignities were followed by larger ones; gradually the captive Jewish population's humanity—their "personhood," we would say today—was being eroded by the regime. Julius Moses died in 1938.

Bernhard sometimes went into a small coffeehouse, the only one Jews were now permitted to visit. Here in 1938 he met Golda Losner, then twenty-four. I would like to say that he fell in love at first sight, but my father was far too dignified to report such a thing. At any rate, life being as uncertain as it had become, there was probably no time for long, drawn-out formalities. Bernhard, attracted at once, offered Golda a ride home.

The pleasant drive turned nightmarish when the police stopped Bernhard's car. A car owned by a Jew was required to bear a special marking; although the officer pretended that Bernhard had proceeded after being told to stop, it became clear that this was merely a pretext. The officer locked Golda in the back seat and forced Bernard into his patrol car, taking him in for "questioning." Golda waited. She managed to get out of the car, and could have gone home, but she continued to wait. Several hours later he returned, bruised and disheveled and showing every sign of a serious beating. Golda was a strong woman, brought up, it will be remembered, under Romanian cultural influence, and, outraged by what had happened, at that moment she took on the role of his nurse/protector/encourager. Golda would become the rock on which our family was built.

Golda's father, Abraham Losner, had left for Palestine a year earlier, in 1937. His dangerous habit of speaking out against Hitler in coffeehouses had drawn the attention of the authorities, and he had been strongly advised to go while he still could. In Palestine he became a part-time journalist for the Tel Aviv papers, but soon after arriving he was stricken by the first of several heart attacks. He could no longer work, and his wife, Betty, needed to go to him. By this time exit visas were increasingly hard to come by; Bernhard and Golda heard of a rabbi who had received one and then decided not to leave Germany, and a friend obtained the visa and gave it to them for Betty. There were severe limits on the amount of cash that could be taken from the country, so they bought several cameras for her to sell in Tel Aviv. They obtained a fine Torah scroll for the same purpose. The cameras presented no problem at the exit inspection desk, but the Torah was contemptuously unrolled by the officers, ostensibly to see if it contained contraband. It is a measure of how hard life in general had become for Jews, how little it availed anyone to respond to the most outrageous insults, that Betty expressed no emotion, but simply rolled the scroll back up and continued checking out.

Throughout 1938 Hitler's machine rolled on. In support of Franco's fascist rebels in Spain, Nazi planes bombed and strafed Spanish villages; in March Austria was unceremoniously annexed; by September 30 the notorious Munich Pact guaranteed that there would be no opposition when Hitler's forces took over Czechoslovakia. Leaving Germany became increasingly difficult throughout the year, the options fewer and fewer. Golda's brother, Samuel Losner, a brilliant medical student who had been allowed to train in Germany because while a Jew he was also a foreigner, left for Belgium. He would go from there to the Belgian Congo, Brazil, and Cuba before he was permitted to enter the United States. Many years later I asked him how he had learned to speak seven languages: "Simple," he said. "I was thrown out of six countries." Golda's sister, Frieda, married a wealthy man and moved to Argentina; years later, divorced, she would live in Bolivia and Peru. She married again, but in 1956 her husband died in an automobile accident and she came to the United States as well.

Golda and Bernhard had decided to emigrate to Palestine, and spent the late summer and autumn of 1938 attempting to tie up all the loose ends of the move, so that even if they could not take much cash with them they could at least save their belongings to help them start again in Palestine. Bernhard's apartment overlooked the Berlin Sportspalast—the stadium used for the 1936 Olympic

Games, because of its size now a favorite venue for Hitler's frequent harangues. They could clearly hear the speeches over the stadium loudspeakers, and Hitler was most explicit about his hatred of Jews and his determination to get rid of them.

Events following the Munich Pact were swift in coming. November 9, 1938 brought Kristallnacht, a night of terror for all German Jews, during which the Nazi Storm Troopers, SS, and Hitler Youth destroyed thousands of Jewish-owned businesses and all the temples they could reach, killing nearly a hundred Jews and injuring many more. The writing was on the wall. Bernhard and Golda married in January 1939 and set seriously about emigrating to Palestine. There was no time to lose.

The British government required that each would-be immigrant buy a parcel of land to be farmed. The price, a thousand pounds sterling, in today's terms probably the equivalent of fifty thousand dollars, was not trivial, and this probably prevented many from taking this route to survival. Fortunately the new couple was easily able to transfer that amount and then they filled out the paperwork for an exit visa—and waited. Many of their friends were doing the same thing: it was becoming harder to find a country that would admit Jews. While waiting, they shopped for clothes and things that could later be sold, which would all be shipped to Palestine in a freight container. With Jews no longer being able to own property in Germany, they were forced to sell both apartment buildings for next to nothing.

In August 1939 the visas for Bernhard and Golda's friends arrived—theirs was not among them. Someone filling out their forms had made an error. Their friends left Berlin; despairingly, they waited. The next month saw the invasion of Poland. The war had begun, and their dilemma became literally a matter of life and death. Bernhard, with his German upbringing, was not willing to leave without a proper visa, but Golda was determined that they would leave Germany now, one way or another. She convinced him that they needed to go to the Alitalia office, "just to see what's happening." Italy, not yet in the war, offered a possible escape route; others knew this, and crowded the office. Golda saw a man slip an envelope to an agent, who swept it out of sight and didn't ask for a visa. She turned to her husband. "Go home, get enough money to make a fat envelope, and bring it back here." He demurred, as what she was proposing was clearly illegal and his upbringing made him unwilling to employ such means, but she insisted. Some time later he returned and discreetly showed her an envelope; she handed it to the same clerk. Apparently it was fat enough, because the clerk, after deftly tucking it away, asked her politely where she wanted to go. "Palestine," she said, but the clerk said the next Alitalia flight from Rome to Palestine would not leave for a month. "We want to leave Germany as soon as possible. How can we get to Italy?" She and Bernhard were booked on a train for Rome, leaving the next day, and bought tickets from Rome to Palestine. They would wait in Rome for next month's flight.

Hurrying home, they rushed to tell Bernhard's mother and sister their news and to say goodbye. Since they could take very little cash with them, they left their bank accounts in the care of Zippora, who was unable to leave Germany just then. They parted tearfully, hoping that she and her daughter and two grandsons could somehow be gotten out later. This was the last time they would see each other. There were more farewells, to friends and family members, then they packed whatever would fit into two suitcases and set out for the train station next morning.

The train, crowded with families leaving Germany, took two days to reach the Brenner Pass, high in the Alps at the Austrian-Italian border. There everyone had to change trains and have their papers checked. This was not completely unforeseen, but suddenly Bernhard lost heart and wanted to take the suitcases back to the German train. Golda was unfazed, and coolly told the border guard that their visas were in Rome. She showed him their tickets from Rome to Palestine, saying, "How could we get these tickets, if we didn't have visas?" She spoke no Italian, but her Romanian was clear enough. The guard, puzzled, went away to consult with his colleagues. Once again, Bernhard made as if to take the luggage back to the German train. Golda gave him a sharp kick in the shins and muttered, “Stay right there.” The guard came back and pointed to the Italian train. They lost no time getting aboard.

In Berlin they had been given the address of an innkeeper in Rome who would help them while they waited for the Palestine flight. He gave them a room and invited them to eat in his restaurant whenever they wanted to. They were deeply embarrassed by not having money to pay, and promised to repay him when they reached Palestine. Meanwhile they tried to lessen the burden they felt they were imposing by each eating on alternate days. Mostly they ate spaghetti, the cheapest thing on the menu that would not have broken their dietary laws. They were not experienced spaghetti eaters, and thus they struggled to spin it onto their forks, losing a lot of it between plate and lip, much to the amusement of the Italian diners.

They were out of Germany, but still had no visas, a dilemma with barely a month to solve. Golda considered every possible alternative, and finally decided to visit the Director-General of Alitalia. She arrived unannounced at his home one evening, and he agreed to see her. With her background as an administrative assistant to heads of companies, she was not exactly a stranger to imposing surroundings and powerful men, but in the circumstances it must have taken every ounce of her aplomb to carry out her mission. They talked privately for several hours, and at last he agreed that she and her husband could fly to Palestine, even though their undocumented status endangered everyone else on the plane, especially the pilot.

Ton the day of the flight to Palestine, the Director-General was called from his office to the Alitalia desk when the Moses couple arrived, and passed them quietly to the plane ramp. But somehow word reached the pilot that two undocumented passengers were aboard, and he refused to leave the gate. The Director-General intervened, and after several hours convinced the pilot to carry them to Haifa, which was then the airport of entry. There the British flatly refused to admit them. Bernhard and Golda told them everything, how they had bought the required farmland and the mistake on their visa applications, and insisted they had tried to enter the country legally, but to no avail. They were told they were considered German spies and led away in handcuffs to await the next flight to Italy. From there they would inevitably be returned to Germany.

The day before, Golda had telegraphed Betty Losner and asked them to meet their flight, so Betty was there, a helpless witness to their predicament. They called out to her, “Do something! Get us out!” as they were taken away, to be put in separate jail cells, one for men, one for women. Betty was deeply shaken, bewildered, with no idea what to do. She walked the streets of Haifa weeping. This was late on a Friday afternoon, and a man on his way to a synagogue saw her. He asked what troubled her, and she poured out the story of her daughter and son-in-law’s trying to immigrate, the mistake on the visa paperwork, and their arrest. Miraculously, the stranger was with the Jewish Agency’s Haifa office, and he acted swiftly. Even though it was the Sabbath, he was able to get through to someone at the Jerusalem office, who sent a telegram verifying that the thousand pounds required for their immigration had indeed arrived. The next flight to Italy would not leave until Sunday morning because there were no flights n the Jewish Sabbath, so there were a few hours’ grace. The man carried the telegram to the British authorities and argued that it proved these were legitimate immigrants, not spies. At last the British agreed, and released them. The long ordeal, with its terrifying climax, was over.

Soon after, the British changed their policy to restrict immigration of Jews; had they entered without visas then, they would likely have spent the war years interned on the island of Cyprus, even if the British had not sent them back.

Clearly, Golda’s Romanian cultural background saved them. Bernhard, brought up to obey the rules and respect authority in the German tradition, could only admire her. European Jews, with their various ethnic and cultural roots, are different from each other, and their attitudes are summed up in a story my father told: If a German Jew shakes the hand of a Polish Jew, he must then check to see if his rings are still on the fingers of his right hand; but if he shakes the hand of a Romanian Jew, there’s no point looking for the rings on the right hand, he should look to see if any remain on the left hand.

Chapter 2: A New Life in a New Land

Palestine/Israel, 1939–1948

Now that the young Moses couple was reunited with Golda's parents, it was imperative that they take up the land they had bought and learn to support themselves by farming. The land was located within a few kilometers of Netanya, which was then a sleepy town of a few thousand on the Mediterranean coast. Several immigrant families who had bought adjacent lands joined forces to help each other build small homes, and formed a settlement they called Beit Yitzchak (the House of Isaac).

As they began their new life, Bernhard and Golda were in their prime: Bernhard was 34, and while not used to the hard physical work ahead, he was young enough to adapt, perhaps even to benefit from the physical labor. He was of average height and weight, with a quiet, attentive manner; Golda was 26, about his height, and already showing the soft outlines that I remember from my childhood. As they aged, Bernhard would shrink slightly, until Golda was slightly taller than he.

Bernhard knew nothing of construction, but he worked with the other men in building each other's houses; the Losners moved in with their daughter and son-in-law because by now Abraham was too ill to work. Everyone who was able, shared in the work of planting citrus trees and growing vegetables, and each family had a cow and a goat, as well as chickens.

It must not be imagined that this was a kibbutz. That model of immigrant settlement, which would become widely recognized abroad, was based on a Utopian ideal that was not part of the expectations of those immigrants who, like my parents, had the means to buy their own plot of land. While neighbors helped and encouraged each other, essentially the fruits of each family's labors belonged to its own members.

The neophyte farmers in Beit Yitzchak were mostly from Germany, where many of them had been professionals of one kind or another: doctors, lawyers, successful businessmen; earlier Jewish settlers in Palestine were preponderantly from Eastern Europe, rural people from poor shtetls. These settlers considered the newcomers elitists, and expressed their scorn by calling them Yekkes—possibly a corruption of Junker, a high-born (and non-Jewish) German. The new settlers were too busy to care what they were called, and Yekkes they remained.

There were many surprises in the early days of the little community. Golda, who had not experienced country life in Germany, with a mixture of amusement and embarrassment would later tell about the first time she saw their goat plant her hind feet and deliver a stream of pellets onto the ground. Golda rushed inside and came back out with a wad of toilet paper, proposing to clean the animal!

The village formed a cooperative to raise chickens for sale, building a chicken house and installing an incubator. They turned the roosters in with the hens, and waited. They expected that the usual fertilized eggs would result from this arrangement, and when a stock of eggs had been collected they were placed in the incubator. More waiting, but it wasn't working. Finally they called in an expert from Tel Aviv, a *mumche*, who would have all the answers. He checked out the settings on the incubator, fiddled with the eggs—still nothing. After some weeks, the *mumche* asked what they had been feeding the chickens. Why, leftovers from the British soldiers' camp nearby, he was told. "Aha!" he said. "This is why you have no chicks. The British cooks put saltpeter in the soldiers' food." Evidently it had the same effect on the roosters.

Within a year or two, as their own knowledge grew, the farmers in Beit Yitzchak grew more vegetables, chickens, and eggs than they needed for their own use, but they had no success selling the surplus. A couple of people had tried, but had failed to find a market. Bernhard told the cooperative that since he had been a successful businessman in Germany he would give it a try. He had by that time acquired a donkey, called Moritz. He loaded Moritz with boxes of vegetables and he and Golda walked to Netanya to sell them. Soon the problem became evident: the Arabs' produce was much cheaper. Even though its quality was also poorer, the Jews in Netanya were unwilling to pay a higher price to the

Jewish farmers. Bernhard went to the British Army buyers, and promised to supply them with the quality they wanted, in an assured supply. Then he told the Yekkes' cooperative he would accept their produce on consignment, for a fee of ten percent of the selling price, the cooperative to receive the rest. From this modest beginning, by 1948 he would develop the largest wholesale produce store on the central Palestine coast.

By the end of 1940 the first baby, Yitzchak Goldberg, had been born in Beit Yitzchak. I was the second baby, and arrived the next year, about two weeks before December 7, when Japan attacked Pearl Harbor and brought the United States into the Second World War. My mother named me Jesse Joel Moses, Jesse in memory of her late father-in-law Julius, whose Hebrew name was Jesse. It is not common for Orthodox Jews to have middle names, but my mother even then had her sights set on emigrating to America, where, she believed, the name Jesse was not popular then. She may have been right; at any rate, when we came to New York in 1954 I began using the name Joel Moses. To my Israeli friends and my family, and in a synagogue, I am still known as Yishay, the Hebrew version of Jesse.

When I was a year old, the village held the biggest party it had ever had, ostensibly in honor of my birthday. Later, as an adult, this seemed very strange to me, since I had certainly not been the first baby born there. At last I realized that the village was looking for a reason to have a party: the defeat of the Germans in the great tank battle of El Alamein had happened in October 1942 and now Palestine was safe from invasion by the German army in North Africa. My birthday was the first good excuse they could find to express their joy.

The Second World War ended in Europe in May, 1945, and in the Pacific in August of that year. I was only three, and my memories from that time do not include my father's tallying up the names of his own family lost in the Holocaust. Only eight members of his mother's family, the Tarnowsky's, were known to have escaped; everyone else—his mother Zippora, his sister and her two sons, and over a hundred in the extended family—all were killed. On the other hand, and as far my parents knew, all my mother's relatives either remained alive in Romania or had managed to leave Germany in time; some of the Romanian family would emigrate to Israel in the early 1950s. After this my father rarely spoke of the Holocaust, never visited Germany, and refused to buy German products.

One of my earliest memories is of standing with my grandfather Abraham beside the dirt track in front of our farm, waiting to cross. I remember the truck coming, my grandfather telling me not to cross; and then me, running as fast as my three-year-old legs would go, right across the track, with my grandfather screaming after me. He died that year, and is buried in one of the earliest graves in the Beit Yitzchak graveyard. In spite of his failing health, my father said, he used to lead the congregation in the High Holiday services, and some of his prayers were chanted better than any trained cantor my father had ever heard.

After my grandfather died, I was moved from my parents' bedroom to my grandmother Betty's. I would share a room with her until I went to college. I remember our dog, Fifi, who was an incorrigible barker, and I recall as a very small boy calling out to her to be quiet. The one night she didn't bark was the night a fox killed all our chickens. Later, after the chickens had been replaced, I remember going out to collect eggs and having the rooster fly at me, all wings, talons, and beak—in defense of his hens' virtue, I suppose.

My father's store in Netanya was thriving. My mother spent most of her time there, managing the office, and relied on her mother to care for me. My memory of my grandmother is as the wonderful Jewish grandmother of traditional tales: hard-working, totally devoted to her family, and affectionate to her small grandson. She cooked my lunch and supper; she taught me to grow vegetables; and she told me fascinating tales from her life in Romania. Her mother, she told me, was a tenth-generation descendent of the sixteenth-century rabbi of Prague, Rabbi Löwe, also known as the Maharal, who features in one Jewish tradition as the maker of the Golem. This creature, which today we would call a robot, was made of clay and endowed with life through a magical spell; its uncontrollable nature and destructive habits finally required its maker to destroy it. The shivery tale, with its overtones of moral guidance, was perfect fare for a small boy. Two decades later, when I dedicated my doctoral thesis in artificial intelligence to "the descendents of the Maharal who are endeavoring to build a Golem," I learned that several of my colleagues who were interested in robots had been told similar stories by their own parents and grandparents. They had also been told of relationships to the Maharal, which I believed may have accounted for their interest in artificial intelligence. Other famous mathematicians and engineers, among them Norbert Wiener and Rudolf von Karman also claimed to have heard about being descendents of the Maharal.

According to what I've been able to learn of her history, my grandmother Betty was born sometime between 1880 and 1883. She had thus been a young woman in the nineteenth century, and told me of her life then. She had visited a Chasidic Rebbe in a nearby town whose devoted following gave him enough donations to support not only the many young men who came to his court to study the Talmud, but also his own princely lifestyle. The young Betty had been enchanted by this Rebbe and his court.

She also maintained some traditions that I found strange, not to say bizarre, such as regular leeching. I got to witness this, as one of her friends would come regularly to place "bankes" on her back: glass cups, each warmed by a candle and containing a leech, are placed on the patient's back. As the glass cools it creates a vacuum which draws blood to the surface of the skin, convenient for removal by the leech. Although leeches still have a small use in most of the world, they seem revolting to many—including this small, nosy boy.

My grandmother, in spite of her devotion to household skills, was not a very imaginative cook, and worked with a very small repertoire: hamburgers every Tuesday, for example. However, I have never eaten better gefilte fish than the ones she made every Friday as part of the Sabbath celebration, and her cakes were outstanding. I especially liked her seven-layer cakes, because I got to lick the bowl afterward. She baked her own Challahs for the Sabbath, keeping out a piece as Jewish law requires. Preparing for Passover, she would heat enough water to fill our largest tub and then add hot irons to bring it close to boiling, after which all the silverware for Passover would be put in. I used to help her by tying each piece of silverware along a long string, rather like a fisherman's trolling line, and lowering it into the hot water. She also taught me to rotate an egg over my head, transferring my sins to the egg so that I would be pure for Yom Kippur. In Beit Yitzchak each fall she made a *succah*, which leaned against our small farmhouse in celebration of the festival of Succoth; I helped decorate it. I am very fortunate to have these memories.

Of course no one is perfect, and my grandmother had her own less endearing qualities: for one thing, she was fiercely territorial about her kitchen, and allowed no one else any authority there. For a few years we had a servant, a Yemenite girl named Aliza, who helped with the cooking and cleaning, but there was no question who was the boss of the kitchen. If her daughter came in, Betty would start coughing until Golda took the hint and left, a tactic that prevented my mother from learning to cook until Betty was in her seventies and quite frail.

They had other conflicts. My mother was well aware that she was not as beautiful as her sister Frieda, nor as smart as her brother Sam. No matter how she tried to prove her worth to her mother, Golda was constantly being reminded of her shortcomings. Frieda, a year older, with a slender figure and sophisticated bearing, had apparently made a good second marriage and lived what seemed to us to be a glamorous life in South America; her daughter Ruth Kauders would later become a fashion model and an editor of women's magazines. Sam was training as a cardiologist in the United States. Golda, who became plumper after I was born, sat in my father's office all day and kept books. Or at least that was the way my grandmother saw things. It was a great trial to Golda, who had taken both her parents into her home and supported them, and would continue to support her mother for her lifetime.

My grandmother was religiously very observant, one of the first to show up on High Holidays and one of the last to leave. Her books for High Holidays had directions in Yiddish; some of them even pointed out where one was expected to shed tears. She had a small library of books in Yiddish—mostly on biblical figures such as King David and King Solomon—which she read and reread.

My father was also very observant, though not to the extent of his mother-in-law. I enjoyed walking with him to the synagogue each week and on all holidays, but he would leave for his store just before the end of the Sabbath, to be on hand when the trucks arrived immediately after sundown on Saturday. My mother was the least religious of all of us, but she went to the synagogue on all the major holidays and kept a kosher home her whole life. If anyone commented on her lack of fervor she would say that she could not forgive God for what he allowed to happen during the Holocaust.

My father's business was thriving, and for us to continue to farm began to seem counterproductive. My parents sold the farm and bought a cooperative apartment in Netanya, just as I was to enter first grade. I was not quite six, and just recovering from a condition that had made me frail and somewhat sickly for my first five years. It was finally diagnosed as nasal polyps, which not only were a reservoir for germs of all sorts, but also, as they grew, gradually prevented me from sleeping soundly. My mother said I would snore and snort all night, in my struggle for breath. I remember clearly how terrified I was as the doctor swiftly excised the polyps, being careful that the fluids from my nostrils flowed into a basin and were not swallowed. After this I slept well and began to gain weight as my general health improved. In fact, weight gain would later become a big problem, as I evidently shared my mother's predisposition.

On the first day of school my mother and grandmother took my hands and led me there. They assured me that everything would be fine because the principal was a good friend of my late grandfather, but still I cried all the way. I don't know how soon I learned not to dread being separated from my family all day, but I quickly adapted to learning. This was a religious school, and each year we learned a different book of the Bible. It was Genesis in first grade; Exodus in second, and so on. We would in earlier centuries have gone on to study Leviticus in third grade if not the first grade, but now it was thought that the strong sexual nature of some of its passages was unsuitable for young children. The highlight of the first year was at mid-year, when we switched from printed Hebrew to script. Although at home we mostly spoke German, I had no trouble learning Hebrew.

I began school in the fall of 1947, and while of course we schoolboys did not fully grasp events in the grown-up world around us, by the spring of 1948 we too were swept up in the excitement of Israel's proclamation of statehood. This happened on May 14, the same day the British left Palestine. War with the Arab nations began that midnight.

During all of Palestine's modern history it had been dominated by one "Great Power" or another. The Ottoman Empire had ruled it for many years, but lost it to the British during World War I. After the war the League of Nations gave Britain a mandate to rule Palestine. There had been since the early nineteenth century attempts to reestablish a Jewish state in the land once ruled by the Jewish kings, but these were opposed by the Arab peoples who had long resided there. The Balfour Declaration in 1917 stated Britain's desire to establish a national home for the Jewish people in Palestine, but that did not actually settle the issue.

During the rise of the Nazis Jewish immigration to Palestine became a flood. The Arabs protested with strikes and boycotts and formed armed guerilla groups. The Jews formed their own secret organizations intent on driving the British out, such as the Haganah and the Stern Gang. The British attempted to arrange for a partition of the country among its factions, but their plan outraged both Zionists and Arabs. The outbreak of the Second World War ended these efforts. After the war the British mandate was continued, and in 1947 a London conference of British, Arabs, and Zionists failed to agree on a proposal for autonomy for each faction. A volatile situation had become explosive.

British rule had been made even more onerous by the attitudes of some of the British occupiers. Jewish Palestinians largely believed the British favored the Arabs: I heard about a story during the Second World War in which couple who were riding a bus, speaking to each other in Yiddish, were challenged by a man who thought they were speaking German. "If you want to speak the language of our enemies," he said, "speak English." In 1946, in Netanya, three British sergeants disappeared, and the British Army declared martial law in the town. My father sent my mother and me back to Beit Yitzchak to stay with our former neighbors, and the British searched everyone's apartment. We were particularly nervous because our upstairs neighbor, Mr. Peretz, was a member of the Stern Gang. The British did not discover that connection or the sergeants, and continued to search the town, using tanks to level hills, looking for the three men who they were convinced were buried in shallow graves. After several days the bodies were found one morning, hanging in the town center.

The failure of the 1947 conference and many more events like the one I have just described convinced the British to abandon their mandate. They announced in September that they would leave the country on May 14, 1948. The United Nations devised a plan that would partition Palestine into a Jewish state, an Arab state, and an internationally controlled sector that would include Jerusalem. The General Assembly vote on the plan was scheduled for November 29, 1947.

This, as it happened, was just a day before my grandmother, who had decided to emigrate to the United States to be with her son—Sam was now a practicing cardiologist—intended to go by bus to visit the U.S. Consulate in Jerusalem. My father strongly advised her to change her plan, as no one knew what would happen, whichever way the UN voted. He was right. All evening we stayed glued to the radio, following the progress of the voting. When the final vote was in favor of partition, the overjoyed Jews burst out of their homes to dance in the streets. I was too young to join them, but I remember hearing a story about a woman who chided her father for dancing in the street. He danced on, shouting, "We waited two thousand years for this, and you want me to stop!"

The Arabs—to put it mildly—were not dancing in the streets. Their rage immediately expressed itself in violence. The bus my grandmother would have taken to Jerusalem was attacked by Arabs with machine guns, who killed almost all the passengers. Among them was a young couple on their way to be married;

their unfinished house stood as a silent memorial for years as I walked past it on my way to school.

Major uprisings continued during the five months that proceeded May 14. David Ben-Gurion, the leader of the Jewish community, was regularly on the radio urging our Arab neighbors to stay in their villages in spite of the advice of neighboring Arab countries' leaders to leave. The leaders, sure that they would drive the Jews into the sea, promised the Palestinian Arabs they could then occupy the Jewish lands and homes. Meanwhile, they should stay out of the way of the advancing Arab armies. Nevertheless, many of the Palestinians fled the territory that would become Israel. Although there have been analyses that Jewish forces caused many of the Arab villagers to leave so that they would not be in the way of the defense of Jewish settlements, I still believe most of them fled of their own will. Certainly, from either standpoint the prospect of a Jewish victory when faced with the armies of six Arab nations looked dismal.

As the British left their major strongholds, the police stations, during the day of May 14, they quietly told the Arabs when they would leave, so they could take over the stations instead of the Jews. That night at midnight, after Ben-Gurion's proclamation of the new state of Israel, the war began.

Israel, 1948–1954

In preparation for the war, we were taught in our school to hide behind sandbags at the sound of an air-raid signal; at home we were told to stand under the biggest beams. In fact, Netanya was bombed several times by Arabs flying British-made Spitfires. The bombs they dropped never exploded, and were probably of World War I vintage. My theory is that the Arab merchants bought cheap bombs and pocketed the difference. Our army would find the unexploded bombs, place them in a giant vat of water, and truck them out of town to be exploded. Hundreds of us children would use this as a celebration, following a few hundred yards behind the vat and cheering when the bomb exploded.

My father was more seriously engaged in the war: he, our upstairs neighbor Mr. Peretz, and two other men were each handed a gun, a few rounds of ammunition, and a handful of rations and sent out to the hills overlooking Tul Kerem, a large Arab town about eight miles from Netanya. Well-trained Jordanian forces were there, which if they attacked Netanya would slice the Jewish community in Israel into two pieces, one north and one south of Netanya. This would have been a major military setback for the fledgling nation. It was the job of my father and the others to run to the nearest Jewish community on the first sign that the Arabs were attacking, and alert the Haganah, the Jewish army. They were also told—rather unhelpfully, it seems to me—to conserve their ammunition, which was in short supply. After a days or so Mr. Peretz came running back to town, and when questioned said they had been overrun and he was probably the only one to escape. The others, he said, were probably dead. My mother was badly shaken, but bore up while a squad of soldiers went out to investigate. Within a few hours my father and the others came back.

“What happened?” everyone asked.

“Nothing happened.”

“So why did Peretz run away?”

“He got scared.”

“So what *really* happened?”

Well, my father said, the Arabs in Tul Kerem had seen them in the hills, and shot mortars at them, but the shells buried themselves harmlessly in the sand. After awhile the three of them got bored and my father brought out a deck of cards. They played until my father had won quite a bit of money. Mr. Peretz could not take the mortar firing after a while, and then the army came and relieved them.

Later on the Iraqi army did try top attack Netanya, but were repulsed by the Haganah, and that was the closest that Netanya was involved in the war. Other Jewish cities were not so lucky. We closely followed the battles on the roads leading to Jerusalem, and cheered the construction of a new road (the "Burma Road") to Jerusalem during the first cease-fire. Sporadic outbreaks and brief cease-fires continued until January 1949, when Israel and Egypt, Syria, and Jordan agreed on an armistice. This armistice was still in force when my family left Israel to immigrate to the United States in 1954; it is not surprising, given the intractability of each side, that other wars followed, in 1956, 1967, 1973, and 1981. But it is surprising—indeed, a miracle—that peace treaties have been signed with Egypt and Jordan. Given the history of Arab-Jewish relations, the situation as this is being written leads me to be optimistic for the long-term future, although I recognize the very serious difficulties of the present.

The founding of the new State of Israel brought a massive inflow of immigrants from many countries. Two of my mother's cousins, Golda and Sarah, emigrated from Romania to Israel. It was difficult for them at first, but both eventually made a successful transition. I was very proud of Golda's son, who became a substitute goalie for Netanya's semi-pro soccer team. Soccer games were played on Saturday, which presented the kids from my religious school with a dilemma. We solved it, after a fashion and using the devious rationalizations of young boys, by not buying tickets on Saturday but waiting for the second half of the game and climbing the fence. I wonder which offense was the greater in God's eye.

It was probably around that time that my grandmother learned of the last days of her own mother, with whom she had lost touch during the chaos of the

1930s. Her mother lived to be 103, and although she had become almost completely blind—perhaps from macular degeneration, a progressive condition that afflicts those of great age, or perhaps from glaucoma—she wanted to see her family once more before she died. The family brought her to the office of a doctor in Bucharest, who seated her in a chair, lined them up before her, and then shone a powerful light into her eyes. For a few brief seconds she was able to see her family, my grandmother was told. Since my grandmother was somewhere around 70 herself at the time, this must have been tremendously cheering for her to hear.

One effect of the immigration was that our school population was growing every month. When I was in third grade they split our class into two. I didn't know until much later that there is a Talmudic rule that classes over fifty (Maimonides says forty) must be split in two. My best friend in all my school days in Israel, Ze'ev Magidi, went to the other class. I unkindly told him that I thought the better students were placed in my class. I kept my friendship with him, however, because we shared some subjects and played soccer nearly every day. I was not a very good player, being overweight and not terribly well coordinated, but I had to be on the team, since I owned the only ball in the neighborhood. I suppose I must have thought this showed that my father was more successful than their fathers—not an attitude that would have endeared me to the others! Ze'ev was a year older, having been held back in the first grade, and sometimes he played the role of older brother, saving me once from some secular-school kids who wanted to grab my yarmulke. One day we were sent home for talking too much in class, but I suggested that instead of going home we go to a lot my father owned near the new high school and dig a hole. This seemed a good idea, as we had seen one of our classmates doing the same thing next to his house. We were sitting contentedly in the dirt, digging away with our hands, when to my embarrassment a high-school girl from my coop apartment house went by with a troop of other girls, and eyed me speculatively. When I recall that small incident I also remember, sadly, that she joined the Israeli Army a year or two later, only to die when she was thrown from a jeep that took a corner too fast. Traffic in Israel was extremely hazardous then, for civilians and military alike; one of my father's best employees was killed crossing Netanya's main street.

Under the new Israeli government life changed in many ways, beginning with the conscription of all able-bodied men for some form of military readiness. My father, whose military background was limited to that one foray into the dunes, was made a captain in the reserves, to the amusement of his family. He was a bit clumsy, but unprotestingly led the fellow reservists in his company every two weeks, practicing marching and target-shooting.

The government also became competition for my father's wholesale produce business. He was a member of a food wholesalers' association called the Teneh; the government store in Netanya was part of a rival association, the Tnuvah. The Tnuvah had access to the kibbutzim and was able to benefit from its larger size, but my father's business continued to have the high regard of the community. He had not abused the near-monopoly status he had enjoyed before the war, and he was greatly respected now.

It is hard, in the United States in these days, to imagine the value that a family could place on a dozen eggs when the ration was one egg per person per week. The black market flourishes in these conditions, but my father's strictness in ethical matters meant he was never tempted to join it. My mother was able quietly to put away some of the egg revenue for herself, she confided years later, which she used to send Betty on the occasional vacation. It was only many years later that I began to realize the tremendous influence my parents' backgrounds had on me, especially the German background they shared. I think I meld American emphasis on individualism with European, especially German, emphasis on the community.

I was doing well in school, in the second grade and enjoying it, but once (only once!) my mother was called to come discuss my performance. The teacher said my handwriting was terrible, and needed improvement. After some discussion and an examination of my written papers, they agreed that the first remedy would be for me to sharpen my pencil more often. My handwriting has unfortunately not improved, and the transition to English didn't help it: writing from left to right is a poor situation for left-handers. I describe my writing today as Israeli hieroglyphics. I was lucky that the teachers in Israel as well as the US did not try to make me write with my right hand. I heard that it was tried on King George and may have caused him to stutter. I believe that handedness is a physiological phenomenon, and may lead one to feel like an outsider in a group of right-handers.

By third grade I found myself increasingly embarrassed by the teachers, who would praise my performance in front of the class. The geography teacher said she had never seen a better exam paper; a religion teacher said that the question I had asked was so interesting he would ask it of his Rebbe; our homeroom teacher was astonished that I knew the meaning of some unusual Hebrew word. Beginning in the fourth grade teachers asked me to help grade the other students' math papers, and although later one of the girls would offer some competition, I had the opportunity to make more A's than she did, since boys in our school were required to take more religion classes than girls.

Still, I didn't feel I was a "class star." Ephraim—*there* was the class star, in my estimation. He had two claims to fame: first, he was the champion marbles player. He'd come in the morning with a small bag of marbles and leave that afternoon with a big bag of them. We played incessantly, before class, during lunch, and a bit after school, although most of that time was reserved for soccer. I lost to him over and over, and I guess we played so often because I was the only one who could afford to lose so many marbles. Ephraim's other claim to class stardom was that he had appeared in an American film. One day, the principal announced that an American film company wanted two third-graders from Netanya to appear in a movie. Ephraim was chosen from our school, and the second boy came from the secular school in Netanya. They were excused from attending class for several months, and when Ephraim returned we all gathered around him, starry-eyed. "What was it like?" we wanted to know, but all I remember of what he told us was that the lights were very strong, and the crew used mirrors to shine the light on the actors. I never saw the film, and Ephraim never lost the distinction of being our movie star.

When I was eight I developed headaches that wouldn't go away. The Netanya doctors X-rayed my head and said they didn't know what it was. My father took me to Tel Aviv to visit one of the Zondek brothers, Hermann, whose examination consisted largely of running his hand along the soles of my feet and telling his nurse a list of conditions I would likely suffer from later in life. He looked at the X-rays, and concluded that I did not have brain cancer and should lose some weight. Although losing weight was as good advice then as it was decades later, it didn't have much bearing on the headaches. My father never brought up the possibility that I might need glasses, because he had always boasted that no one in his family had ever worn them. (Of course this didn't mean they didn't *need* them, but that's another issue.) Many years later, my father would wear reading glasses; of course within a few years I was wearing them for my nearsightedness, although I continue to believe that early correction would have helped.

My father was so concerned about my weight that he enrolled me in a summer gymnastics program. While trying to jump over the vault, I fell and hurt my right wrist. I cried from the pain, but the instructor snapped at me that boys don't cry. After class I ran to my mother's cousin Sarah's apartment and cried some more. When I went home my father repeated the admonition that boy's don't cry. After a day of pain my grandmother said enough was enough, and took me to a doctor. He reset my broken wrist, and my arm was in a cast for the rest of the summer.

The next summer my parents made a better choice, and enrolled me for individual swimming lessons. This skill has stayed with me, and I continue to enjoy it. My favorite stroke is the breast stroke I learned then. During the other summers in Netanya I usually went to day camp. I loved those camps. They were in the midst of woods, and the smell of these woods will always be with me. We would go to the seashore nearly every day. Netanya's lovely beach has since made it one of Israel's major resorts, but in those days there were only two unremarkable hotels near the beach. One day I built a sandcastle that was so good, the camp counselors photographed it. Another day we practiced putting up tents. Toward the end of one summer prizes were awarded for various achievements, and for the first time a prize was awarded for checkers playing. I had lost only one checkers game all summer, and was declared the camp's checkers champion. After my initial flush of triumph I looked at my prize and complained that it wasn't as big as the one for the 100-meter dash. My counselor pointed out that the 100-meter dash was the key competition, and the only reason they had added one for checkers was because I had complained about never winning any prizes.

My mother and I took summer vacations in various parts of the country, and my father would join us for a few days during these trips. One of them took us to a new Dan Hotel in the town of Herzlia, a few miles south of Netanya. Although in my opinion the beach was not as nice as Netanya's, my mother stayed out on it long enough to be so severely sunburned that we had to call a doctor to treat her. She most enjoyed the breakfast, our first experience of being allowed to eat all we wanted in a restaurant. Another time we went to a *pension* near Jerusalem, where there was little to do other than enjoy the mountain air and eat. They had a European-style dining room, with breakfast, an eleven o'clock meal, lunch, tea at four o'clock, dinner, and late supper. My mother loved it.

About this time my mother began to worry about her weight. In addition to her love of good food, there was doubtless some genetic predisposition to weight gain, and I showed the same tendency. My father never had a weight problem, so I assume I inherited it from her. She began seeing weight-loss specialists, but instead of changing her diet they prescribed a series of medications. In the 1940s and 1950s this approach was fairly primitive, often involving such stimulants as Benzedrine; whatever they prescribed for my mother, their main effect was to make her jumpier.

She also had a favorite hairdresser in Tel Aviv, and dressmakers who supplied the latest French styles. Her hairdresser would turn up again, years later: when I was in high school in the United States, my friend Ira Sherman showed me a LIFE magazine layout on the ten most beautiful girls in the world. One was an Israeli soldier, and Ira asked if I knew her. I protested that of the thousands of girls her age in Israel, it was very unlikely I would have known this one. Not long later my mother got a phone call from her Tel Aviv hairdresser, who was staying at the Waldorf Astoria. The girl in the layout was her daughter, who used to keep me company while my mother got her hair done. She was then only twelve, and I was even younger. After the photo appeared El Al used it to promote travel to Israel, and a young man from Louisiana fell in love with her photo, wrote to her, and eventually convinced her to marry him. The mother was on her way home from the wedding in New Orleans, and must have looked my parents up in the New York phone book. We found the story amazing, and so did Ira when I told him.

As I was starting fifth grade, my mother went to Yom Kippur services and became so nauseated that she had to come home early. The doctor came, informed her that she was pregnant, and told her to stay in bed for most of the pregnancy. This unexpected development meant that she could no longer work in my father's office, and although by that time we were financially better-off, we still lived in a small apartment, so it could not have been altogether good news. My mother was thirty-eight, and it had been ten years since there had been a baby in the house. My parents had not wanted me to be an only child, but two failed pregnancies since my birth had discouraged them. As I learned much later, during the Second World War my mother had also had an abortion. Her difficulties during pregnancy may have had something to do with my brother's later illness. When he was born in May 1952 he was named Abraham after my late grandfather, and was given no middle name. I remember that the hospital swarmed with people during my brother's Bris, the Jewish circumcision ceremony. He cried during the operation, but quieted when the drops of wine were placed on his lips. When the baby came home he joined my parents in the front room of our four-room apartment, which served as a bedroom and living room, while I continued to share my grandmother's room. The ten years' difference in Abe's and my ages prevented us from ever becoming close; other parents of widely-spaced children say it's like raising two families.

In 1953 we celebrated my grandmother's seventieth birthday with the biggest party I can recall in Israel. Actually, no one, probably not even my grandmother, was sure of her age. Record-keeping had been very poor in Romania when she was born, and I also suspect that she may have been a bit older than her husband and embarrassed to admit it. My parents outdid themselves to make the celebration a grand affair, inviting dozens of people, many more than could fit in the apartment. Since we lived on the ground floor, the guests could spill out onto the yard, which made it very lively and informal. We had caterers from the local hotel, and gave them extra provisions from the store. I don't recall any music, but I do recall my grandmother beaming at all the attention she was getting.

That autumn I entered sixth grade. I was not quite twelve, a bit younger than the others, who were beginning to have their Bar and Bat Mitzvahs. The girls celebrated their Bat Mitzvahs when they turned twelve, and though I don't recall going to any of the services, their Saturday night parties were memorable. They were usually held on the roof of someone's house, and featured eating, singing, and dancing. While I joined enthusiastically in the first two, I was too shy to dance. One night I walked home with Ze'ev Magidi and one of the best-looking girls in the class, Aliza. I was surprised that she wanted to talk to me; she even seemed to like me. Ze'ev's Bar Mitzvah occurred that year, with his relatives celebrating in his home afterward. I was the only boy invited, and sat next to him. There were so many people, and the room became so hot, that when his father opened a bottle of beer some of it ended up on the ceiling.

A major event in sixth grade was the start of foreign language instruction. We began with English, and for those of us who would go on to high school there would also be Arabic and French. High school was optional, and was not free; attendance was free and compulsory only through eighth grade. Our English teacher was Australian, and one of the songs she taught us was "My Darling

Clementine." Sometimes I think my polyglot English accent retains a hint of Australian English.

At about that time, the government declared an emergency, referred to as Tsena. Just before Tsena took effect I went into town with my mother and grandmother to try to buy household items, because the emergency would impose additional taxes as well as food rationing. Everyone else must have had the same idea even earlier, because the stores were almost empty of goods by the time we showed up.

Sometime during that fall, I went alone by bus to Ramat Gan, outside of Tel Aviv, to see the Maccabia Games. The previous year they had been held following the Olympic Games, and had featured Jewish and non-Jewish athletes from the Olympics. These games were opened by Ben-Tzvi, the President of Israel, and Prime Minister David Ben-Gurion. There followed a spirited rendition of the national anthem that sent chills up my spine.

In seventh grade I fell during a basketball game during the lunch break, landing on my left hand and breaking the wrist. I didn't cry this time, and there was no time lost in getting me to the school nurse and then to the doctor for a cast. Later that year we had a grammar quiz, and although everyone expected me to do well, I flubbed the question. The teacher offered the ingenious excuse that the cast had made me miss it.

Unknown to me, during that school year my mother was trying to convince my father to accept her brother Sam's invitation to come to the United States. Sam had visited us in 1952, and pointed out that I would get a better education there, and that given his success in both Germany and Israel, my father should do well there too. Shortly after Sam's visit, Frieda visited us from Peru. My mother was made even less content with Israel by seeing the fashionable clothes her sister wore. Golda had never been happy in Israel. She longed for the Berlin of her youth, and the wealth she and Bernhard had enjoyed in the Nazi era. She longed for life in New York, a home with one of the elaborate kitchens shown in the American magazines. Most of all, she felt that having given fifteen of her best years to building up Israel was enough, that the fighting with the Arabs would never end, and she didn't want to give one and maybe both of her sons to the wars.

My father, a successful businessman at 49, was doubtful of his ability to start again in a new country. He resisted his wife's arguments, and those of his mother-in-law, who wanted to go see her son and meet two of her other grandchildren, and it wasn't until my mother got me to add my entreaties that he finally agreed. Preparations to leave were made throughout the school year, in secret because the Israelis viewed such departures as close to treason.

My mother hired an English tutor for me, who began by having me read the Little Golden Books, which are appropriate for five-year-olds. I also began practicing for my Bar Mitzvah, which would take place in the United States. We had to bring the teacher into our confidence, since the version that would be read in the United States, and which is part of the Ashkenazi tradition, would be different from the Sephardic version used in Israel. The cantor in Netanya's main synagogue remained unaware of our plans, and I heard him boast that the introduction he would give me on my Bar Mitzvah would be one of his best. The cantor knew me because he was our music teacher as well. My mother also had a dozen pairs of short pants made for me. This would be a cause of intense, but mercifully brief, discomfort for me, as neither of us knew that in the United States teenaged boys in the 1950's wore long pants. We donated our furniture to Golda and Sarah, my mother's cousins.

My father had sold his business to the Teneh association in 1952, staying on as its manager. It made it much easier to wind up his business affairs. He settled his taxes with the government and had a lawyer manage our apartment, as well as the plot of land where I had once tried to dig a hole with my hands. The apartment was soon rented at a rate that would pay the taxes on both. The weekend before we were to leave the country he met with the man he had selected to be the next manager at the store, and told him that he had recommended him to become the new manager.

We slipped out quietly at five in the morning to a waiting taxi. I had wondered just where I would turn for a farewell glimpse of the apartment house, but then the taxi rounded a corner and the house was gone from view. We went to Haifa and boarded a venerable, creaking tub named the *Jerusalem* headed for New York via Greece, Spanish Morocco, and Halifax, Nova Scotia. This, it turned out, would be its last voyage before it was scrapped. On our first day at sea I wrote a card to Ze’ev Magidi telling him that we had left for the United States. I didn’t hear from him for another thirty-three years.

When the ship stopped at Piraeus, Athens’ port, I got off to walk about a little. I saw a shoeshine boy and got a shine, but since I had no idea what to pay him I held out several coins in my hand, and he took his pick. He probably took ten times his normal fee. In Spanish Morocco my parents and I went to an outdoor café, where we overheard Moroccans discussing going to Israel. We asked them when they were leaving, and they said in a couple of weeks. It turned out that they were taking the *Jerusalem* on its return voyage, and when we warned them about the condition of the ship they just shrugged. They seemed eager to leave the country, so we wished them well.

It took seven days to get across the Atlantic, long enough that we got to know some of the other passengers. One boy, a tap-dancing whiz, put on a show nearly every day. My brother, two years old now and very cute, was a big hit, especially with the women. There was gambling aboard, which I’d never seen before. One day some dolphins swam alongside the ship. Much of the time my father and I sat on deck, reading and looking out to sea. While this was calming and very pleasant, it also left big rust-stains on our shirts, because maintenance standards had become very loose and the old ship was oozing rust. We had ruined about a dozen shirts by the time we noticed it, a serious dent in our small wardrobes, but the ship’s owners reimbursed us later.

Our next stop, Halifax, was only a day or so from our destination, New York, so on the last night aboard ship the adults were invited to the traditional Captain’s Dinner. There had been a lot of excitement and anticipation, the women bringing out their best dresses for what was sure to be a memorable event. Unfortunately that’s when we ran into Hurricane Carol. A hurricane in the North Atlantic is about as memorable an event as anyone could ask for, and given the condition of the ship we were lucky that the captain was seasoned and competent. The ship rolled and struggled in the troughs of giant waves, then roared through their crests with a sickening lurch. We had orders to strap ourselves in our bunks, but I couldn't forget the discussions of the Captain's Dinner. With the stubborn optimism born of ignorance, I set off to have a peek, crawling along the halls and up the companionways toward the dining room. It took about half an hour to get there, and to my disappointment it was empty! Now I had to crawl back to my bunk. I finally made it and strapped myself in, but not before I had left several meals along the halls. In the morning I learned that everyone else had stayed obediently strapped in, but most of them had been as sick as I was.

Chapter 3. Junior High, High School (1954–1959)

On September 1, 1954, a perfect autumn day, our ship sailed into New York Harbor. The five of us, my grandmother Betty Losner, my parents, my two-year-old brother Abraham, and I, crowded at the rail with hundreds of other eager passengers, were rewarded for our long journey by an unforgettable sight: Liberty Enlightening the World, holding up her torch, I felt, to us in particular.

There had been a surprising problem with our arrival. In spite of getting us through the storms and safely headed for port, it turned out that the captain didn't realize that August, like July, contains thirty-one days. Our arrival date had been given as September 2, and of course this meant we had to hurriedly telegraph my mother's brother, who was to meet us, to do so a day early. We found Uncle Samuel on the dock, then located our luggage, heaped in a helter-skelter fashion. My father nearly had apoplexy before we managed to round it all up. At last he was satisfied that nothing had been lost, and Uncle Sam hailed a taxi for the trip to Brooklyn, where he had rented an apartment for us. During the taxi ride he gave my father a savings account containing about six thousand dollars, which my father had transferred from Israel to tide us over while my parents found work. My father controlled himself when his brother-in-law told him that the gift packages he had sent us in the past year had been paid for out of this money. Although these CARE packages had contained food, such as salamis and coffee, difficult to obtain during the Tsena period in Israel, I saw that my parents would gladly have done without these small luxuries and kept the savings account intact.

We found ourselves in a furnished basement apartment, dark and very small, which would have to do until we had settled into our new lives. There were four rooms, similar to our coop apartment in Netanya. The biggest shock to me, used to the sweet well-water of Netanya, was my first sip from the kitchen faucet. The famously bad "Brooklyn water," laced with chlorine, nearly made me retch.

After a night spent in exhausted sleep, the first order of business was to buy food. My grandmother got directions to a store—probably from my uncle—and she and I set forth. In those days in Israel, as in Europe, one store had sold dairy products, another vegetables, and so on; our first experience of the American supermarket (which was in fact a "superette" by today's standards) was astounding. The variety and the elaborate packaging convinced my grandmother and me that the tales of America's wealth were true.

A few days later my father and my uncle took me to the Flatbush Yeshiva to be enrolled. The head of the yeshiva listened to our story and said that I could not be enrolled. My uncle pointed out that I spoke perfect Hebrew, but the head of the yeshiva replied that I spoke hardly any English. I needed to spend a couple of years in public school, he said, where I would learn English, and then I could be reconsidered. My mother and I went the next day to enroll me in Andreas Hudde Junior High School, in our neighborhood, and I was enrolled in eighth grade, as I would have been in Israel. The counselor asked whether I wanted a concentration in art or in music, but my sketchy English vocabulary let me down: When my mother asked me what the question was, I said the choice was between shop and music. I chose shop but wound up in art. They asked whether I wanted to learn French or Spanish; I asked for German or Hebrew, but they offered neither. My mother said she had learned French when she went to school, so I chose French. It wound up being my hardest subject for the next four years, since I had difficulty juggling various languages simultaneously; speaking German at home, remembering my Hebrew, learning English, and then trying to learn French on top of all that.

At the age of nearly thirteen, the great transition my family had decided to make was exciting to me, but I had no strong feelings either way. In Israel my mother had tried to paint a glorious picture of what I should expect in New York; the reality was something else. My parents had no jobs, and I did not really speak the language well enough to get along in school. After a few weeks, however, my parents found jobs at the minimum wage, eighty cents an hour. My parents worked very hard. My father, who had owned a scrap-metal firm in Germany, found a job loading metal tubes onto trucks, not easy for someone who was nearly fifty years old and unused to physical labor. My mother, who before marriage had been a multilingual secretary, worked in a candy factory packing candy-boxes. My grandmother Betty cooked, cleaned, and took care of my two-year-old brother, spending a great deal of time shopping in order to keep the grocery expenses down. It was my job to succeed in school.

I believe the teachers really tried to help me in that first year. I went to speech class once a week, and thus missed half my French classes. The best I can say about French was that the teacher gave me my lowest grade ever, but 65 was still a passing grade. One of the students who lived near our apartment took me under his wing for the first few months. The most difficult class was social studies, which required a fall term paper. I struggled to write just three sentences about Andrew Jackson. It was so difficult that I actually shed tears in that class. Surprisingly, the English class was not that hard, since they emphasized spelling that year. I could memorize the spelling of words with the best of the others. I thus think I received an 85 that term, not bad when you consider I barely understood the words I was spelling out.

The mathematics class was a revelation. The emphasis was on fractions; in Israel we had studied fractions in the sixth grade. I did not complain about the advantage I had, but I was left with an indelible feeling about the level of math and science education in the United States, knowledge which I would use a quarter-century later, on President Carter's Committee on Mathematics and Science Education.

In art class I was teamed with Bruce Fisher, who wanted to become a fashion advertising artist, and Ira Sherman, who wanted to become an architect, like his father. Our project, Ira decided, would be to draw the City of Tomorrow, based on the 1939 World's Fair view, which assumed that people would have personal planes or helicopters. This appealed to me because of my interest in Jules Verne, many of whose books I had read in Hebrew translation. We spent a great deal of time on this project, the teacher was getting more and more impatient with us, and finally I tipped over a bottle of India ink on it. I was mortified. We cleaned it up the best we could, and Ira took it home.

In spite of this rocky beginning, Ira and I became good friends. We used to eat lunch together, initially with Bruce, which is when I learned about their career dreams. I expected to become a doctor, just like Uncle Samuel, and hadn't considered what that might mean monetarily. I was surprised when Ira pointed out that he expected to make $10,000 a year as an architect. This was much more than my parents were making, with both of them working. Looking back, I can see that we were poor—but at the time the economies practiced in my family seemed quite unremarkable.

My Bar Mitzvah occurred three months after our arrival in Brooklyn. Although I had started in Israel to practice chanting the Haftorah (the selection from the prophets in the Old Testament that is read each week), I went to the Orthodox synagogue on Kings Highway in Brooklyn for additional practice. I must admit that my most important memory of that study is that this was where I first ate bagels with cream cheese and lox. There may have been bagels in Israel, but I'm sure we never had lox. It would have been an incredible delicacy, unavailable during the Tzena period.

My parents invited a few friends and family to the synagogue on the Saturday when I was to chant the Haftorah. I was surprised to see another boy also celebrating his own Bar Mitzvah. He told me as we were sitting on the dais that he figured I would do a better job than he. I admired the book he had for his Haftorah, and he loaned it to me when he was done. I chanted the Haftorah quite well, impressing the congregation (I was later told) with my Israeli accent, but when it came time to say the final blessings I could not find them in the other boy's book. Eventually the Rabbi had to point them out to me so I could finish. This is a painful memory to this day.

My parents had ordered a modest lunch afterward in the synagogue. We were a small group: my Uncle Samuel, my father's cousin Lottie Brieger, her husband, a photo retoucher, their son Fred, and a close friend of my uncle's, Marcel Halpern, a Wall Street investor. It was Halpern who had helped Samuel get out of Germany, and who, helped by his brother in Israel, had placed my family's money in the account in the United States, so my parents were determined that he see such an early return on his efforts. I was congratulated by all, my mistake ignored. But I recall being unhappy that we had such a small event, certainly compared to the ones I attended in Israel.

As all the adults in our family cooperated in improving our situation, we were soon able to move into a second-floor apartment on Nostrand Avenue, which boasted two bedrooms as well as a living room. The key feature was a small DuMont television, a great help to me in learning the language. My vocabulary was quite small still, but toward the end of the school year I could carry on a conversation with the other students. While the rules governing football were still beyond me, I had mastered many of the intricacies of baseball, which in those days of the Brooklyn Dodgers battling the New York Yankees in what were called "subway series" became a passion of mine.

During the summer of 1955, while Uncle Samuel and his family vacationed I worked in his office, placing electrocardiograms in their proper files. After he returned he called me into his office one day to show me his latest invention. He had me lie on the examining table and placed a jar filled with some kind of oil on my chest. He turned out the lights and used a flashlight to look at the surface of the oil. He told me that he could tell if a patient had had a heart attack by the motion of the surface waves. He asked what I thought about it and I said it looked pretty simple, which didn't please him at all, although I still think it was a good answer. Many great ideas look simple when one finally understands them. A few years later, after Uncle Sam had died, a group of Russian doctors visited his hospital in Brooklyn, and raved about this invention. My son, Jesse, actually found Uncle Sam's paper on this invention in the Harvard Medical School library. Nowadays there are many other approaches to analyzing the operation of the heart muscle.

The ninth grade, the last one in junior high, was also the freshman year of high school, a system that eased the transition to high school. The teachers told us that they wanted us to perform well so that our junior high would look good to the high-school teachers. My performance improved greatly that year and I recovered my position as the best student in my homeroom class. There was actually a vote on this matter during the year. I voted for Ira Sherman and he voted for me, but I won. I was not close to being the best student in the ninth grade, since we had several classes of high-IQ students who had skipped the seventh grade. Most of the ones who did well on the entrance exam in the sixth grade went to the exam school in Brooklyn, Brooklyn Technical High School, but many chose to stay in their local area. None of them were in my classes in junior high, but I knew about them—they were called SP students, for Special Progress. I believe that their IQ had to be at least 132. I had taken an IQ test shortly after we arrived in the States in the eighth grade, and again in ninth grade, but these scores were not shared with us, although they had to be relatively low, due to my lack of knowledge of English.

Math was by far my best class that year. We were learning elementary algebra that year. I had encountered algebra in Israel, and found it engaging and satisfying, so it was not surprising that I did well in it. Our homeroom teacher, Mr. Sternberg, taught us social studies as well as art. He knew little about art. To impress him Ira and I made papier-mâché dolls and acted out a skit in French. He liked the French element, so we got a good grade in art. One day we were studying geography, and I asked him the name of the capital of Turkey, knowing that it was Ankara. He fell into the trap and said that it was Istanbul, the largest city in Turkey; I said that it was not, and when the encyclopedia was consulted Mr. Sternberg was quite embarrassed. Two years later I was shocked to read that Mr. Sternberg had committed suicide. I was sixteen by then, and stricken to think that my question might have contributed to his low self-esteem, and though I no longer believe this, I have regretted ever since not being kinder to Mr. Sternberg.

This year I took several new shop classes, and among the usual projects for boys were several surprising ones: sewing a shirt from a Simplicity pattern (there was no chance that anyone could ever have worn mine); crewelwork and cooking (I still recall how to make deviled eggs and set a table); the latter in a class for boys only. Our cooking teacher was a lady named Tom: her father had wanted a son, she said.

My parents were also going to school at night to learn English. My father told about the woman who was asked the meaning of the word “garbage.” She retorted, in a tone implying that any sensible person would know this, “Garbage is garbage,” which my father thought very funny. He and my mother each received a junior-high-school equivalency certificate that year. My mother went on with her studies and later obtained her high-school equivalency certificate, although my father did not.

Living in the United States had affected on our religious lives. We continued going to Orthodox synagogues, where men are separated from women, although my mother increasingly complained about the separation. We also saw many people whom we knew to be members of our congregation driving their cars on Saturdays. In Israel it was relatively easy to avoid driving on Saturdays. First, hardly anyone had a car. Second, hardly anyone who had a car drove on the Sabbath. Finally, on Saturdays the stores were closed anyway. We never had a car in New York, and walked or took the subway to shop. Eventually we succumbed to the attractions of the city and joined Conservative synagogues that permitted my mother to sit next to my father and me. Such congregations also did not question driving on the Sabbath.

During this year my grandmother announced that she was leaving us to join her son Samuel’s family. This forced my mother to quit her job in order to take care of my brother. But Samuel’s wife, Shirley, refused to let him install Betty in their home, so he rented an efficiency apartment for her near his house. After a week or so my mother and I visited her and convinced her to return to our place. My mother was thus able to achieve her goal of once more being a bilingual secretary, using both her English and German in a new job.

A few months later Samuel became very ill. Ironically for a heart specialist, he had developed clogged coronary arteries. We visited him in the hospital, but he was in an oxygen-rich environment and could barely communicate with us. In a time when today's sophisticated procedures were not even a dream, he died a few weeks later at the age of 45. His death had a great impact on me, due not only to my sorrow at our loss: Uncle Sam had been chubby; I was already beyond chubby, and became worried about my own health.

At the end of the school year my mother, grandmother, and brother attended my junior high school graduation ceremony. It would be the first of several such ceremonies for me. I was frustrated that summer because at the age of fourteen I was legally able to work, but I had no job. I read books, watched television, and walked to Ebbets Field for some Brooklyn Dodgers games. The World-Champion Dodgers were on their way to another World Series in which they would face the New York Yankees. When they finally won in 1955, one could not sleep that night in Brooklyn because of the noise created by the continuous honking of car horns. Ebbets Field games were a form of theater, with an orchestra that played "How Dry I Am" whenever Leo Durocher, the Giants manager, had a sip of water. The bullpens were along first and third base. My father and I would try to get seats along the third-base bullpen line, from which we joined others in heckling the opposing relief pitchers. I remember seeing Sandy Koufax as a "bonus baby" acquiring much-needed seasoning. We enjoyed watching him practice his pitching, and keeping the catcher jumping up and down in order to catch the errant throws.

The summer of 1956 also saw the nationalization of the Suez Canal by Egypt, under Gamal Abdel Nasser. Despite the long-standing requirement of free passage to all, Israel had been denied its use since 1950; now Israel, with air support from Britain and France, invaded Egypt. British and French troops followed. President Eisenhower forced all three armies to withdraw. An armistice brokered by the United Nations allowed the canal to be freed of sunken ships and other war wreckage, but did not clear away the remaining political problems. The impact of this withdrawal on the British, French, and Israeli governments and publics was very significant. My family, who had followed these events closely, felt that it showed the British public that their nation was no longer a first-class military power; my mother used the occasion to argue once again that in leaving Israel we had made the right decision.

Like most high-school students, however, I placed world events, even those affecting Israel, in the background as I concentrated on my own immediate future. For sophomore year, 1956–1957, I went to Midwood High School, just across the street from Brooklyn College. With a thousand students in each class, the enrollments were far larger than the school's capacity, and we shared time with the other grades. The sophomore class came about noon and left after five; the junior class came about ten in the morning and left around three, and the seniors came at eight in the morning and left around one. In the masses of students, I still had no classes in common with the SP students.

Math that year was plane geometry, which I enjoyed; our science that year was Biology and we had to dissect a frog—*not* so enjoyable. French was still my toughest subject. One day the French teacher, possibly going against policy, pointed out to me that while my first IQ test was about 50, my second was about 100. I believe she thought this was due to my earlier language difficulties, and believed I could do better yet, so she recommended that I take the test again. Eventually I would take an IQ test four times in the Brooklyn public schools.

All through my years at Midwood we lived in an apartment over a candy store. It had three rooms, a kitchen, a bedroom, and a living room that converted to a bedroom. My grandmother and I slept in the bedroom, and my parents and my young brother slept in the living room. Our next-door neighbors, the Sperlings, had two sons, Philip and Arnold, both a bit younger than I, whom I occasionally helped with their homework. My mother and Mrs. Sperling became friends. My father liked to take walks with Mr. Sperling, who worked in a store that made and sold mink coats, and who was taking an evening course on the Talmud in Brooklyn College. I recall walking one evening with my father and Mr. Sperling, who pointed out that the Talmud was being written and edited as late as 500 AD. I brashly disputed such a late date, and he became upset at my challenge. He was right, of course.

At about this time my father got very involved with the process of obtaining reparations from the West German government. He applied for himself, for my mother, for my grandmother, and later for my aunt Frieda. The initial applications were for loss of a profession, both for himself and for my mother. In addition, he applied for payment for the loss of the container with their clothes and furnishings, left in Germany upon their escape in 1939. I believe that the payment in the latter case was a penny on the dollar of valuation. He could not apply for the loss due to the distress sale of the apartment houses, because those houses were in East Berlin, and thus now in East Germany, which at that time refused to pay reparations. The payments, equivalent to pensions given to German government workers, would not begin to arrive for some two years, at which time my father would receive the highest-level pension, and my mother and later my grandmother lower-level ones. It would be a great help as my parents continued to establish the family in its new home.

My father and a partner formed K&M Metal Company in 1957, a scrap metal firm in the Bronx similar to the one he and his father owned in Germany. I used to visit it, and my father would show off the huge electromagnets that they used to lift ferrous metal parts. He was particularly scrupulous in the matter of buying copper pipes, which were frequently stolen from buildings under construction and sold at scrap-metal prices. I believe that my father sold the co-op apartment in Israel as well as his quarter-acre of land there, to finance his share in K&M Metal. This cut us off from Israel almost completely. My parents would still get a few letters around the Jewish New Year, but there was little contact otherwise for many years.

That year we learned that my aunt Frieda’s husband had died suddenly in an automobile accident while they were living in Lima, Peru, leaving Frieda to care for his ailing mother and for their two children, Ruth and Robert. Mr. Kauders by all accounts was a wonderful person, highly valued by the firm that employed him, which promised Frieda a lifetime pension and help in getting a job. Frieda decided to bring her entire family with her to New York City. Shortly after they arrived she bought a house in Queens and convinced my grandmother to join her in order to take care of the children and her mother-in-law. Again, my mother had to quit her job as a secretary, and our financial condition once more became quite tenuous.

The end of the school year drew near, and I took my first Regents Exam, in which I received a perfect score in plane geometry and a slightly lower one in biology. Meanwhile, my uncle's friend Mr. Halpern had arranged for me to take my first real job, spending the summer as a Wall Street runner for DeCoppett and Doremus, an "odd-lot" house, meaning that they specialized in stock transactions of fewer than a hundred shares each. There were two firms that handled such purchases and sales on behalf of all the other brokerage firms, each with about two to three dozen brokers on the floor of the New York Stock Exchange at all times.

This work, the best job I ever had—or ever would have, it turned out, a job that was fun and entailed no responsibility for making decisions—not only introduced me to the traders' world but also gave me glimpses of the wider world, including my first sight of a computer. It began when I filled out the application and, not knowing how to fill out "Marital Status," entered "none." The secretary who reviewed the application laughed. There were tests to see where I would best fit in: English (my lowest score—no surprise there); Arithmetic (good score but I made several errors in quickly adding up columns of numbers); and Sorting, my best score. I was offered a job as a runner, at a starting salary of a dollar an hour (minimum wage, by now), and an extra two dollars to buy supper if I had to work longer than nine hours in a day.

A two-week introduction to the firm followed our first few days of acting as runners. It was here where I saw my first computer, which actually was a calculator nearly the size of our apartment in Brooklyn, with huge fans to remove the heat it generated. All it did was to add the broker's fee of 1/4 point (1/8 point for lower-priced stocks) to the price of each share and multiplied the result by the number of shares. I was very much impressed by this machine; I may have thought then that I would work on such wonder-brains for a living, but I am not sure. I certainly had no idea that computers—never mind calculators—would increase in capacity by so many orders of magnitude over the next fifty years.

We saw the room where about twenty young men sat opposite a large board that flashed the NYSE stock prices. DeCoppett and Doremus offered a service to its clients that allowed them to call and get the current price of a given stock, as well as the Dow Jones average, which was calculated by one of the men every half hour or so. Then we saw the room where the accountants managed the round-lot certificates and the orders of odd lots, issued requests to the banks to create new odd-lot certificates, and sent the runners off to the banks and brokerage houses to pick up or deliver stock certificates and checks.

I also discovered for myself that things are not always as they seem. During our brief tour of the floors of the two stock exchanges I was shocked to discover that, placed in the telephone trading booths out of sight of the visitors in the gallery above, were a number of photos of nude women; I imagine these vanished years later, with the appearance of women on the floor. Also I learned that in the "partners' kitchen" in DeCoppett nothing was ever cooked, instead whenever a light on the kitchen's order board lit up it meant that a certain partner wanted a drink, which was then taken to him. Food would be brought from a nearby restaurant at a partner's request. I was never allowed to enter any of the partners' offices.

The runners were not all young men like myself; most were retirees on Social Security. Our job was to make four trips a day to the other brokerage houses following a set route. During the late afternoon we might make runs to the local banks to get checks from other brokers certified. We also prepared stock certificates for delivery next day, and checked the stocks delivered to us for correct signatures. I was a hawk on this point, catching a surprising number that had been incorrectly signed—if the face of the certificate said "Mrs. Jane Jones," and she had signed it "Jane Jones," it required a special stamp to guarantee the signature. In the evening we stood by the big metal boxes that held all the firms' stock certificates as they were moved into the safe in the basement. Our insurance required that nine people accompany the boxes, although none of us was armed. On rare occasions I was sent across the street with two other runners to pick up petty cash at the First National City Bank, about twenty-five thousand dollars in small bills each time, a considerable sum in 1957.

I most enjoyed going to Merrill Lynch, our largest customer, where the weekly checks to settle our accounts were the largest a runner would handle, often in the tens of millions. Another enjoyable aspect of the job was sitting on the steps of the historic Treasury Building, where Washington was inaugurated in 1789, and watching all the girls go by.

Then toward the middle of the summer I discovered another aspect of the "wider world," the behavior of working-class teenaged boys. A new rule that records had to be kept for seven years meant that the firm needed thousands more record boxes, and I was one of those sent off into a nearby loft to make them. Making boxes is a pretty boring task, but I didn't mind doing it for a few weeks. Most of the others were unskilled boys who spent much of their time telling each other about their sexual conquests, many of which took place in Coney Island. At first it was quite titillating, and even believable, but as the stories gained in competitive detail and became ever more incredible, I stopped believing them.

In the fall of 1957 I began my junior year, the year in which the direction of my life began to reveal itself. I began to be placed in the advanced classes where I studied alongside the SP students, and I was encouraged to solve math problems with the Midwood math team. This was something of a fluke: I had received a grade of 100 in intermediate algebra, and though the teacher suffered in my estimation because of it, he recommended that I apply for the math team. I took an exam, and scored a 30 out of 150 points. Surprisingly, this was good enough to allow me to attend the team meetings, although not to be a team member that year.

New York City high schools took math competitions quite seriously in the 1950s. Even though Midwood was not an exam school such as Bronx Science, Brooklyn Tech, or Stuyvesant, its team was one of the best in the city, and in those years the best teams in the New York City were also the best ones in the entire country. The team met each day during the lunch period. Our coach, Mr. Shapiro, would put problems on the board, assign a time to solve the problem, and we tried to solve them while we ate. How do you get to be one of the best teams in the country? You practice, and then practice some more.

Every few weeks we competed with teams from other high schools. Each team had five students, but when the meet was at Midwood the rest of us could participate, answering the questions on an unofficial basis, and watch the real team sweat. Each meet had six problems and a total of thirty points. The season entailed five meets, or 150 points. In addition to the meets there was a national exam given by the Mathematical Association of America. This was the type of exam that Mr. Shapiro used to determine who could join. I don't recall my score that year, but it was far better than my original 30. Practice helps!

Abram Shulsky, widely reputed as being the best junior-year student in Midwood, was on the math team. His father wrote a finance column in one of the New York papers, and once gave a talk to our school assembly. Abram was also one of the SP students in my advanced U.S. history class. Our teacher, Mr. Gordon, had written the high-school paperback text on U.S. history. The rumor among the students was that he bought stock in IBM in the 1940s, and, we assumed, became very well-off from it.

Now I can see that Mr. Gordon's class was close to college-level. Each of us had to choose a topic from a list, and give both a written and oral presentation on it. I had read some books on the Reconstruction era, and thought I understood the topic reasonably well, so I chose it. When I gave the oral presentation, there were few questions from the students, but Mr. Gordon asked which books I had read. He then pointed out that the authors were all from the South and had a biased view of Reconstruction. He told me to read additional books to get a more balanced picture. It was a valuable lesson. Years later one of my classmates at college became the nation's expert on the Reconstruction Era.

Since Midwood offered Hebrew classes in addition to French and Spanish, I decided to refresh my Hebrew by taking a semester of third-year Hebrew. After the first test the teacher handed out the papers, and when he came to my desk he said to the class that he had never before seen a perfect paper in his Hebrew classes. Embarrassed, I had to own up to my Israeli background. The midterm exams were the most important ones in Midwood, and the teacher said that whoever had the highest score would win a prize. I supposed he and others assumed that I would get this prize; instead I made a grammatical mistake on the exam, and the prize went to one of the girls in another section. He asked me afterward what had happened, and the best I could come up with was that with my unfair advantage, I did not feel I should win the prize. I imagine now that the mistake was probably a subconscious version of that, and was not deliberate.

That spring my mother heard from my grandmother that she was having difficulties in Frieda's house. The kids didn't like her cooking, and the additional job of taking care of the elderly mother-in-law was taking a toll on my grandmother's own health. She wanted to return to us, but according to my mother, my grandmother wanted it to appear that Golda had called and asked her to return, so that Frieda would get upset at Golda rather than at her. My grandmother's return permitted Golda to find a job, but the situation created a rift between the sisters. My mother argued that Frieda was rich enough to afford a live-in maid all along, and did not need to have my grandmother leave us and cause my mother to quit her job; she further claimed that my grandmother had agreed to give Frieda money on a regular basis out of the family food allowance that my parents gave her. My grandmother and Frieda always denied this, but Golda could not be convinced otherwise.

One day that spring Abe, now about 6, came to my grandmother and told her that the button missing on his shirt was in his nose. My grandmother couldn't see the button in the nose, and since Abe seemed to be fine otherwise she waited for my mother to come home and examine Abe herself. My mother couldn't see the button either, and wanted to ignore the whole thing. My grandmother persisted, however, and all four of us went to the hospital. At the hospital the admitting nurse couldn't see a button, and we had to wait some hours before Abe was seen by a resident, who couldn't see the button either. My brother was convincing enough that the resident kept on shining a light into the nose until he did manage to see something. He used forceps to try to get the button out, but to no avail. He asked a nurse to set up an operation so they could cut the nose open, and in the meantime kept on trying to get the button out. Just before he was going to give up on this approach and operate, much to everyone's relief, he did manage to pull the button from my brother's nose.

In May of 1958 I began to take the SATs. On the math part, I missed a perfect score of 800 by one question. My performance in English was relatively poor, however, only 562, showing that I needed to practice my vocabulary and grammar during the summer. I bought a deck of vocabulary cards, intending to practice them during my free time at DeCoppett and Doremus in the summer.

I enjoyed the summer job as much as before, walking the assigned routes, with no other responsibilities. My pay was increased to $1.10 an hour, and with some of the money I bought myself a gold watch. The retired men among the runners would carefully check the number of hours they worked because the Social Security Administration would deduct from their monthly checks if they earned over a certain amount in the year.

That summer, one afternoon while I was out on my route, the police came into the office and arrested a couple of the older runners for handling betting slips for a bookmaker. I knew this was going on because the slips were placed in the copy of the *Daily News* that was on a runner's desk. I thought that this was a relatively innocent operation and still have no idea who reported these men to the police.

That year, 1958, was one of three recessions during the Eisenhower administration, and one that hit our family hard. Trading on Wall Street was slow: on a good day three million shares changed hands, and the Dow Jones average was just below 600. It had seemed notable when the Dow rose above 600; it broke the 1000 mark about a dozen years later, and following significant declines in the early 1970's it took until the early 80's for it to break 1000 for the second time. The 1958 recession had a disastrous effect on my father's business, which did so poorly that he finally decided to sell out to his partner. He was unemployed for the next several months.

Sputnik had gone up on October 4, 1957, to be followed a month later by Sputnik 2, both launches made possible by the Soviet Union's progress in missile technology. Here at the height of the cold war, it was abruptly made plain to the American government that national educational standards had fallen far behind. There was an immediate rush to fund education assistance programs, particularly in the sciences and mathematics. The National Defense Education Act came into effect in 1958; of more immediate interest to me were the newly created New York State Math and Science scholarships, which not only had a higher maximum award than the usual Regents Scholarships, but lacked the Regents Scholarships' citizenship requirement. One day in the fall term of 1958 I was asked by one of the school's guidance counselors why I had not applied for a Regents Scholarship. I told him that I couldn't qualify, since I wasn't a citizen. He was embarrassed not to have known that. In retrospect, the effect of Sputnik on the US was one of the best things that ever happened to me and more generally to the science community in the US.

The fall term of 1958, the start of my senior year, was full of preparations for college entrance. It was also the one in which I was freed from the need to study French, and I chose another math subject, college-level calculus with Mr. Shapiro. As a result I had my best performance ever, averaging 97 for the five subjects that term. My SAT scores had also continued to improve, nearly perfect in math but in English better, but still not very good. I also took the Math and Science Scholarship exams, and was also asked to take the IQ test again; unfortunately the proctor left the room and one of the students discovered that the exam paper was torn in such a way that one could turn a page over and find out what the correct answers were. I assume that we all received perfect scores. I was not surprised to be asked to take the test over again, but when they did they handed us the very same exam! I don't know my score, but it was surely higher as a result of this oversight than it would have been otherwise.

I applied to four colleges—Columbia, MIT, NYU, and Brooklyn College, the last being a mandatory application to what would be called a safety school today. Several hundred students from my class were expected to go to Brooklyn College, which was just across the street from the high school, and required no admission interview.

The interviews for the other three colleges went well. During the one for NYU, I discovered that I had made the mistake of applying to NYU downtown, instead of the uptown division, which still existed in those days and specialized in engineering. The interviewer wanted to know why I was applying to NYU; I must have said something about staying in the city, meaning staying at home. The MIT interview was in an office of a lawyer on Park Avenue. I don't recall much about it, except that I was nervous. I really wanted to go to MIT, but could not do so unless I received a full scholarship, since the Math and Science scholarship, were I to receive it, could only be used in New York State. The Columbia interview was held on campus in the admissions office. The staff member asked me whether I liked living in New York City. I told him I had lived in a village where everybody knew everybody else, and I didn't like it; he asked what books I had read, and mentioned *Babbit*. It is hard to believe, but it was on the basis of such questions that I was admitted and ultimately attended Columbia College.

Like many seniors, I slacked off a bit in the final semester and my average grade dropped to 96. I began working after school hours in our firm in Wall Street. Classes for seniors ended in the early afternoon, so I was able to return to work at DeCoppett and Doremus from 2PM to 9PM five days a week. Those of us who worked in the evening had an arrangement with a local sub shop: whoever picked up the subs received a free one. I picked up the subs most nights and my dinner each weekday night that term was a tunafish sub with the works.

During the second term we took the Mathematical Association of America (MAA) exam early in the term and finally scored higher than Bronx Science, which gave us a good deal of satisfaction. Midwood's top finishers—Abram Shulsky was first, Harry Saal second—placed third in the nation, and third in New York City; my own score of 93, while only fourth in Midwood' placed me in the top fifty nationwide. However, when we went on to the first city-wide meet, a math team comprising the top five scorers in the MAA test, I succumbed to nerves and missed four of the six questions, whereupon Mr. Shapiro took the fifth-place student and me off the team to start the second meet. Since the replacements did poorly on the first two questions, and I did not, I was put back on the team. From that point on we rarely missed a question and wound up third in the city, behind Brooklyn Tech and Stuyvesant, just as we had in the MAA test. The top three finishers in each school were allowed to enter the Pi Mu Epsilon contest, which granted scholarship prizes to the top scorers. I was told to show up at NYU one Saturday. When I did so, Shulsky was at some pains to point out that I was just an alternate. I was chagrined and disappointed; the examiners let me take the test, although they did not score it.

In March 1959 we took the Achievement Exams. The seniors on the math team got together informally after the test, and discovered that several of us, including me, had made mistakes on it. I was quite surprised when I learned that I scored 800; Shulsky, who had the same score after turning in a perfect exam, must not have been happy. Another surprise was that I received the second-highest score in the school in the chemistry portion, although I had not taken the college-level class at Midwood. This was because I had concentrated in my preparation on mathematical exercises, which dominated the test.

Abe Shulsky went to Cornell and later obtained a doctorate in political science at the University of Chicago. In the late 1980's I saw a reference to him in the book *The Closing of the American Mind*, so I wrote to the author, Allan Bloom, usually associated with neo-conservative thought, and asked where Abe was. He replied that Shulsky was one of the smartest people he had ever met, and that he was working for the Rand Corporation near Washington. In 2003 one could use Google to find out that Shulsky was working for the Pentagon, and is one of the key people responsible for the US policy in Iraq. I guess one has to be careful in letting ideologists, however smart, take over policy decisions.

In April I was told that I had won a New York State Math and Science Scholarship and had been admitted to all the schools to which I applied, a source of satisfaction to my family, which was always supportive and proud of my progress. Brooklyn College was free, so scholarship was not an issue. NYU granted me a scholarship, but said that if I won another one, they'd deduct its value from theirs. MIT gave me an "honorary scholarship"; that is, they sent me a nice letter saying so, but there would be no money. I was greatly disappointed that Columbia gave me no scholarship. So the key to college choice was going to be the amount of money I received from the one I had won, which limited my choice to NYU and Columbia. The most the Math and Science Scholarship awarded, based upon family income, was $850 a year, nearly the full tuition in an Ivy League college in those years. Since my father was still unemployed when I filled out the form, I received the full amount and could enroll at Columbia, a better school than I had ever expected to attend. Since I lived at home, the additional cost to my parents was low, and I was able to supply a significant portion of it through my savings from the summer job.

Many of the seniors, like most teenagers, were celebrating the end of high school; for me the final weeks were filled with Regents Exams and a test on calculus. Five was the top score in each of two sections, and all the other seniors on the math team received two fives, whereas my scores were a 4 and a 5. Mr. Shapiro found this very disappointing; he would have been surprised that I would later become a calculus expert, and would be mentioned in an edition of Thomas's widely used calculus text.

The last few weeks in the term went by quickly, and it was time to graduate. My mother and brother came to graduation. The first time I saw Midwood's football field was when our ceremony was held there. I received one of the three mathematics prizes, and in a class of a thousand ranked forty-ninth; my friend Ira Sherman was fiftieth. I believe had I not had so much to learn in

English, and had French thus been easier for me, I might have graduated in the top five in the class.

My father bought an unfamiliar beverage he knew to be appropriate to the occasion, champagne, and we celebrated my graduation in our apartment. (The champagne was too dry, the family agreed. We abandoned it.) My parents were very proud of me; I was happy to have made them so, and looked forward to beginning college.

Two or three months earlier, my father had been able to return to the job he left to start his own firm. For the rest of his life he was disappointed not to succeed in his own business, as he had in Germany and Palestine, but his duty to his family outweighed such concerns. Years later, my mother said that she had confidence that he would succeed on his own terms, but had not counted on the effects of age—or a major recession. She insisted that I was better off in the United States, since in Israel I would have had to do military service and might have died in the wars. At this time, too, the German reparation payments also started to come in, enabling my parents to join the American middle class.

I spent the summer between high school and college, my fifth summer in the United States, working once again for DeCoppett and Doremus. One of the managers invited me to become a clerk in the accounting department at higher pay and prestige, but I surprised him by saying no, that I liked being a runner. I received the standard ten-cent increase in hourly pay, remained a runner, and continued to look forward to the start of my college work.

My clearest memory of that summer is of the time one of the young runners got his head stuck in the elevator doors. He entered the elevator, but changed his mind and tried to leave just as the doors closed. The elevator stopped; the doors jammed around his neck. This being a source of great hilarity as other runners passed by, management quickly figured out new trips for us, to get us out of the building and restore some order, but the kid with his head sticking out of the elevator continued making jokes about his situation to all who passed. I had been sent out, and walking up Wall Street I saw two men in overalls laughing as they moved closer to our building. I knew where they were going! They managed to get on top of the elevator and pried the doors open. For weeks afterward the kid would show off the marks on his neck left by this adventure.

While enjoying this last summer as a Wall Street runner, I looked up the reading list for Columbia's core courses, and started reading these on my own. These included works by Plato, Aristotle, and Aristophanes. I didn't really understand what I was reading, but I figured that this was better than ignoring the reading list. I could hardly wait for the new school year to begin.

Chapter 4. Columbia University, 1959–1963

Before my freshman year at Columbia began, I was to spend a few days living on campus during the required freshman orientation period. My father accompanied me on the subway to 116th Street, where we were met by a sophomore who slapped a blue and white beanie on my head and carried my suitcase to one of the residence halls. Mostly the time was taken up by various placement exams. I did well in the chemistry test, placing in the top group, but I did not intend to take chemistry in my freshman year. I signed up for five subjects, two of them part of Columbia's "core" courses—a writing course and one called Contemporary Civilization, largely on political philosophy. I also signed up for calculus—a continuation of what I had taken at Midwood; a course on number theory; and introductory German. My advisor, an assistant professor in political science, knew little about advising. When I showed him my course selection, he asked, "Are all these kosher?" In spite of my surprise that he did not know the answer himself, I simply said "Yes," and he signed the form. Students are still complaining about advising over forty years later.

Since coming to America my firm resolve had been to become a medical doctor just like my uncle, but because Columbia had no pre-med curriculum, and because I had to select a major that first term, I chose the subject I'd enjoyed most in high school: mathematics. I suppose I thought these first courses would gain the credits I needed for graduation, and I could take the pre-med courses later; I'm sure it hadn't occurred to me (as often happens with entering freshmen) that I might find something more interesting to do with my life, after I had learned about other fields.

The key subject in the math curriculum was Set Theory, which carried several prerequisites. I set out to convince Professor Kolchin, who made these decisions in the Mathematics Department, that I had fulfilled all the requirements. He responded that they did not feel that freshmen should take this subject. I persisted, so he looked up my calculus scores. When he saw the 4 and the 5 he had a good reason to deny me the course, and in retrospect he was quite right.

A highlight of the orientation period, for me, was the lecture on the history of Columbia University, from which one feature stands out:

Columbia, founded in 1754 as King's College and the fifth-oldest college in the United States, was a Tory institution during the American Revolution. One day, Myles Cooper, then the President of the College, noticed a large crowd gathered outside the doors to the college, listening to a young man engaged in passionate oratory. President Cooper recognized him as Alexander Hamilton, a former King's College student and apparently an unruly one. Thinking Hamilton was exhorting the crowd to attack the College, he opened a window and shouted down to the crowd, "Don't listen to that man! He has always been a troublemaker." Actually, Hamilton had been trying to calm the crowd, but when they heard this they attacked the College in earnest. Fortunately for Cooper, there was a rowboat at the Hudson River end of the building, and he was able to row to New Jersey and thus save his life.

Another feature of the orientation was a swimming test. Each entering student who could not swim three laps in the Olympic-sized pool was required to take lessons until he could. After one lap I was told to get out and register for a swimming course. Since in those days Columbia admitted only men, swimming was done in the nude. We were taught various strokes in the swimming course, of which I remember the breaststroke and the backstroke. The swimming final took place in the middle of the first term. Since my legs are normally stronger than my arms, and I had never been athletically inclined anyway, I took the easy approach, using a backstroke and leg kicks to complete my three laps. It was not pretty, but it worked.

The five academic courses I had chosen were especially challenging, partly because I was still struggling with English. Columbia's English department was quite famous, attracting many top-flight instructors, such as the Van Dorens, both father and son, who were well known on the campus (and would gain general notoriety following the quiz-show scandals, when it was discovered that the younger Van Doren had cheated). Columbia also attracted many students who were interested in journalism and could discuss current events in class with great facility. I was not as facile, partly because I was still not that familiar with English, and partly because debate was not one of my strong points. All this made the Contemporary Civilization class difficult for me.

Our instructor in Contemporary Civilization was the philosopher Sidney Morgenbesser. In later years I met some of his doctoral students who had become well-known professors at MIT, and whose favorite story about him involved a colloquium at which a noted philosopher of language stated that there are many languages in which a double negatives is a positive, but no language in which a double positive was negative. Morgenbesser, sitting in the back of the room, quickly interjected in a dismissive voice, “Yeh, yeh.”

Professor Morgenbesser’s technique in our class was to start by stating a proposition from one of the political philosophers whom we had been asked to read for that week. This would be followed by the line, “Now, Mr. __, what do you think of that statement?” I, and I suspect most of the others, dreaded being asked our opinion, because no matter what we said, Morgenbesser would deftly take the other side and inevitably beat us in the debate. He was relatively kind to me since it was clear that I still had an accent and furthermore could not state or defend my position very well; the others had their positions shredded with a fine rapier. The biggest mistake was to miss one of the class periods, for he would be sure to call on you the next time.

One day, seeing that the class had clearly not read the required material, Morgenbesser made up a quiz on the spot. He told us each to write an essay on the concept of *virtu* in Machiavelli, and then left the class. We looked at each other, and it was apparent that most of us did not know what *virtu* meant. One of the students opened the text and started reading the material. Pretty soon almost everyone was reading that section of the text. Machiavelli felt, the readings said, that the Italian renaissance needed to restore some of the Roman *virtu*es. The next week Morgenbesser came in with the graded essays. He said that he inverted the grades: those who did well, and thus clearly cheated, received an F, and the others received higher grades.

The class in English composition met once a week, when we were expected to present an essay. The first was on any topic we chose; I did fine on this one. The second essay asked us to compare and contrast two short stories. I could make neither heads nor tails of these stories and their relationship to each other, and my essay showed it. The instructor gave me an F for this composition, the first time I had failed an assignment or test. This was a terrible shock, even though other students tried to encourage me by saying that it was "policy" to flunk a student on some such composition. During the term we were asked to read a short story in *Playboy*, which, the instructor said, had the best short-story writing at that time, adding that we should not be ashamed of sex. Later in the term a young female graduate student substituted for the instructor and wanted to continue on the sex theme. At one point she said, "A wore is a wore." We couldn't figure out what she was saying until it dawned on us that she meant to say, "A whore is a whore," but did not know how to pronounce the word. Most of us snickered nervously.

I signed up for introductory German because I had never formally learned the language. I understood that with my informal experience of German I had an edge on the other students, but since I was among very good students, especially in English and related core subjects, I felt I could use whatever edge I could get. Our instructor, Professor Speer, had been a translator in the Nuremberg trials; the language lab instructor was Professor Bauke, a young assistant professor who would later become my instructor in two other German courses. He asked me one day as I was working in the language lab, why I was taking the course, since my German accent was so good, and when I explained my situation he commented, "Okay."

It was the mathematics courses that saved my grade-point average and kept my mind engaged and enthusiastic. The calculus course, a continuation of the work I did in Midwood, was taught by Professor Elliot Mendelson in a large lecture room. I assume that there were also small recitation sections, but I do not recall them.

Mendelson also taught the course on number theory. One of the students in this class, Russell Abbott, another math major, was also in my Contemporary Civilization section. We remained friends all through our college and graduate-school years. Something that first drew us together was that each of us, realizing that although the number-theory course was listed as a graduate subject, there were no prerequisites for it, had signed up for it. One day Morgenbesser walked into Mendelson's class to ask a question and saw us there. He looked surprised, since he knew we were freshmen and this was nominally a graduate course, but Russ and I were right: With no prerequisites, it could indeed be taken by freshmen.

I had finals in most of my subjects, and the instructors gave us some advice on what to expect. Morgenbesser said that he tried all term not to take a position on any issue, but that he thought he might have taken one on the issue of free will versus determinism. He wanted to let us know that he had changed his mind on the issue, but did not want to tell us whether he changed his mind an even or an odd number of times. Thus we had to rely on our own views on the final.

Mendelson had used the classic number-theory text by Vinogradov during the first half of the term, and switched to a more advanced text by Pollard thereafter. He said that the final would be based on the 150 proofs in Vinogradov, and none in Pollard; I spent several days memorizing every step in the Vinogradov proofs. The finals were in Columbia's gym. Each student was handed a glassine envelope upon walking into the gym. We sat in long rows, and waited for a gong to sound the beginning of the exam with his or her particular final exam in it. When the examination officially started we were supposed to rip open the envelope, take out the exam paper, and begin working. As we entered the gym I studied the number-theory envelope carefully, and was able to see through the envelope one of the problems. It was Theorem 126, and I mouthed the steps to the proof to my friend Russ prior to the sound of the gong. It didn't help him, however, because he forgot the steps I told him.

We were told that the Registrar would take months to generate a grade sheet, but if we wanted to know our performance on a more reasonable timetable we should give each instructor a self-addressed card asking for the grade on the final and the final course grade. As my cards came in I saw that I had obtained a C+ in both Morgenbesser's course and in English composition, and grades in the A range in the three other subjects. I just barely made the Dean's List, which required a B+ or better average.

During the term my family and I had moved from the three-room apartment above the candy store to a five-room apartment on Oakland Place. This gave me my own bedroom, which helped me study for the final exams. Oakland Place was a relatively quiet side street where it was possible to play in the street itself. It had a racially mixed population, and thus my brother, now about seven, played with many black children. My mother pointed out that Abe tended to "buy" friends by offering them candy and ice cream. She thought this unusual, and noted that I had never done it.

When the second term began I replaced the number theory course with an introductory physics subject. I had done well in physics at Midwood, but did not expect to do nearly as well in it in college, and I was right. The other courses were continued, except that I could not keep Morgenbesser's section due to a schedule conflict. This had its plusses and minuses. I was not nearly as scared as I had been walking into Morgenbesser's class, but the instructor was not nearly as interesting. He specialized in the psychoanalyst Jung, and had little experience in teaching political philosophy.

The instructor in the physics course went through the usual lecture demonstrations. The one I remember best had to do with a very long pendulum that was attached to the ceiling of the lecture room in the Pupin building. The teacher said that he trusted physical laws, and to prove it he would place himself at risk. He pulled the end of the pendulum to his chin and released it. If the pendulum had swung just a bit too much, then the chin would have been injured, at the very least. Of course, the pendulum stopped just short of the chin, and all was well. I recall physics tutorials where we tried to solve homework problems in the text. The hot topic those days following Sputnik was space travel, and some of the problems had to do with satellites. I could solve the problems but found them hard. We also had laboratory experiments in physics, one of which required us to obtain a value for the gravitational constant using marks on a spark paper. I was not very good at such experiments, either.

Early in the term I stopped by an office where they tested students' ability to speak English. As expected, I was then told to take a speech course. In contrast to those I had taken in junior high and high school, the approach was relatively theoretical, examining such topics as why the Japanese could not differentiate between the sounds for "L" and "R." Each of us had to give a talk in front of the class. A member of Columbia's football team—who would be, years later, the Columbia football coach and then a well-known entrepreneur in Silicon Valley—talked about the components of a three-point stance in football. I talked about the upcoming world cup in soccer, noting how weak the U.S. teams had been, and the likely strength of the West German, Brazilian, and Soviet teams. My accent improved over the years, although I can't say that the various speech courses I took had much to do with this. Some people say that my accent is due to the eight years I spent in Brooklyn. I think that my German and to a lesser degree Hebrew had something to do with it too, as did the Australian-born English teacher in Israel. I have asked people to guess the source of the accent, and the guesses have been all over the globe, including many states in the union.

In March 1960 I took the test to become a citizen. My father and mother had been naturalized earlier, and my brother became a citizen through them. This happened in 1959 after my family had been in the US for the required five years. I was nearly 18 then, and it was decided that I should become a citizen in my own right. My mother reported that when she was asked in the citizenship test who the governor of New York was, she replied that he was rich, that he came from a family made wealthy by oil, that his son had just married a pretty woman—she simply could not think of the name Rockefeller.

My examiner was impressed with the fact that I went to his alma mater, Columbia, so he decided to make this test a bit more difficult than usual, I think. The preliminary questions included one on whether I was ever married, and I said “No.” The next question was on whether I had any children. I protested that I had already said that I was not married. The examiner replied that in his experience one question had little relationship to the other. He then asked who would take over for the governor of New York, were he to die, and I said the Lieutenant Governor. He asked for his name, and luckily I remembered that Wilson was the Lieutenant Governor at the time.

He next asked for me to recite the preamble to the U. S. Constitution. I stumbled and he helped me so that we finished the paragraph. He next asked what the meaning of the phrase “54' 40" or fight” was. I said that it was a border dispute between the United States and Britain; he disagreed. We went back and forth on this point. Finally I asked him what he thought the answer was. He said that it was a dispute between the United States and Canada. I responded that Canada was not independent of Britain at that time. He was displeased by this retort, and terminated the test by asking me to write the sentence “I go to school.” I protested that this was too simple, and he said testily, “Just write it.”

Months later I went to the Kings County Court House in downtown Brooklyn to be sworn in as a citizen. There were many people there, most of whom appeared to be Italian, as the judge himself was. He talked about his parents coming on a boat from Italy and how they struggled, and how proud he was of them. It reminded me of my situation. In any case we all raised our hands at the appropriate moment, and thus I became a U.S. citizen. In fact I became a dual citizen, since I did not relinquish my Israeli citizenship. Israel makes it rather difficult to renounce its citizenship, as does France. (It is said that Napoleon could not understand why anyone would want to renounce French citizenship.) The reason for me to consider renouncing Israeli citizenship was that I owed two years’ military service; if I were to join the Israeli military now I would lose the

U.S. citizenship I had just obtained. I solved this dilemma after a fashion by not visiting Israel for thirty-three years, and then only after obtaining a temporary dispensation from military service.

Toward the end of the term we had to write a major paper for the Freshman English composition class. I chose to write on why Byron decided to go to Greece and join the revolution against the Turks. My thesis was that he was bored with life in Italy and wanted a new challenge. The teacher liked the thesis and I received a grade in the B range this term. My grade in Contemporary Civilization also rose to a B-, similar to the physics grade, and the others remained in the A range, so I was on the Dean’s List once again for the Spring term, and once again just barely.

The year 1960 saw the Kennedy-Nixon presidential race. During the campaign I saw Kennedy in a motorcade in New York City, and was deeply impressed. He seemed to have an aura that I saw again later, on the couple of occasions when I met President Clinton. I shared the values of the Democratic Party and intended to vote for Kennedy in my first election. The discussions in the presidential race carried over to my summer job in Wall Street. There were other college students working for my firm that summer, two of whom came from Fordham and were committed Republicans. A slogan often heard that summer was “Better red than dead.” Their view was that dead was better, and I felt otherwise. The political difference made for a chilly atmosphere that summer.

While I worked on Wall Street, I also took a summer course on differential equations at Brooklyn College in the evenings. I was surprised to receive a 90 on the first test, with ten points deducted for penmanship. I agreed that the handwriting left something to be desired, but complained that this was no reason for taking off points, especially in a mathematics course. The instructor gave me back the points, and I eventually received an A in the course. Interestingly, my son Jesse took a similar course at Columbia, using essentially the same text I had used 40 years earlier, written by Boyce and DiPrima.

Sophomore Year, 1960–1961

In the beginning of the sophomore year I still expected to go to medical school, and continued to major in mathematics while continuing with German and physics. I switched from Contemporary Civilization to the second major core course at Columbia, then called simply Humanities A. I was able to take the set-theory course denied to me the year before, and added Advanced Calculus, which turned out to be a relatively advanced undergraduate course.

I well remember the first lecture in Humanities. It was in the Philosophy building. We had no idea who the section instructor would be. In walked an elderly man with a goatee. He did not introduce himself. Instead he recited the first page of the *Iliad,* from memory, in Greek. This is how we met Moses Hadas. Hadas was chair of the Greek and Latin Department. He had such a resonant voice that he held the mace and led Columbia's graduations and would annually introduce the President of the University. Needless to say the class was stunned by this performance.

Hadas's style was similar in many ways to Morgenbesser's. It was said that both were ordained as rabbis as well as obtaining a Ph.D. Hadas was easier on the students since he left the final retort hanging and thus did not embarrass them. He was kind to me. I suspect that he realized that I still had difficulties with the English language. I also grew during the term, and I do not mean physically. I recall that during one of the hour-long Humanities exams I realized that Plato did not believe in progress. I was excited by this surprising discovery, and made a note of it in the exam book. The course also had short weekly quizzes in order to make sure that we did the readings. One week the readings were in the Old Testament books of Genesis and Job and the New Testament book of Matthew. Whoever made up the quiz assumed that many of us knew this material pretty well, and the quiz was unusually difficult. Hadas pointed this out when he handed out the grades. He noted that even he had difficulty with some of the questions. I missed one question in 25 on that quiz. The class average was 8 points out of 25.

Morgenbesser and Hadas had a great effect on me. Not only did I become more well-rounded—as would be expected from a liberal arts education—but their courses also had an impact on me professionally. Plato was discussed in both subjects, and I now realize that I am somewhat of an idealist like Plato, as are many mathematicians. I also now realize that Germans tend to like Plato. Plato's student, Aristotle, on the other hand, was a scientist and the creator of a form of logic. I am not a fan of Aristotle, although I respect him greatly.

Professor Assmus's course, Advanced Calculus, was misnamed. The text he used was indeed called by that title, but it was actually based on a senior-level seminar offered at Princeton by Nickerson, Spencer, and Steenrod, leading mathematicians in the branch of mathematics called algebraic topology. Spencer and Steenrod appear in the recent biography of the mathematician John Nash, who won a Nobel Prize in economics for his work in mathematical economics, entitled *A Beautiful Mind*. There were about a dozen students in the class, of whom a third were sophomores, including the student who would become the class valedictorian, Richard Rassala, from Brooklyn Tech, whom I had met in the swimming course. He eventually graduated with an average that was higher than A, which was possible since A+ grades existed at Columbia, and went on to obtain a Ph.D. in mathematics at Harvard and become a professor at Northeastern. Like many mathematicians he later began to teach computer science, and still later attended a course that my former doctoral student Richard Zippel taught at MIT.

What I recall best about Assmus's classes was that I was lost in them. It appeared to us that only Rassala really understood the material. My best experiences in these classes came during the final exams. During the fall term Assmus essentially told us that the final would have two questions, and he hinted at what the first one would be. Moreover, he said that if we were lost during the final he would be there to answer our questions. For each question that he would answer the highest score we could get would be reduced one notch, that is, from A+ to A, from A to A-, and so forth. The final was indeed composed of two questions, and the first one was easy, partly because of the earlier hint. The second one had several parts, and I squirmed in my seat for a long time, since I was not making any progress on it. No one had ventured to ask Assmus a question, but finally, with only twenty minutes left and little to lose, I went to the front of the class and told Assmus my predicament. He asked me to explain the problem to him. I did so, and he said simply that I was confused, that I should sit down and think about it some more. That did not appear to be very helpful and I returned to my seat, but as I sat down I realized that I had indeed been confused, and I went on to answer much of the problem. As it turned out, only Rassala was able to answer the question fully, and he and I had the top scores on the final and the only A's in the course.

The set-theory course, taught by Elliot Mendelson, was heavy on proofs. At one point Mendelson proved in class—taking most of the class hour to do it—that one plus one does indeed equal two. The course was as I had expected, about mathematical fundamentals; I am less enamored of such logic- and set theory-based fundamental approaches these days. With the two mathematics courses, German, and an A- from Hadas I was easily able to get on the Dean's List that term.

In the spring term I took a course in music, to fulfill another of Columbia's core requirements. It was taught by an instructor who continually apologized for his poor piano-playing as he tried to describe various musical concepts to us. Since my parents had no particular interest in classical music, it had not been part of our lives at home. In Israel we had a class in the fifth grade in which the boys learned to play the flute and the girls learned the mandolin, enough to prove to me that musical performance was not my game. I enjoy chanting in the synagogue, but this would not help in this course. However, I learned a great deal about the history and a bit of the structure of musical compositions. In the beginning of the term we spent a good deal of time on the sonata form. I recall a student asking why Mozart was able to write so many symphonies, when he was clearly so constrained by the sonata form. The instructor's answer was revealing: "He was able to write so many symphonies because he was so constrained by the sonata form." I have used this answer many times since, in making an argument in favor of abstractions, such as the sonata form.

Later on in the term we learned about a period earlier in the twentieth century in which composers created pieces that sounded like compositions of earlier centuries but were not. Sure enough, the final contained a major section where we had to estimate the period of the recorded piece we listened to. The trick pieces were featured in this part of the test.

Assmus's Advanced Calculus class was even more difficult, even mysterious, this term. I had no idea what the point was for exterior algebras, a key topic during this semester. In order to prepare for the final I decided to read an older book that was also titled *Advanced Calculus,* by R.C. Buck. In reading this book I realized that earlier efforts to explain calculus in multiple dimensions were somewhat vague, and that the definitions and proofs in our course clarified these notions. I was thus able to restate the basic theorems in Buck and re-prove them easily, using the concepts in our text. I walked into the examination room and wanted to tell the others about my discovery, but everyone shut me up and went on discussing the esoterica related to the course. I was pleasantly surprised to see that over half the final was on restating theorems in Buck and proving them using the notation in the course. Rassala and I were the only ones to get above 70 on the final. I believe the average score was below 20. Rassala was clearly very smart. I was simply lucky in this course.

After the set-theory course, in the spring term I took a course in logic with Mendelson. This turned out to be a course on recursive functions, then considered to be useful in theoretical computer science. Mendelson enjoyed teaching from his notes while we waited for a new text by Raymond Smullyan, which appeared toward the middle of the term. I am not sure we understood much of the material. This was to be Mendelson's last term at Columbia: denied tenure, he left for Queens College, where he produced a famous text on logic a few years later from his Columbia course notes.

The most interesting part of the logic course was the final: one question with seven parts. We were supposed to show that the Sweet functions defined in the exam were equivalent to the recursive functions we had been studying. Unfortunately, if you got stuck in the beginning you had a hard time proceeding. I could see that most of us were lost; a few left the examination early, but one student, Tom Guttman, appeared to persevere. He was probably the best student in the class and also the best computer programmer at Columbia. I turned in the postal card with the examination, and waited for Mendelson's response. It came back soon enough. My grade on the final was "Terrible." My final course grade was B+. I realized later that "Terrible" might not have been the worst description of performance on the final. I think it was probably "Awful." What Mendelson had done, mercifully, was to use the grade on the midterm for each of us as the final grade in the course. I believe this was the only time Abbott had a higher grade than I did.

While I continued with the Humanities A course, I could not continue in Hadas's section because of a scheduling conflict. The graduate student who taught my section was not nearly as interesting or exciting as Hadas. I also heard that Hadas was unhappy with the performance in his own section that term. My grade in Humanities dropped to a B+, but I was able to remain on the Dean's List.

My daily subway trips to Columbia lasted about an hour and a quarter each way. I got on in the beginning of the line in Flatbush and thus would generally get a seat going to Columbia. At first I tried to study during the ride, but the noise and the movement of the train made it difficult to concentrate. I thus would stare at the other passengers—a no-no in New York, but I got away with it for years. That winter there was a tremendous snowstorm. Reasoning that Columbia never closed, since most students lived on campus, I made a great effort to go there. But I slipped on the subway steps and came down hard on my buttocks. In great pain, I completed the ride to 116th Street in Manhattan. I was shocked and angry with myself when I realized that Columbia had indeed closed that day, and I still faced the painful return trip. It took several visits to a doctor to repair the damage.

Despite the long daily commute, I did participate in a panty raid that term. Well, I sort of participated. Around five o'clock one early Spring afternoon I was walking along Broadway toward the subway when I saw that one was in progress: The girls in Barnard College, on the other side of Broadway, were waving panties out the windows of their dorms, daring the young men from Columbia to cross the street and pick up the undergarments, a common springtime expression of youthful high spirits in those days. The men were quite willing, but members of New York City's Finest had been called in and they patrolled Broadway on their horses. While this prevented the actual exchange of panties, everyone had enjoyed the shouting and flirting.

During the summer of 1961, instead of working on Wall Street I took two consecutive summer chemistry courses at Columbia. I am not sure of all the reasons for this. Clearly if I were serious about going into medicine I would need to take organic chemistry, and thus introductory chemistry. I also read that MIT required chemistry of all its students, and I thought that I would want to go to graduate school there, if I did not go to medical school. I enjoyed these courses, partly, I suspect, because they were easier than regular Columbia courses. Each one took nearly six hours a day, five days a week, leaving no time for a job.

We had labs each afternoon. I took out one of the girls in my lab, and we went on a date to Yankee Stadium. This was Roger Maris's great year, when he hit 61 home runs, and Mickey Mantle hit 54. Sure enough, Maris hit one in our game, his 57th. After the game we went to a restaurant, and I learned that this young lady had considered becoming a debutante a few years earlier. This wasn't my style, and I guess I wasn't her type either. We didn't date again.

My grades in the two summer courses were A and A+, which raised my average at Columbia. In those days the Selective Service used college grades in part to determine who continued to get student deferrals; this was a strong incentive to keep my grades up.

Junior Year: 1961–1962

My junior year at Columbia was actually my final undergraduate year. I had accumulated enough credits from Midwood, summer courses, and small overloads each term that I could graduate that year, if I chose to. Russ Abbott, Ralph Kopperman, who was another math major, and I all chose to graduate in three years. I didn't realize then that one long-term effect of graduating early is that one becomes an alumnus of a class with which one has relatively few connections, but even if this had been pointed out to me at the time, it would not have changed my plans.

For this final year, I signed up for two courses to satisfy the Columbia requirements—political science and a survey course on art—and continued in German with a German conversation course with Bauke. I also continued in math, taking an abstract algebra course and a graduate course in mathematical analysis. My key subject, however, turned out to be an introductory programming course.

Bauke's course was simply fun for me, since German conversation was my strength. He understood that, and the two of us would lead the others along. I recall that at one point he asked if people recalled the address of every place that they had ever lived in. I said that I did and he said that I was very self-centered if that were so. This must have startled me, since I remember his comment so well.

For the art course each of us had to buy a box of photographs, and each class seemed to feature a darkened room and one slide after another. The Parthenon was emphasized during the early part of the course, and then we were marched through the highlights of two millennia of Western art. The most enjoyable part of the course for me was going to museums and writing about what I saw. I remember best the Fragonards in the Frick Museum. The Rococo style seemed like fun; unfortunately it ended with the French Revolution.

I think I obtained a B+ in the algebra course, quite a bit below my usual standard in mathematics subjects. This is surprising since my thinking is clearly very much in tune with abstract algebra, and the approach would play an important role in my professional career. The course in mathematical analysis was a high-powered math subject. We used a European text by Riesz and Nagy.

Like Assmus's course the previous year, it could be viewed as a recapitulation of calculus using more precise definitions and proofs.

But as I said, the computer course was the key to the term. The teacher, Mr. Leeson, had written a text on the IBM 1620 computer that we would use in the course. The class was held at the IBM Watson Laboratories, very close to the campus. This was IBM's original research lab, before the creation of the Thomas J. Watson Laboratory in Yorktown Heights, New York; the famous computer language, Fortran, had been developed there a few years earlier. The lab contained an IBM 650 in addition to the 1620. These computers were toys in comparison to the laptop on which I am working right now, and their cost was hundreds of times as great.

Our first programming projects were in Fortran. Programming seemed more like fun than study. We would punch out the programs on cards and read them into the 1620 computer. This was a mini-computer-like machine and each of us got to run our programs on it, since there was no operator on that machine. In the latter part of the course we learned about the assembly language for the 1620, and thus I was able to begin to understand how the Fortran compiler for it worked. The 1620, one of the last decimal machines, had a memory of 20,000 decimal digits. Instructions were usually 12 digits long, so that one could not write a very long program and keep it in memory.

The IBM 650 was an older computer. IBM, I believe, made its fortune in the 1950s by underestimating the sales of their computers. For example, their marketing people felt that they could sell fifty model 650 computers, and they priced them accordingly. We had serial number 1501, and in those days I do not think people cheated in the serial numbering scheme. The 650 relied on a drum memory, with a drum that stored 2000 instructions. Each instruction contained the address of the next instruction to be executed. The trick was to estimate where the head of the drum would be when the next instruction was ready to be executed, and then store the instruction at that spot or just beyond it. The assembler for the 650, the Symbolic Optimizing Assembly Program (SOAP), tried to do just that. The 650 also had a Fortran compiler, but it required several passes on the 650 before the deck of cards could be fully compiled. The chance that everything would work correctly in such a case was not very high. Our 650 had an additional core memory of 64 words. I believe the price for such a memory was half a million dollars!

Toward the end of the term, Mr. Leeson announced that one of the students in the class would be chosen to work for IBM in the lab the following term. I was thrilled to be the one chosen, although it meant that I would have to come in on Saturdays, when the lab was used to teach some of the best high-school math and science students in the city. The instructor was a teacher in the Bronx High School of Science. I supported this activity by helping the students use the computers. One of the students was Gerry Sussman, who was fourteen at the time (I was twenty). He recalls that I gave a lecture on Fortran, and claims it was the worst lecture he'd ever heard. (He later told me that the reason he smoked a pipe in the 1970s was that he saw me do it.)

One day the father of one of the students called and asked my advice. His son, a junior in high school, had been admitted to Princeton at the age of 15. I advised against sending him that year on the grounds that the son was not mature enough. The father protested, saying that he himself had gone to college at that age. Nevertheless, I was glad to learn that they took my advice when I read that the son was admitted to Princeton the following year and given their highest scholarship.

I decided to apply to graduate school, rather than medical school. Mathematics and now computer programming were fun and easy. I realized that I had been so involved in this area that I had never really prepared to go to medical school by taking the appropriate courses, although Columbia was a hotbed of students trying to get into medical school, especially Columbia's Physicians and Surgeons. In order to get into graduate school I had to take the Graduate Record Examinations. GREs were like the Achievement tests in high school, although they contained more advanced material. In those days the highest score was 950, which meant that I was able to obtain a score over 800 in the quantitative part, although my English score was still below 750. Kopperman received a score near 950 in math and claimed that the examination questions had not changed from the ones he took the previous spring. I applied to math departments at Columbia, Michigan, and MIT, asking Professors Mendelson and Bauke and Mr. Leeson for references. Now it was time to wait for the responses.

During the spring term, my final term as an undergraduate, I took an introductory economics course and continued the algebra, analysis, and German courses, as well as a second-level programming course with Dr. Ken King, the director of the Columbia Computer Center. King had obtained a Ph.D in physics at Columbia and knew about numerical methods for solving scientific problems. The part of his course I found most interesting was the section on a new language, called LISP (for LISt Processing). LISP was intended for research on Artificial Intelligence, whose goal was to create programs that would perform tasks that humans would consider as requiring intelligence. We did not have a manual for LISP, only the original paper by MIT's John McCarthy, nor did we have a LISP system to try out programs. I would be able to address these weaknesses during a visit to MIT the following summer.

One day I was walking along the campus and I saw Harry Saal, my former teammate in the Midwood Math Team. Harry was a year behind me in Midwood. In his senior year he obtained the highest score on the MAA test in the country. Harry was admitted to Columbia the previous year. He wanted to major in math at Columbia but had just taken the set theory course, taught that year by Professor Kadison. Kadison made up a tricky true-false final, and Harry obtained a poor grade, possibly a D. When I met Harry he was thinking of switching to Physics, a popular major in those years just after Sputnik. Harry eventually obtained a PhD from Columbia in experimental physics, and worked for many years for IBM, including some years at their center near the Technion in Israel. In the 90's I saw Harry on an airplane and found out that he had formed a company in Silicon Valley to help people find flaws in their internal computer networks. He had just sold the company and became a multi-millionaire. Thereafter he spent much time being a philanthropist. Harry is possibly the most financially successful graduate of Midwood High School.

Later that term Professor Bauke asked me and another student to take a German translation exam that would determine who would win a prize as the best student in the introductory German classes. I won the prize, and was invited to a party. Professor Steer brought a keg of beer and handed out the prizes. Mine was a set of books donated by the German Consulate of New York.

In April the responses from my graduate applications came in. I found out that MIT admitted me and offered me a Research Assistantship; Columbia admitted me and would offer me either a teaching assistantship or possibly a fellowship, the former depending upon the outcome of an oral exam to determine my ability to teach undergraduates. If I didn't get the TA-ship, I would be notified about the fellowship. Somehow Michigan hadn't received all the components of my application, and thus did not admit me, but the news of the two acceptances kept me from worrying about it.

The oral exam was with Professor Kadison. When I arrived at his office he told me to go out and think about how I would present the concept of integration in the calculus to a freshman student. I had just had the concept of integration redefined in the course on mathematical analysis I was taking. I went back in and presented this advanced approach to integration, whereupon Kadison said that I would not get the assistantship, but I might still get a fellowship; indeed, soon afterward I was awarded an IBM fellowship. I thought, as many students do, that a fellowship that required no work was far better than an assistantship that did, so I accepted the Columbia offer, and turned MIT down for the second time.

In May I was surprised to learn that I was chosen to become a member of Phi Beta Kappa. Professor Ernest Nagle, a well-known philosopher, presided over the official ceremony. As each of us went forward he showed us the secret handshake, and we fitted our hands into his. Afterward there was a small party for the new members with some of the faculty. Moses Hadas was there, beaming at me. I was embarrassed by the presence of this great man, who had apparently voted for me. There are very few people by whom I have been as impressed as I was by Hadas. I realized that faculty members had to vote for the students when I was informed by the political science instructor whose course I took that fall that he could not vote for me because of my less than stellar performance in his course.

Graduation at Columbia is normally a majestic affair. It rained that year, and so the ceremony was moved to the Cathedral of St. John the Divine, a vast cathedral that could easily hold thousands of graduates. I do not think it could also hold all the families of these graduates. They probably watched the affair via closed-circuit TV. Our speaker that year was Dean Rusk, the Secretary of State, who was warmly welcomed. Recall that this was 1962, and Vietnam was not yet a major issue. I was pleased to learn that I was graduating Magna Cum Laude. I also claim to have been very close to a Pulitzer Prize: Columbia grants the Pulitzers, which are listed on the inside cover of the graduation book. My German prize was on the facing page, three inches from the nearest Pulitzer.

In my experience Magna cum Laudes outperform many Summa cum Laudes in the “real” world. The reason is that many students who obtain a Summa are so intelligent that they can obtain high grades with little effort, and thus may not be prepared for the hard work ahead. For most of us, success in life involves intelligence plus hard work. A bit of luck does not hurt either.

During the summer, instead of the usual vacation trip with my parents, I took a vacation on my own, traveling on the bus, which was more common than plane travel in those days. First I headed to Boston, to visit MIT. Their computer center did research on time-sharing, a technique whereby many people could use an expensive computer at nearly the same time. I also visited the Artificial Intelligence Group and inquired about LISP. A staff member gave me an old tape with the LISP system on it. I still had no manual, but I was now well ahead of the situation at Columbia earlier that year. Next there was a twelve-hour bus ride to Washington, D.C., where I took the guided tours of the White House and the Capitol, and finally a bus back to New York.

IBM Fellow, 1962–1963

Ken King told me that as an IBM Fellow I would get a desk in an office at Columbia's Computer Center. The Center had an IBM 7094 computer that cost $3 million at the time. I cannot help noting here that my laptop runs nearly a thousand times as fast as the 7094, has a thousand times as much main memory, and costs about $2000. I believe no technology in human history has changed as much over such a short period of time.

I was pleased to work in the Computer Center because I wanted to do some projects on the 7094. The project I eventually completed was the COSTAR (COlumbia STAtistical Routines) program, which allowed a user to access a statistical routine by just giving its name, without having to write a Fortran program to do so. Making COSTAR work required understanding in detail how Fortran's runtime system would access a subroutine from its name. In doing this bit of research I determined that if one called a subroutine by the name Y00000, this would confuse the Fortran system, and it would call an entirely different program. I called this to the attention of IBM, but I guess they made it into another feature of the system, since they did not fix the bug. I became a minor star at the Center when I introduced COSTAR to the user community. Earlier that year I also found a flaw in the Fortran compiler for the 1620. This involved expressions using exponentiation with negative exponents. I tried to fix the bug myself, but there was far too little space available in which to add the requisite code. One reason for the Y2K problem that occupied so many during the year 1999 is that old software was written in an era in which computer memory was so expensive that saving two digits in the designation of a year was worthwhile. Of course, my colleagues and I had no idea that any of the software that we would write in the 1960s would still be used thirty or more years later.

Given the work I was doing on COSTAR, I had taken a summer course on statistics in Columbia's Teachers College. This was a relatively elementary subject, but it did give me an introduction to the approaches used in the field, which I felt would be helpful in the COSTAR program. I learned that the level of work in Teachers College was lower than that of Columbia College, at least in mathematics, although this should not have been surprising.

The IBM Fellowship paid my tuition and gave me a stipend of $1800, with which I was for the first time able to live on campus during the academic year. I obtained a small room in John Jay Hall. With my interest in computing I decided to switch to applied mathematics from the relatively pure mathematics of my undergraduate years. I took courses in the Math and Statistics Departments on the numerical solution of partial differential equations, complex variables, and probability. I also took a course on numerical computation and another on symbolic methods, in the School of Engineering.

The fellow in the room next to mine in John Jay was a Lebanese graduate student in political science. The first Saturday night during the term we got into an argument over the "real" meaning of the Balfour Declaration, in which the British government promised to create a homeland for the Jewish people in Palestine. Several students gathered around us on our floor, and I heard one say that it was going to be an exciting year. Actually, we never argued again; our only other contact during the year was when my mother called him frantically one weekend, because I had not called her at our usual time, and was not in my room.

I wanted to work part-time in the Computer Center, and Ken King agreed. I needed to get the permission of the Mathematics Department, however, because they had provided my fellowship. The chair of the department, Samuel Eilenberg (who was called S^2P^2, for Smart Sammy, the Polish Prodigy), had a tremendous impact on modern mathematics, largely though his work in the 1940s in founding the field of algebraic topology. As department head he strongly defended departmental turf. When I asked him for permission, he said "No." I was supposed to study math with my fellowship, he said, and there were to be no distractions from a job in the Computer Center. This was not a good way to start the year. I found out later that during the year Eilenberg visited the Computer Center, and when he saw my name on one of its doors, he blew up at King. Ken explained that he gave the IBM fellow an office each year as a courtesy, and that I did not have a job at the Center as well.

The course on symbolic methods was taught by Professor Stephen Unger, who had just spent a sabbatical year at Bell Labs. While there he had learned COMIT, a string-processing language developed at MIT. He wanted to use it to analyze electrical circuits. I was interested in such languages, so I offered to do a term project to help him. What was interesting about this project was that I used half a dozen different tape drives in each run on the 7094. I recall that Unger once came to see the computer in operation during such a run. What was frustrating about COMIT was that we did not have a listing of the system so that we could consider modifications in the language. The developer, Victor Yngve of MIT, did not wish to provide such information. The folks at Bell Labs were also frustrated, and they eventually developed the SNOBOL language to deal with this issue. SNOBOL became quite successful in the following decade, while usage of COMIT languished.

Professor Walter Strodt taught the course on partial differential equations. Much of the course material was based on notes from the Courant Institute at NYU. This course and Unger's taught me a few facts of academic life, most importantly that Columbia was not at the center of research either in computer science or in numerical computation. Having a fellowship was fine and dandy, but being at an established center of research in my field was more important. I started reading the journals in computer science and I became increasingly interested in Artificial Intelligence. Marvin Minsky of MIT had written a survey paper of the field in 1961. I learned that one of his students, James Slagle, had written a program in LISP to solve integration problems in the calculus. This was very exciting to me, because it combined my interests in mathematics with my growing interests in computer science. I told Unger that I wanted to rewrite the integration program in an assembly language in order to increase its speed. Implicit in the desire to rewrite the program was my idea that I would leave Columbia and work for Minsky at MIT. Of course, I would need to be readmitted first, so I applied once again to MIT. To be on the safe side, I also applied to

Harvard. Whether I would remain at Columbia depended on my grades and a qualifying exam for the doctoral program, which I would take in the spring.

On the last day of 1962 I was working as usual at the Computer Center when it dawned on me that people were leaving early. Soon a female staff member and I were the only ones left. I could tell that she was a bit lonely, so I stayed and talked to her. To my discomfort I realized that she was hinting at spending some time in my room, but I let this suggestion pass. A few years later, after a talk I gave at a conference in Atlantic City, as she and I sat next to each other on a bus back to the city I asked her out, but she refused. She was not Jewish, and this probably held me off the first time; by the second time, I had become more liberal but she had become choosier.

I was, however, quite smitten by another female staff person at the Center. She realized this, and one day took me aside to tell me that she had just gotten engaged to an Orthodox Jewish man. I was not happy at hearing this news.

I found the computing projects I undertook increasingly more interesting than my mathematics coursework. The Complex Variables course was taught by Professor Lorch from his own notes. Both his lectures and the notes were quite clear. The introductory material in complex variables is one of the few mathematical subjects that is fully developed—that is, the fundamental theorems on integration apply to all cases of interest. In contrast, number theory has many easily described open problems, such as Fermat's Last Theorem, which took three centuries before it was solved in recent years, to considerable fanfare. The course on Stochastic Processes was taught by a Hungarian professor named Takacs, who had worked at Bell Laboratories. He would describe examples of stochastic processes involving light bulbs or vacuum tubes. Unfortunately, his accent made the opening word of most proofs, "Suppose," sound like a brand of women's hosiery.

In April 1963 I was notified that MIT had admitted me again, and once more offered a Research Assistantship. Harvard made the same offer. I accepted

MIT's offer, and looked forward to working with Minsky in AI as a graduate student in mathematics.

The written doctoral entrance exam at Columbia served a dual purpose. It decided whether you would be admitted to the doctoral program, and also whether your performance was sufficient for a Masters degree. A few days after my classmates and I took the exam, I met Eilenberg in the hall. He smiled and told me that they had stopped grading my test when it was clear that I had passed the Masters hurdle, since they knew that I intended to go to MIT. I think both of us were relieved that I was leaving Columbia.

Russ Abbott and Ralph Kopperman left for Harvard and MIT after graduating from Columbia the previous year. They were sharing an apartment in the Boston suburb of Allston, an enclave of low-rent apartments thickly populated by students, but not very convenient for commuting to MIT. Kopperman was now a graduate math student at MIT, and Abbott a graduate student in Computer Science at Harvard. I made arrangements to share the apartment with them. Now I needed a summer job in the Boston area. In late May I heard that someone was interviewing for summer jobs in MIT's large, defense-oriented Lincoln Laboratory. I asked to be interviewed. The interviewer already had a full slate of candidates but agreed to see me late in the day. I had learned something about Lincoln Lab's computers from Professor Unger, who was a few years earlier a graduate student in Electrical Engineering at MIT. This impressed the interviewer sufficiently that he offered me a summer job. Unger had also strongly advised me not to apply to MIT's Electrical Engineering Department (now the Department of Electrical Engineering and Computer Science), since its doctoral admissions exam was notoriously difficult, especially for someone without an undergraduate degree in EE.

Just as I was about to leave for Boston, I received a call from Professor Patrick Fischer at Harvard, who told me that they had awarded a fellowship to a student who turned it down, and now they would like to award it to me if I changed my mind and went to Harvard. I told him of my interest in AI and in working with Minsky. He said that he knew Minsky's colleague McCarthy (who had created the LISP language), and that MIT offered a poorer research environment than Harvard did. I told him that I had my mind made up, but he convinced me to visit him at Harvard when I came to the Boston area.

Chapter 5: MIT—Graduate Years (1963–1967)

Abbott gave me a ride from his parent's home outside NYC to the apartment in Allston and showed me my bedroom. I wasn't expected to share in all the food costs, since I ate kosher food only, and the others did not. As a result I would often eat dinner at Rubin's Kosher Delicatessen on Harvard Street, within walking distance of our apartment. There were several regulars who would eat dinner there each night. When Mr. Rubin retired, the restaurant moved into a fancier place down the street. I still go there occasionally with members of my family.

I had agreed to share Abbott's car expenses in exchange for rides. I didn't know how to drive a car, which was no great loss in New York, but was a bigger issue in the Boston area, as I would soon discover. Lincoln Laboratory was in Lexington, approximately twenty miles from the Allston apartment; getting to it without a car was a nontrivial undertaking. If Abbott couldn't take me to Harvard Square, there was a roundabout route by subway. At Harvard Square there was a Lexington bus that stopped at Lincoln Lab. The total trip from Allston was about an hour and a half, but I was used to this from my trips to Columbia.

My first day at the Lab was spent filling out paperwork. I received a temporary security clearance, but until I received full clearance—a few weeks later—my access within the Lab was restricted. Next I met my boss for the summer, Jack Nolan. Jack was tall and thin and wore a mustache, as I recall. He had obtained a Masters in Electrical Engineering for a thesis using MIT's first digital computer, the Whirlwind, and had written the first program that performed differentiation as is done in the calculus, that is, using symbols. Thus he was impressed by my interest in integration, which I expressed to the interviewer back in Columbia. Jack told me that another researcher at the lab, Jack Arnow, had proposed a computer with a memory each word of which would have 4096 bits. The question was how such a memory could be used to retrieve information expressed in natural language. Nolan would be away during the summer, which meant I would be left essentially on my own to deal with the problem. Nolan would be working on campus, at the Project MAC Summer Study, studying the implications of time-sharing, for which MIT had just received about $3 million a year, an enormous amount of money for research at the time. The Summer Study would kick off the project. MAC stood for Multi Access Computers as well as Machine Aided Cognition, which latter term indicated that Minsky and the entire AI Group were also part of the Project.

Soon after my arrival, Kopperman announced that he would leave the apartment after the summer. Thus the costs for Abbott and me had just increased quite a bit. Nevertheless we did not take on a third roommate. I do not recall why Ralph wanted to move. He had made quite a bit of progress on his Ph.D. thesis at MIT in the area of model theory, and was likely to finish his degree within a year. Even in those days, a two-year doctorate was pretty unusual. Ralph eventually became a Professor of Mathematics at the City College of New York. I met him again many years later when he complained of the need to teach remedial mathematics to entering students in the College.

Abbott and I formed the habit of going to a new restaurant each Saturday. I kept somewhat kosher by not eating meat or seafood at these restaurants, a practice I still hold today. There were a number of restaurants in Boston that were widely known for their good food, their unique ambiance, and sometimes both. Some of them are at this writing, forty years later, still in existence, but seldom as they were then. For example, we often went to Durgin Park, long established in the old marketplace near downtown Boston. It was famed for the smart-cracking insolence of its waitresses and for its long tables, where every party was forced to sit across from each other and next to strangers. For 95 cents the diner obtained an entire meal—soup, corn bread, a main dish (cod for me usually), dessert, and coffee. In the 1970s a major renovation of the market took place, and the butchers and wholesale produce men moved out and small specialty shops, boutiques, and souvenir shops moved in. Durgin Park changed character. Its clientele became tourists, and its prices quite high. Another restaurant was the Wursthaus, in Harvard Square, which managed to persist in spite of constantly escalating rents until the late 1990s. Here one could obtain German specialties, and beers from all over the world. In downtown Boston, between the former "clothing district" and the theatre district, Jacob Wirth's maintained a European tradition of sawdust on the floor, professional waiters with long white aprons covering their white shirts and bow-ties, genuine German-style cooking, and beers brewed in Wirth's own brewery, always served at cellar temperature to bring out the flavor. Jake Wirth's is still there, the ambiance only slightly dented by the presence of some waitresses and a number of ferns hanging here and there. A Bostonian will readily see that Russ and I were well fed, even though we did not go to expensive restaurants on these Saturdays.

Although my boss was not in residence at Lincoln, there were numerous interesting co-workers in the group. Peter Falb was a control theorist who was reputed to be independently wealthy and to spend mornings talking to his broker; Michael Athans, also a control theorist, was working with Falb on a text published as *Optimal Control Theory*. Michael became a professor of EE on campus, Peter a professor at Brown. In later years Peter spent a great deal of time on the MIT campus and Athans eventually retired to Portugal.

In order to solve the problem of information retrieval using natural language, I researched information retrieval, and soon came across recent work at Harvard by Ed Sussenguth that seemed related to the problem. My approach was to assume that the question was phrased in logic with ANDs and ORs, and that the information was to be organized in memory in the form of a tree structure. I decided that the key issue was the degree at which the tree structure branched, and determined that the optimal branching factor was related to e, whose value is approximately 2.7 and is the base of the natural logarithms. I also showed that the speed at which one could traverse such trees did not vary much when one was close to e. Thus a branching factor of 3 was acceptable. I wrote this idea up and waited for Nolan to return at the end of the summer. In the meantime I prepared for the new work in AI that I hoped to undertake in the fall, beginning with an in-depth study of Minsky's papers. Little did I realize then that I would spend parts of my career railing against tree structures and their relation to problem solving and AI.

Minsky was unusually well read in a number of areas, such as psychology and neurophysiology, and his papers had numerous references. Since Lincoln Lab had an arrangement with the libraries on campus that they would reproduce papers requested by Lincoln staff members, I sat down and requested about a hundred papers that seemed important in Minsky's writings. Sure enough, the papers landed on my desk a few days later. It wasn't until years later when Lincoln Lab reported to me as MIT's provost that I heard that my request for so many papers was considered outrageous by some at the Lab.

Nolan finally came back toward the end of the summer, and I explained my results on tree searching. He was disappointed, and felt that I missed the point of his assignment. Natural language understanding is exceedingly difficult for computers even today, but I had assumed much of the problem away while I concentrated on looking up information that was already digested. I could not disagree with Nolan's criticism, but it was too late to do much about it. Even today I am not sure how a 4096-bit computer word structure would be of major value in natural language understanding.

As the new term began, the Research Laboratory for Electronics (RLE), my laboratory sponsor, gave me an office on the second floor of Building 26, near the heart of the main campus. My officemate, Paul Zeiger, was finishing a thesis in the theory of finite state machines in the EE department. In a connecting office a husband-and-wife team worked with Professor Jerome Lettvin on neural electrical signals: I believe they experimented on the conduction of electrical signals in the long nerves in lobsters, and then ate the lobsters. The breadth of research in that office was indicative of the breadth of work in RLE at the time. I was disappointed to learn that Minsky was no longer in Building 26, having moved to a building in the new complex called Technology Square, just across the railroad tracks from MIT. Technology Square was where Nolan had worked during the summer. Building 26 was a relatively new building then. It housed the central computer for MIT, which was a copy of the IBM 7094 computer at Project MAC in Technology Square. Even though Building 26 was relatively new, it had some of the features of the original buildings at MIT, which were built in 1916. The basement of the building had exposed pipes, just as the main buildings' basements did. The building was quite utilitarian, as I think the main buildings were. These buildings continually remind me of the engineering focus of MIT at that time, and for many years thereafter.

As a research assistant I was allowed to enroll in two courses, so I began the term by choosing two graduate math subjects, one on mathematical analysis taught by a Professor Siegel, the other on stochastic systems, similar to those I had taken at Columbia.

I was pretty confident when I began Siegel's course. I had a surprise coming. Siegel began the lecture by listing all the results that he would be assuming. I knew some of them, but certainly not all, and was shocked by the amount of material that he assumed we all knew. This approach may have been intended to drive students out of the course—it certainly had that effect on me. I now knew that MIT's Math department was more high-powered than Columbia's, at least some of it was.

This impression was confirmed in the other course, on stochastic systems. I became pretty depressed. Fortunately, the math department had relatively few requirements, and thus I could even take courses outside the department and still meet the departmental requirements, if I exercised some care in my choice of subjects. I switched both courses for ones in the EE Department, signing up for one by Victor Yngve of COMIT fame, and another by John McCarthy, the designer of LISP. McCarthy had just left to go to a newly established department of computer science at Stanford, but his course was ably taught by a staff member, Michael Levin. In contrast to my experience in the math courses, I found these courses easy and fun.

As I was making these changes I repeatedly tried to see Professor Minsky. My Research Assistantship was with the RLE and not directly with him, but I wanted him to supervise my work. He told me that he was “full up” with graduate students. Nor was he interested in redoing Slagle’s work on integration, or apparently that of any other of his former students; it was time, he felt, to do something requiring Artificial Intelligence in a different field, and this is what each of his graduate students was doing.

I simply could not let him take this stand. The third time I tried to get another appointment with Minsky, his determined secretary, Cynthia Solomon, physically blocked my access to his office. I began to waylay him as he walked to his class and walked with him, pressing my suit all the way. Eventually he relented to the extent of allowing me to attend a seminar on AI he was running that term. My RA continued to be funded by RLE, however, rather than by Minsky.

Michael Levin had been a co-author of the LISP 1.5 manual, and under Mike I actually began to learn how to program in LISP. One of the key examples in LISP was how to program a symbolic differentiation program in a few lines. Recall that this was also Nolan’s Masters thesis problem, but he had used Whirlwind, with only 64 words of memory! I used this differentiation example as my term paper for Yngve’s course. COMIT was a string-processing language. Thus I wrote a program for differentiation that assumed a linear, FORTRAN-like, input for an expression in the calculus, and then used COMIT rules to convert the expression to a LISP-like form, which had the operation, such as +, in the front; that is, I converted the expression to prefix notation. Now the LISP procedure for differentiation came into play, and I converted the resulting LISP expression back to a linear FORTRAN-like string in infix form. One of the problems with this approach was that a simple example became a long string. For example, differentiating 2*x resulted in 0*x+2*1 instead of the answer 2. So I wrote a few rules for simplifying expressions. Yngve was very happy with this paper; I found out later that he submitted it as part of the report on his research that he presented to his sponsors. The project taught me a few lessons: first, that string processing is not the way to go in dealing with the automation of calculus;

second, that simplification is extremely important in any symbolic manipulation system.

Since I was expected to spend half my time doing research, I decided to get a copy of James Slagle's program and analyze it. Slagle's work was very impressive, at the very least because he was blind. Apparently he had learned to read IBM cards by feeling the holes in the cards. I found out that he developed a pattern-matching program, ELINST (ELementary INSTance) as a key part of his system for integration, which was called SAINT, for Symbolic Automatic INTegrator. Slagle was Catholic, by the way, and had attended St. John's in New York. This might explain the name given his system. A technical problem that Slagle faced was that LISP at the time did not have a compiler, and thus programs used up a great deal of computer memory space in addition to being slow even for those days. The computer he used, the IBM 7094, had 32K computer words, a tiny amount by today's standards. Thus, Slagle had to husband program space very carefully. His pattern matching program, a key to SAINT, used a technique of passing procedures as inputs to other procedures. LISP was then one of the few languages that could successfully handle such a technique; it saved a great deal of space in the 7094 memory. The technique is quite involuted: to gain an understanding of it, I had to try the program on various inputs. I also discovered that Slagle relied on a simplification program written as a Masters thesis by Saunders a year or so earlier. To obtain speed, Saunders wrote in LISP's Assembly Program, LAP.

I read one of Slagle's preliminary papers, written in 1959. It gave a key reason why SAINT performed integration the way it did. Slagle wanted to test out a problem-solving theory that relied on searching tree structures. The idea was to look at patterns in the integrand and try all the methods that fit that pattern. These attempts yielded new integration subproblems, so this tree-searching approach was used on these problems as well; eventually a sizable tree of subproblems would be generated. When a subproblem was solved, then one went back up the tree, and combined that solution with solutions to other subproblems. One either could fully solve all the subproblems, or would run out of methods to consider that would create new unsolved subproblems, in which case the program would report that it could not integrate the problem.

It took me some months, but I eventually came to the conclusion that Slagle's methods, especially in problems involving trigonometric functions, tended to generate overly many subproblems. This made the tree of subproblems get fairly sizable, and thus permitted a test of the problem-solving approach in which he was interested. On the other hand, I did not feel that from a pure integration point of view Slagle's methods were ideal. I increasingly felt that his approach to problem solving in general was not very effective in many, if not most cases.

In Minsky's AI seminar I met a number of his other students. Tom Evans was just completing his doctoral dissertation on the analogies used in IQ tests. He wrote a program which assumes a description of the geometric shapes used in the analogy, such as circles, triangles, and lines, enabling determination of the answer that was the closest analogy by looking for differences in the descriptions. I thought this was an impressive achievement, just as Slagle's was. Dan Bobrow had already finished his dissertation and was now an assistant professor in the EE Department. His work was on algebra word problems, such as those found in ninth-grade texts. For example: "Mary is twice as old as Jane. Mary was three when Mary was as old as Jane is now. How old is Mary?" By looking for key words, he converted the English into linear equations. Solving the equations yielded the solution to the problem. My first few Masters students would work on extensions of these two dissertations.

Another graduate student who was a bit older than me, but also just starting out with Minsky was William A. Martin. Martin was an EE student who was working on Minsky's project, called the Mathematical Assistant. The idea behind this project was that a computer ought to be able to help a working engineer by allowing him to solve a complex problem one step at a time. Computers had been used in doing numerical calculations from the first day they were built. The calculations that Bill Martin was interested in involved mathematical expressions or formulas, not just numbers, and this is what made it interesting to Minsky as well as me. The computer would be asked to perform the symbolic calculations, such as those in the calculus, and the human would figure out which steps needed to be taken next. Martin had worked on a graphical display of mathematical expressions on a cathode ray tube which was part of the DEC PDP1. The PDP1 was one of the earliest minicomputers, and MIT had one of the earliest models, partly because DEC's founder, Ken Olsen, was an MIT graduate and a former employee of Lincoln Lab. I enjoyed talking to Martin, although we did not work together on the project at the time.

Arthur Samuel was visiting Minsky at the time, and also participated in the AI seminar. Samuel had just retired from IBM. He sent me a letter when I received the IBM fellowship a year earlier. Samuel was well known in AI for having written a checker playing program that actually learned to improve its level of play by modifying coefficients as it played that determined its next moves. It had once beaten the person who soon thereafter became the Connecticut state checker champion. He regaled us with stories of how he managed to get significant time to exercise his program. He went to the factory that built the IBM 704, one of the largest and most expensive machines of its time. While the new machines were being tested, he managed to get into the factory in the evening and run his program. Samuel later on spent many years at the Stanford AI Lab run by John McCarthy.

One day I was eating lunch in Walker Memorial, the main dining room at MIT. I ate alone at one of the large tables, and an elderly man sat down opposite me. He said, "My name is Norbert Wiener. How are you?" I said, "I'm fine. Thank you." Of course I had heard of the great Norbert Wiener, one of MIT's legends, and certainly the biggest name in its Mathematics Department. I was just too shy to engage in a conversation with the great man, even if he invited it. I had read

Wiener's two autobiographical books, *Ex Prodigy* and *I Am a Mathematician*. In one he mentioned that he walked from Columbia's campus down to Times Square one night. I decided to do the same while I was a graduate student at Columbia. A decade later this nighttime walk would have been quite unwise. The most unusual thing that occurred when I made the walk was that around 50th Street a pimp came out of the shadows and asked if I wanted to meet one of his girls. I simply walked on to Times Square. Wiener created the study of cybernetics in the late 1940's. Cybernetics was a precursor to AI, and I knew that Wiener knew Minsky and thought highly of his work, but I simply could not get myself to point out my involvement with Minsky during the lunch. A year or so later I heard Wiener give a talk in which he pointed out that Samuel's checker playing program learned so much that it could beat Samuel. Wiener died a few years later. Over the years I collected as many stories about Wiener as I could, and have repeated them whenever I could find a new audience. Here are a few of these stories.

The most famous story about Wiener is the following: One day a student saw Wiener walking along the infinite corridor (a quarter mile long corridor connecting many MIT buildings). The student stopped Wiener and engaged him in a conversation. When they were about to break up Wiener asked the student which direction he had come from. The student pointed to one end of the corridor. Wiener then said: "Good, I must have had lunch already." I first heard this story in Israel, and it was said of Einstein. I believe that they used Einstein because everyone had heard of Einstein, but the story is clearly of Wiener. While the corridor story is the most famous one, the following story gets the biggest laughs.

One day Wiener had moved from one block in Cambridge to another. When it came time to go home he remembered that he had moved and the block he moved to, but forgot the exact house to which the family moved. He did not want to embarrass himself by asking people to point out the house, so he figured out a strategy for finding his new house. He drove to the block where the house was located, and asked the first person he met, a young girl, “Young lady, I understand that a new family moved into this block today. Can you please tell me which house they moved to?” The young girl looked up and said “Daddy! We moved to the house next door.” I researched that story, and it is not true. Wiener’s daughter claimed that her mother sent her to the old house. When her father showed up there, she led him to the new house.

One day Wiener came home by cab from MIT after giving a talk in Providence, Rhode Island. His wife asked him where he left the car. That is when he realized that he took the car to Providence, but returned to Boston by train, leaving his car in Providence.

Here is a story about Wiener that I first heard from Jerry Lettvin. Wiener often read books while walking along the corridors of the Mathematics building (Building 2 in MIT lingo). He did this in an unusual position. He held the book above his head and looked up to read it. Why did he do so? Wiener had very poor eyesight. He wore bifocals and even trifocals as he got older. As those who wear bifocals can attest, it is tricky walking down stairs because the lenses for reading are then closest to the ground, and things look pretty unclear. So Wiener rearranged his lenses, so that the reading lenses were on top. Thus he read with his head up high. How did Wiener know where he was going while he read along the way? He used the forefinger of his right hand to count the doors to his destination. This leads to the following story. Wiener was walking to his class, and was quite lost in thought while reading a book. He counted down the number of doors along the way, entered the class, walked all around it, and walked out. A student had to go and bring him back.

Wiener was not the best classroom teacher. There are several stories about his teaching. Wiener was teaching an undergraduate class. He wrote some statements in a proof on the blackboard. One student raised his hand and said that he could not understand how one statement followed another. Wiener looked at the blackboard and said, “It is obvious.” The student persisted “But, Professor Wiener, it is not obvious to me.” Wiener looked at the board again, studied it carefully, and then announced “It is obvious.” The student persisted in not understanding how one statement led to the other. This time Wiener walked out of the classroom, went into his office, and returned triumphantly and announced “Ah. It’s obvious.” The student still was confused. So Wiener exclaimed “I don’t understand why you are having so much difficulty. I derived it three different ways.”

During the Second World War Wiener developed a theory about the processing of the signals that emanated from radar. This theory led to the study of signal processing, a field that had a major impact on electrical engineering after the war. Wiener taught a course on his new theory right after the war, when the material was declassified. He had a tendency to turn toward the class after making some statement, and then nodding. He felt that his statement was understood when one of the students would nod back. Eventually one of the students was effectively designated the nodder and class could not continue until he nodded. Wiener’s teaching drove many students off, including one Friday the latest nodder. Only three students were left, each strong-willed. One was Jerry Wiesner, later to become Provost and then President of MIT. Another was J.C.R. Licklider, a well-known psychologist, later to become Director of Project MAC. When class resumed on Monday, Wiener made a statement, turned around and nodded. None of the three students wanted to nod. So Wiener nodded again, and there still was no response. Wiener kept on nodding, and finally Licklider, the psychologist, caved in and nodded. He nodded the rest of the term.

Wiener had numerous insecurities. His father had bet a Harvard colleague that he could bring up his son to be a genius. Later on his father would claim that Wiener’s intellectual capabilities were due to the father’s efforts. No wonder Wiener was insecure. One day he was chauffeuring a famous Hungarian mathematician, named Paul Erdös. Wiener suddenly stopped the car on a main thoroughfare in Cambridge, Memorial Drive. Wiener turned to Erdös and said, “Do you think I am a good mathematician?” The frightened passenger said, “Yes, of course you are. For heaven’s sake, please drive!”

I heard one story at MIT’s celebration of the centennial of Wiener’s birth. A lady got up and said that her family and the Wieners had summer homes on a pond in New Hampshire. She saw Professor Wiener swimming there many times. He always used the same stroke. She asked “Does anyone here know what stroke he used?” No one knew. She said that he used the backstroke because he wanted to continue to smoke a cigar while he was swimming.

I tried to get MIT’s Mathematics Department to issue a collection of Wiener stories on that occasion, but they did not wish to do so. There are many other Wiener stories, of course, but this memoir is not about Wiener. So we shall move on with our tale.

One day in late November in 1963 I went to Project MAC in Technology Square, and one of the graduate students told me that the President had just died. I was not particularly bothered by this, figuring that MIT would find another president soon enough. I realized as the discussion continued that they meant that the U.S. President, John F. Kennedy, had just died. I was shocked, as was the rest of the country. We were glued to the TV for the next three days until Kennedy was buried on the Monday following. My twenty-second birthday went by quietly that day.

One day in December when I returned to the apartment from MIT, Abbott looked very serious. He told me that my grandmother had just died, and that I was to go back to Brooklyn for the burial services. I was surprised by this news. I knew that my grandmother had been getting weaker. My parents had arranged for her to have a helper, a young woman from South America who was able to come to the States because we promised her a job. The young woman stayed in my grandmother's bedroom, and my brother moved into mine. Although I was not aware of the details at the time, it appears that my grandmother's lungs were filling with fluid due to the weakness in her heart. When she was brought to the hospital in Brooklyn, they gave her a pacemaker. Pacemakers were quite new in those days. Unfortunately, her pacemaker failed after a short while, and she died. The doctors asked for permission to autopsy her and see if they could figure out why the pacemaker failed. My parents gave them the needed permission, although Jewish custom is not to disturb the body of a dead person at all.

My grandmother was buried at the Mount Lebanon cemetery, not far from where her son, Sam, lay. My aunt Frieda attended, as did Shirley, Sam's former wife. Shirley had by then remarried. I think all six of my grandmother's grandchildren attended the service. The rabbi in my parent's synagogue gave the sermon. We prompted him with details of her life half an hour beforehand. She had a difficult life, having been through three wars. She managed to see all her children in NYC, and that did make her happy, although I would not say that she was a person who became happy easily. Actually, I recall her telling me that I was not a person who was happy for long after some success or another. She had a great influence on me since she brought me up for many years while my mother worked, both in Israel and in the US.

I registered for two courses once again for the spring term of 1964. Minsky's course on AI was an obvious choice. I also wanted to take another course in computer science theory. The only one then available was on recursive functions and was offered at Harvard by Patrick Fischer, the one who wanted to give me a fellowship. Minsky kept insisting that I not redo Slagle's thesis, and I was therefore looking for some theoretical approach to integration as an alternative.

Fischer used notes on recursive functions that were written by an MIT professor, Hartley Rogers. In contrast to the notes used in Mendelson's course in Columbia, these notes were more elementary in parts, and very clear throughout. The class was also very interesting. One of the students was Pat's brother, Michael Fischer. Another was a Swedish student, Stal Aandera. He later became a well-known theoretical computer scientist. We also had some advanced graduate students in the class. One was Joyce Friedman. I recall that when Fischer returned the midterms, he noted that Joyce's exam was written by a professional. Joyce later became chair of the Computer Science Department at Boston University. Abbott was also in the class, thus continuing a tradition going back to Columbia.

Although none of us knew the import of this fact until decades later, the Unabomber was a graduate student in mathematics at Harvard at the time. He sent Pat Fischer a letter bomb in the 1980's. Fischer's secretary opened the package and her hands were severely injured in the subsequent explosion. What many of us were aware of at that time was that Mssrs. Alpert and Leary were at Harvard that year. Numerous Harvard students took LSD and related drugs at that time under their tutelage.

Minsky's course on AI was a riot. Marvin never prepared for class very much. He certainly had a topic in mind for the lecture, but he would wander all over the place as students' questions would cause him to make significant shifts in topic. It was clear that his mind was very broad and quite deep. The students must have all been impressed with him.

I began to do research on the question of whether integration was an unsolvable problem. The great German mathematician David Hilbert (1862–1943) led the mathematics community in the quest for a proof that all true mathematical statements were in fact provable. The community was shocked when Gödel proved in 1931 that this was not so. I have since wondered whether it was entirely accidental that such a negative result would be produced during the Depression. Later in the 1930s logicians expanded the number of unsolvable questions, showing that there was no computer algorithm that could fully determine the solution to each of a set of questions. Hilbert had in 1900 defined a set of mathematical problems that took the mathematical community decades to resolve. His tenth problem was to find an algorithm that determined whether each polynomial with integer coefficients had a solution using integer values for all the variables. It was known that the problem was extremely difficult; in the early 1960s a Russian mathematician showed that it was unsolvable in general—that is, there is no algorithm that given any polynomial in several variables with integer coefficients could decide whether the polynomial had a solution where all variables had integer values. The question I explored was whether integration problems in one real variable could be changed to polynomial problems in multiple integer variables. If this were done carefully one might be able to prove that integration was in general an unsolvable problem too. This would have been consistent with the 1905 conjecture of the British mathematician G.H. Hardy, who believed that, contrary to Hilbert, the problem of integration, that is deciding whether an integration problem had a solution in terms of the usual functions in the calculus, and that had stymied so many great mathematicians in the nineteenth century, was not solvable in finite time and space.

Hardy was Britain's leading mathematician for several decades. He defined what a pure mathematician did in an essay entitled "A Mathematician's Apology." I like the following story about him. He summered before World War I in the home of Harald Bohr, the brother of the famous physicist Niels Bohr, and a great mathematician in his own right. Each summer Hardy would send Bohr a postcard from the ship he took back to Cambridge. In it he would claim to have solved the Riemann Hypothesis, then and now considered the hardest unsolved problem in mathematics. He would also state that due to the small space in the card the details would follow when he returned to the university. Each year Bohr would get a second card from Cambridge University telling him that Hardy had found a flaw in the proof, and that he'd see him again next summer. Bohr finally got fed up with this exchange and asked Hardy to explain the business with the two cards. Hardy's response was that there was a small probability that the ship back to England would sink, and when Bohr received the first card he and other mathematicians might not be so sure that Hardy had not solved the Riemann Hypothesis.

Hilbert was arguably the greatest mathematician of the first half of the twentieth century. He had some characteristics that remind one of Norbert Wiener, among them that he was easily confused. Several stories about Hilbert bear repeating here. One relates to a new mathematics professor in Göttingen. The tradition was that the new professor had to take a carriage on his first Sunday and visit the homes of each of the other mathematics professors. The new professor did so, and finally arrived at the home of the great David Hilbert, but he was so overawed that he did not know what to say. Eventually Hilbert became confused, shook the young man's hand, went outside, entered the man's carriage, and left. On another occasion, he and his wife were giving a party at their home. Just before the guests were to arrive Mrs. Hilbert noticed that David's collar was dirty. She asked him to go upstairs to change his shirt. The guests started arriving but Hilbert did not return. Eventually his wife went upstairs to investigate. She found that he had indeed taken off his shirt, but then, confused, he took off the rest of his clothes, put on a pair of pajamas, and was fast asleep. And there was the time he was asked to give a eulogy at the burial of one of his young collaborators who had died suddenly. He started by saying what a great loss it is to the world for such a young man to have died. He then pointed out that just recently this young man proved an important theorem, whereupon he lost track of the solemn occasion and began proving the theorem in front of the largely mystified crowd.

I began to make progress on the unsolvability of the integration problem. The first step was to convert a polynomial in several variables to a more general function in the calculus containing only one real variable. This was doable with different trigonometric functions used as codes for each variable in the polynomial. In addition it must be specified that the real variable, x, say, needed to be an integer. This could be done by using the code $\sin^2(\pi x)$, since this expression is zero if and only if x is an integer and is positive otherwise. Now we had a problem that if it were zero, the original polynomial had integer solutions and the integral existed; if, however, the value of the polynomial was much higher than zero, the original polynomial did not have integer solutions and the integral did not exist in closed form. The final case was if the value of the expression was small, but not zero. In this case we had no definite conclusion. If I could get around this issue, I'd be close to the proof.

In the spring of 1964 Abbott and I decided to split up as roommates. He needed greater privacy than our apartment provided, since I seemed to have the unfortunate gift of coming home when he was on the couch making love to one of his girlfriends. I looked around for suitable roommates, and saw an ad for an apartment with a kosher kitchen for MIT students on Massachusetts Avenue in Cambridge, within walking distance from MIT. It was run by David Smith, a graduate student in physical chemistry. The previous year the apartment had had four roommates. During the summer there were three of us, but the third one, a grad student in EE specializing in lasers, left after the summer. What made the apartment so interesting was that in addition to Smith and me we also inherited two kosher eaters who came for dinner five nights a week, paying us a dollar apiece each night. We had kosher meat delivered from Newton a couple of times a month. One of the eaters had a car, and we went to a supermarket on Memorial Drive to load up on cans of food about once a month. David taught me how to cook a few meals. For example, I learned how to cook liver, making it kosher by placing a lot of kosher salt on it to draw the excess blood, then rinsing it before cooking. My wife Peggy will not, however, let me cook liver these days for the simple reason that she hates eating it. Chicken was our specialty on Friday nights, but David kept the preparation of the chickens to himself. The eaters complained about the lack of variety in the meals, due in part to the fact that neither David nor I had a car and thus could not purchase fresh fruits and vegetables. We used this handicap as an excuse to serve Kool Aid for our beverage; nevertheless, the eaters continued to come regularly each evening that year.

One of the eaters prided himself on how broadly educated he was. He was a graduate student in physics, I believe, but he knew how to play an organ, and knew a lot about the classics. He realized that I seemed to know a lot of things as well, so he would tease me at dinner with questions about one thing or another. He quit after a while when he recognized how difficult it was to stump me. Like most people whose chief interest is reading, I had taken my knowledge for granted, and was surprised to find it was broader than that of the average person.

That summer my old classmate, Ira Sherman, was visiting the Boston area, and spent a day at our apartment. He had graduated from RPI with a degree in architecture, just as he had planned on doing in junior high school. His mother had Multiple Sclerosis for many years, and died a few years later. Eventually Ira took over his father's architectural firm in Brooklyn. I envied him in the first year or so of college because he had to take drawing classes with nude female models. It may not have had the same effect on him since he turned out to be gay. He still has an architectural firm and lives with his partner in Brooklyn.

Minsky still did not have the funding to support me, so I spent the summer with IBM again, in their center in the same building as Project MAC in Technology Square. One of their projects was in symbolic manipulation, for which they were building a system called Formac. Jean Sammet, the project leader, hired me to work under Bob Tobey, one of the leaders in the project. Tobey asked me to use my LISP knowledge to figure out how they could improve Formac. I read the manual on Formac and used it on some problems. Formac had built-in simplification routines, and as expected these routines had their limitations. I wanted to extend the capabilities of Formac in this area, but Formac lacked the machinery for doing so. Thus I explored the idea of adding a pattern-matching capability to Formac, not unlike the one that Slagle had developed for his SAINT program. I had some difficulty figuring out how to program such a pattern matcher, even in LISP. So I went upstairs and used my own account on Project

MAC's timeshared computers to try out my LISP code. Tobey came looking for me one day, and ordered me back to the IBM center.

The Formac group would be called dysfunctional today. Tobey would regularly argue with Sammet, whom he clearly did not respect. The project had a manager, Elaine Bond, whose role I did not quite understand. Elaine eventually rose very high in IBM's management hierarchy. The project had other students in addition to myself. One was Michael Fischer, Pat Fischer's brother. He was close to finishing his doctoral thesis at Harvard. What he did for the project was unclear to me. Nor was it clear whether one could implement any significant changes to the Formac system at that point. I continued to argue for a pattern-matching capability, but I was ignored.

Jean was a determined person. She would see the project to completion, and use it as a stepping-stone to increasingly greater roles in the computer society, ACM, the Association for Computing Machinery. She eventually became President of the ACM and a member of the National Academy of Engineering. One of her major achievements, in my view, was that she nearly single-handedly created the group of people who were interested in symbolic algorithms and systems, SIGSAM, the Special Interest Group for Symbolic and Algebraic Manipulation, becoming its first chairman. I became its third chairman a few years thereafter. IBM lent its considerable stature to the nascent field of symbolic computing with its support of Formac. Unfortunately Formac was a relatively poor product, but that gave the rest of the community of system builders a considerable advantage.

In the fall of 1964 I enrolled in two subjects in the Mathematics Department. One was on numerical methods, and was taught by Professor Gilbert Strang. The other was on recursive functions, and was taught by Professor Hartley Rogers. It could be argued that I should not have been taking the latter course after the one at Harvard that was based on Rogers's notes. My excuse is that the theoretical thesis I was pursuing was in a related area. Professor Rogers was still revising his book, and would offer new material. Moreover, it was time to find three mathematics professors to examine me for the doctoral Oral Examination, and Rogers could be one. I believe Rogers, whose lectures were clear and insightful, was the best mathematics teacher I had ever had. While he was revising his book on the subject, people continued to send research results to him, but the moment the book was published, the research in recursive function theory appears to have nearly dried up.

Professor Strang had an interesting lecturing style. He would stop in the middle of each lecture looking quite confused. He'd turn around and ask us to help him make the next step. Normally none of us knew what to do, and after a few seconds he would magically figure out what to do next, and the lecture would proceed. I fell for this technique every time! He was a great lecturer too, especially in his undergraduate subject on linear algebra, and won several awards for his teaching in his career.

I continued my research on proving the unsolvability of integration. In the middle of the term I heard, and at this point I do not recall how I heard, that a Daniel Richardson on the West Coast had just proved this result. I obtained his proof, and it went along exactly the same lines I had been pursuing. He managed to get around the difficulty I had had, only to encounter yet another. Richardson's solution is controversial: it uses the absolute-value function, which enabled him to complete the proof but which most people do not believe is a proper function in the calculus.

I was very disappointed with this turn of events. I went to Minsky and told him about it, obtaining an agreement from him that I could pursue my original goal of creating an integration program that would do as well as was then possible. Minsky and others probably expected me to be devastated, but I was not, since I was now able to do what I had originally set out to do. I can see now that had I obtained a doctorate based on Richardson's result I would probably have wound up as a mathematician or a theoretical computer scientist, and only a third-rate researcher in either one of these areas. Once again it appears that getting a

Ph.D. at the age of 22 or 23 has some advantages, but also some disadvantages when it comes too easily.

First I needed to develop the software foundations for a new integration program, beginning by creating a new pattern-matching program that would have greater capabilities than Slagle's ELINST. It would be called Schatchen, the Yiddish word for a male matchmaker, which I thought amusing and appropriate. Schatchen did not use LISP's feature for passing programs or procedures as data, but was written as a language itself. This was largely due to the fact that LISP compilers of the time could not correctly handle the procedure passing feature. A few years later I wrote a paper explaining why this was so. The paper was called "The Function of FUNCTION in LISP." As can be seen, I permitted my puns to enter my programs as well as the titles of my papers.

I had several goals for the spring of 1965. Not only did I have to pass the doctoral oral exam in mathematics, but it was important that I attend my brother's Bar Mitzvah in New York. I really prepared for the Bar Mitzvah. I do not recall why I thought that the cha-cha would help me in the party that would follow it, but I went to an Arthur Murray studio to learn some ballroom dancing. Likely my parents were behind this move. I also had a suit tailor-made at Brooks Brothers. I knew only that the name was synonymous with fine tailoring; I did not realize that they specialized in an old-fashioned style. I needed to get several fittings, and just barely managed to get the suit in time. Abe was an outstanding student in all his subjects in junior high school, and his performance in the synagogue was very good. My parents had invited about 75 people to the party afterward, in contrast to the small party they had been able to provide for me a decade earlier. My father, always a great joke teller, tried to tell jokes in English, but his natural language for telling jokes was German and his English ones were not a big success. I got a chance to practice my dancing with my cousin Linda, Sam's daughter, and only stepped on her feet once or twice. I heard afterward that some people were concerned that Linda and I might get married, since marriage among cousins was not disallowed in Jewish tradition, and was used by rich

Ashkenazi families as a way of keeping their wealth in the family. Of course, there was no chance of this since I immediately went back to Boston. This happy occasion would turn out to be the last time my extended family came together. Linda later married a very orthodox Jew. Unfortunately, she died of brain cancer at a relatively young age, and her mother, Shirley, wound up taking care of her children.

The doctoral oral exam was unusually difficult to arrange. Minsky, who was spending the spring term at Stanford, expected to be in the Boston area for just one day in May, leaving the same day for New York to deliver a major paper at an international computer science conference in New York. So we scheduled the exam for the morning of that day. Shortly before the exam I found out that Hartley Rogers, one of the other two examiners, would be in California the day of the exam, but if we scheduled his part for a week before Minsky would arrive he would report his findings to Minsky. I was very nervous when I walked into the classroom to be examined by Rogers. I had had many oral exams in Israel, and was more comfortable with the form than most Americans. Nevertheless my mouth was very dry. Foolishly I argued with Rogers over which question he would ask. So he asked one that I felt would be more appropriate to what I considered my level of accomplishment. Of course it was a much harder problem, and I had great difficulty with it.

Come the day of the exam, and guess who did not make it. Minsky, of course. Prof. Strang was there, and after he had waited half an hour for Marvin to show up, he started to ask his question. I did not fare extremely well on this one either. Minsky walked in about an hour late, apologized to both of us, and simply asked for me to talk about my thesis plans. I talked for about five minutes, and was asked to leave the room. Minsky and Strang deliberated for another ten minutes, and then Marvin walked out and told me that I had passed. I can only imagine that Minsky told the others that he expected me to do well on the thesis, and that no matter how poorly I might have done on their questions I should be passed.

I must have been fairly sure at that time that I would eventually obtain a Ph.D. and have a reasonable career; even so, passing the doctoral oral exam was a major psychological boost. The effects were immediate: my weight had risen to about 250 pounds that spring, so I celebrated passing my orals by deciding to lose weight. In ten weeks I lost 60 pounds, mostly by not eating on Sundays and skipping meals on other days as well. I now needed an entirely new wardrobe. That summer four college coeds leased the apartment across the hall from David Smith and me. Some of them were Jewish, and I could often be found in their apartment. Losing weight did not hurt my ability to relate to these young women.

David Smith finished his doctoral research that year. His thesis involved analyzing some elements near absolute zero degree temperature, which required obtaining liquid hydrogen. Harvard had a facility that generated the liquid hydrogen, and we both dreaded the day when David had to transport the explosive liquid across Cambridge in his car. He did it successfully, his apparatus did not leak, and he finished his experiment. David became an assistant professor at Penn State after submitting his dissertation to the Chemistry Department. One of the things that impressed me about David was his dedication to Judaism. He would walk several miles on Saturdays to visit the Boston Rebbe, Rabbi Soloveitchik, whose great fame I only realized decades later. He would also read the Torah on some Sabbath mornings. I did not realize how difficult this was at that time, but found out with my sons' Bar Mitzvah two decades later.

When David left I had to find another apartment. Tired of rooming with others, I took a room about the size of a dorm room, very close to the campus in what is now an MIT dorm, Random Hall. The rent was $15 a week, and a maid came in once a week to clean up the room.

We had a crisis in our research program in AI in the fall of 1965. Those of us who were using the LISP system on the IBM 7094 at Project MAC would soon be unable to use it further because the time-sharing system, CTSS, the Compatible Time Sharing System, was being modified. CTSS and its follow-on system, MULTICS, were the key projects at MAC. The head of these projects was Prof. Fernando Corbató, known as Corby. Some of the students said "Project MAC" stood for Minsky Against Corby, although I believe that their relationship was quite cordial. I would later work closely with Corby on academic administration. He was a superb systems thinker, as his work on CTSS and MULTICS clearly showed.

All the people who had initially developed LISP for the IBM 7094 machine had by then left MIT; due to Minsky's management style there was no one in charge of modifying LISP so that it would continue to run on CTSS. Since I needed to use the system for my research, I decided to make the modifications myself. I found an old listing of the assembly code for LISP which contained hand-written comments regarding errors and missing features written by the original group of programmers. I used this information to make patches in the binary version of LISP. In a weekend's worth of work I was able to get the LISP system to operate under the modified version of CTSS.

This made me a minor hero. Corby was happy, as were many in the AI Group who were dependent on LISP. I also became an adjunct to the growing group of hackers who worked for the AI Group, immortalized in Steven Levy's 1984 book, *Hackers*. Success with the patched system made me eager to make further improvements to the LISP on CTSS. I managed to get a tape with the assembly code, and could now make the binary changes at the level of assembly instructions. I could also contemplate making additional changes.

A key problem was that CTSS was not really oriented to reading and writing magnetic tapes, as were the usual IBM 7094 systems, such as the one at Columbia. Data, such as the assembly listing, were stored on a disk. Unfortunately, disk capacities in those days were relatively small, and my space allotment did not permit me to keep more than one copy of the LISP system at a time. One day, to my surprise, I logged onto CTSS and saw that I had an allotment of 32 million words. I knew that this had to be an error, but I managed to use it all up by the end of the day, and made rapid progress on modernizing the LISP system; the next day I logged on and my allotment had reverted to its normal level. What happened was that when CTSS had obtained a new disk, its second, on the previous day, one of the new hackers, Tom Knight, just admitted as a freshman, managed to assign the entire disk capacity to me that day. When the system administrators recognized this, they reverted to my original allotment. Nowadays a laptop computer would have a hundreds of times as much disk capacity as CTSS did for all its users at a small fraction of the price.

Most of the hackers were building a new LISP system for the DEC PDP6 minicomputer, as well an operating system for it, which they called ITS, the Incompatible Time-sharing System. This was a pun on CTSS, which claimed compatibility between time-sharing and batch processing. My early success with the CTSS LISP led me to continue activity related to LISP—at the time I didn't know this activity would continue for over fifteen years!

I now began to concentrate largely on research. Courses are really quite secondary in most doctoral programs; my own doctoral experience was strongly shaped by Minsky's approach to supervising his students. Basically he did not really supervise anyone in the usual sense. You went to him on occasion and he would listen to you and make some extremely interesting comments, some of which may have been relevant, many of which sounded outrageous, and others seemingly irrelevant. Students who finished a dissertation under Minsky were thus largely self-taught, and usually did well in their careers. Abou*t* a dozen of them became MIT faculty members—probably a record. I enjoyed talking to him because at the time AI was under attack as a field, and Minsky was its great defender. He also had many interesting friends that I enjoyed listening to.

Minsky's house was remarkable. He'd invite the students there on various occasions. It is a very large one in Brookline, near the BU Bridge. A rope hung from the living room ceiling, which he used to climb in order to impress us with his physical abilities. The basement was remarkable in another way: it contained enormous quantities of electronic parts that Minsky had gathered from government surplus sales. When we needed a part at MIT someone would often volunteer to go to Minsky's basement and look for it. The foyer featured an old jukebox. You'd need a nickel to play a record, but then you would get the nickel back.

Minsky's colleague, replacing John McCarthy as his intellectual partner, was Seymour Papert. Seymour was a child prodigy who was born in South Africa and obtained a Ph.D. in mathematics at Cambridge University. He somehow latched onto the great child psychologist, Jean Piaget, and spent seven years in Geneva working with children. Seymour came to MIT in 1963 to work with Warren McCulloch, a noted neurophysiologist, and soon afterward began to collaborate with Minsky. Thus in the mid-1960s I think Seymour and I were the only members of MIT's AI Group to have been born abroad. In retrospect I believe that this was the reason we tended to become intellectual allies, especially between 1965 and 1968. Seymour later spent most of his time on the language LOGO that he helped develop and on its use in K-12 education. Although I have been interested in K-12 education, I have not de-emphasized content, such as history or geography, to the extent that Seymour and his students have.

By the fall of 1965 Minsky's senior graduate students had all completed their dissertations, and in a period of only two years I found myself the senior student. The unwritten responsibility for leadership came naturally to me. AI was then a fringe intellectual field, and some of the students attracted to it were in a mental state best described as in somewhat tenuous balance. A few of them became psychiatric patients in the local hospitals. Although Marvin's wife, Gloria, was an MD, he was uncomfortable visiting these students, and he would sometimes ask me to do so. For reasons I do not fully understand, I have not been terribly uncomfortable dealing with people in either manic or depressive states.

During this time I used to come in late in the afternoon and leave around five in the morning. Sometimes there would be calls from overseas operators in the middle of the night. One came from Paris, and when the operator asked for Seymour and I said he wasn't there, I could hear a woman with a French accent calling, “Seymour, Seymour, where are you, Seymour?” A similar late-night call came from London and featured a woman with a British accent. Seymour surely had an active social life in those days.

The Northeast Blackout occurred early in November of 1965. A series of cascading events beginning with the failure of a relay in Ontario, Canada eventually led to the loss of electric power throughout the Northeast and parts of Canada*.* Our lights went off, and when we heard on the radio that the nearest military airfield, Hanscom Field, was also without lights, Minsky said that the blackout was likely caused by the Russians. He told us all to go home, which I certainly did.

By 1965 I was becoming increasingly disenchanted with the dominant AI paradigm, which was that problem solving involved a search for solutions to problems, using heuristics to reduce the search space. I asked whether it was felt that one could reproduce Einstein's Theory of Relativity from basic axioms and a great deal of search. In principle the answer was clearly "yes," but in practice it seemed silly to me to ignore all knowledge of physics beyond a few axioms. Moreover, the search space was likely to grow exponentially, and would be impossible to explore in a realistic fashion. I also felt that a child's ability to speak relatively quickly implied that it had become an expert at this task that did not rely on much search, even taking into account neural parallelism in the brain. When I was told that using knowledge beyond the axioms was akin to cheating, I began to look for allies for my view of problem solving, which relied on structured knowledge. The person I found who appeared most sympathetic to this view was Papert. I felt at the time that this was because he and I shared cultural backgrounds different from those dominant in the United States.

Minsky was one of four major figures who founded AI. A second was McCarthy, who championed both an axiomatic approach to AI as well as knowledge of "common sense." (This differed from my approach to knowledge in that I championed specialized and structured knowledge.) The remaining two founders were the team of Newell and Simon. Herb Simon is a major intellectual figure in the social sciences who began in political science, then played a key role in the Chicago movement in economics, influenced management education, and created the fields of cognitive science and AI. One of his key ideas is that problems could be solved by searching tree structures. He won a Nobel Prize in economics for pointing out that decision trees can grow very quickly, so that rational managers who hope to use such trees to make decisions will likely be disappointed, and thus needed to rely on “bounded rationality.” Allen Newell was the more technical computer scientist of the team. Together they created the General Problem Solver (GPS) system, which used differences between the goal and the current state of solution in order to generate new solution steps. GPS created a tree structure of intermediate situations, and spent its time searching the tree.

Given the positions of these four major figures, I tended to emphasize the lack of search (and thus the reduced use of tree structures) when specialized knowledge was available. In retrospect I probably overemphasized the undesirability of search, and underemphasized the structured aspects of this knowledge. Later on people began to buy into the need for knowledge in AI, but the issue of structure remains to this day one that I have not been able to sell it very well. I now believe that the U.S. culture tends to underemphasize the use of structures that I call layered structures. This tendency, I believe, is related to an ideology that places emphasis on competition rather than cooperation. That ideology is likely a result of the religious and cultural backgrounds of the people who began to settle in America, largely from Britain, in the 17^{th} century.

I now studied the mathematical attempts to solve the integration problem, and learned that the former head of the Columbia Mathematics Department, J.F. Ritt, had by 1950 made the biggest effort to combine much that was known about integration. Ritt committed suicide in the early 1950s, a possible reason being that his work received little attention. Indeed, there had not been much activity on the integration problem since the time Hardy essentially said that it was an impossible problem to solve. Much initial progress was made by Jean Liouville in the 1830s; the great German mathematician, Bernhard Riemann, made progress in the 1860s in the most difficult part of the problem, involving roots of polynomials in the integrand, in the process inventing the field of algebraic geometry. Many other famous mathematicians worked on integration problems related to Riemann's, but there were no breakthroughs. Algebraic geometry made great strides on other problems, but appeared to have left the concern over integration behind.

In parallel to my reading of the mathematical literature I formulated an approach to the integration program that I was going to design. I wanted to create a matrix of patterns on the one hand and techniques on the other. Thus an integrand that had only exponentials would have one technique associated with it, but if the integrand had both exponentials and logarithms then another technique might be used. I took my time developing the entries in this matrix.

I knew that in Project MAC I had access to a system which solved a class of problems that Slagle's system did not attempt to solve: a method of integrating rational functions (ratios of polynomials) that had been written by a group headed by Carl Engelman. Carl worked for the MITRE Corporation, an off-shoot of the MIT Lincoln Laboratory. Since he was interested in AI and his group used LISP for their work, they were allowed to work at Project MAC. The group used algorithms for factorization of polynomials that largely came out of van der Waerden's classic texts on algebra. The algorithms were not particularly efficient, but this was not a significant issue for me since the problems I expected to integrate did not involve polynomials of high degree or many variables. I had a technical problem in that Engelman's system, called Mathlab, used the entire 32K word memory of the IBM computer and I expected to need all this memory for my own software. Fortunately there was a technical solution that permitted one program to use another program in its entirety by giving it inputs on one disk file, running the other program, and getting its results in another file. This is what I planned to do with Engelman's system.

In May of 1966 a recent Ph.D. from Yale University visited MIT looking for a faculty position in computer science. His name was John Donovan. I did not attend his talk, but he came to the lab as I worked one evening, and asked me what I was working on. I said a project in AI. He remarked that it was unlikely that I could get a faculty position in AI at MIT. He intended to call himself a systems researcher, because it was clear that MIT would hire systems people rather than AI people. I had not thought of my career beyond the dissertation, but I found this a curious comment. Then he asked, "What exactly are you working on?" "Symbolic integration." "Oh," he said, "that's too bad." He'd just been to Carnegie-Mellon University and met a student there who was finishing a dissertation under the chairman, Alan Perlis, in the area of integration. This news was greatly upsetting to me, especially in light of my experience with Daniel Richardson.

Without telling Minsky what Donovan had said, I went to work to finish my doctoral work as quickly as possible. In two weeks of strenuous effort I managed to write most of the program. I ditched the matrix approach, using instead a three-step process. The first step solves easy problems, which comprise around 80% of the problems likely to be faced in practice; the second step solves more difficult problems using a set of methods triggered by patterns; the third step is more general and slow, and uses some of the mathematical ideas I found in the literature. Two dozen years later I saw the same three-step process used by Michael Hammer, a former MIT faculty member, to describe methods used in the reengineering of processes in business firms. I cannot be sure whether my work influenced Mike, but it would not surprise me. He used an example of an IBM subsidiary that eventually broke problems into three parts: the first solved easy problems (as I had guessed, this took care of about 80% of them); the second used a set of specialized methods to solve about 15%; and the remaining relatively complex problems would be solved by trained professionals, lawyers in this case.

The flow of subproblems in my program uses recursion. The subproblems get progressively simpler, and thus the program is guaranteed to terminate, whether the problem is integrable or not. This is in great contrast to Slagle's program, which could do a tree search in some cases forever. I decided to name the program SIN, for Symbolic Integrator, another pun, intended to contrast with Slagle's name for his program, which was SAINT.

After finishing what I considered the bulk of my dissertation project, I decided to write it up quickly in a memo so that I could get credit for it before the CMU graduate student whom Donovan had mentioned had finished his work. The impact of the memo was immediate and very positive, even from Minsky. One day shortly thereafter as I passed the office of an Assistant Professor, Douglas Maurer, I saw a copy of my memo. I naturally came in and realized that he was retyping the memo. Maurer had launched his own journal on mathematical algorithms a few months earlier, and he apparently intended to publish the memo. I protested that I had not submitted the memo for publication, not in his or any other journal. He was surprised, saying that he had thought I would appreciate rapid publication, but he agreed to drop the matter.

I sent the memo to a few people, including Slagle. He wrote me a very negative letter containing a dozen integrals that he had determined could not be integrated by SIN. I was stunned to read this, crushed that he was able to find flaws in my system so quickly. After a few days of depression, however, I decided to find out what SIN would actually do in these cases. To my surprise SIN returned the integrals in less than a second. Each case was solved in the first stage. I had allowed the authority of Slagle's letter to cloud my analytic judgment. I wrote him another letter, carefully apologizing for not describing the first stage well enough and enclosing a copy of the computer output. I have not heard from him since. He became a well-known AI researcher at the Office of Naval Research.

While I was concentrating on integration, the bulk of the funding in the AI Group was in robotics. The three major AI centers, MIT, Stanford and, Carnegie Mellon, all received significant funding from the Department of Defense to create robots that could see and manipulate. Minsky, deciding that the vision problem could not be so difficult, assigned a sophomore, Gerry Sussman, and a few of his friends the task of creating a vision system over the summer of 1966. The project was called the Summer Vision Project. Gerry was a former student in New York City's special program for math and science students that took place in Columbia. I was shocked that vision was taken so lightly. This added ammunition to my view that the AI community tended to ignore structural knowledge. Not surprisingly, the Summer Vision Project did not meet its goals.

Earlier I noted that one of Jean Sammet's major achievements was getting together the community of people interested in symbolic mathematics. She did this largely by heading a 1966 conference on symbolic and algebraic manipulation. I submitted a paper on a program for solving systems of polynomial equations. Now I do not recall why I did the work, since it is not closely related to integration; it may have been a residue of my summer working for Jean on Formac.

Finding exact solutions to linear equations with integer coefficients is not an easy task: a key problem is the growth in the size of the integer or rational number coefficients in the exact solution. The problem in the case of polynomial equations is much more complex. If the system is often reduced to one with a single variable by a process of eliminating the other variables, the degree of that final polynomial will grow geometrically with the number of equations and the degrees of the variables in them. It is rare that such polynomials can be solved in closed form. Even then, backing up such solutions will result in equations with coefficients that are much more complex than integers. I used the resultant algorithm to eliminate a variable from a pair of equations. The algorithm came from the text by van der Waerden, a key text in college algebra in the first half of the twentieth century that Engelman's Mathlab system relied on as well. When the final equation could be solved in closed form, I tried to back up the solutions and thus obtain a full set of solutions. If I could not do so, I used a numerical root finder and backed up those solutions. My approach had several weaknesses. In particular, the final polynomial may have been of far too high a degree than absolutely necessary, and thus introduced spurious roots. Nevertheless, the algorithm was better than others that had been proposed up to then. My paper was accepted by the conference, and was even accepted to be published in a special ACM journal issue based on the conference.

At the conference I had a chance to meet the other researchers in the field, including Anthony Hearn, a physicist then at Stanford, who had developed the Reduce system to solve theoretical problems in high-energy physics called Feynman diagrams after Richard Feynman, the great theoretical physicist. Similar work on symbolic systems and their applications in physics resulted in a Nobel Prize by a pair of Dutchmen decades later. I met Stan Brown of Bell Laboratories, who developed the ALPAK system for solving problems involving polynomials and rational functions, problems that often came up in the theoretical investigations done at Bell Labs. Jim Griesmer and Dick Jenks led a project at IBM Research that competed with Formac, which was also an IBM system. Most of us recognized that Formac was not an outstanding system, and apparently others at IBM recognized this as well. A key person at the conference was George Collins, who obtained a Ph.D. in mathematics at Cornell. Collins worked at IBM Research at the time. Part of his work was on quantifier elimination methods, which can be used to determine whether systems of polynomial equation with integer coefficients have a solution where all the variables are real numbers. It was said by Jim Griesmer that he spoke so little during the day that the only thing his co-workers, such as Griesmer, recall him saying was when he gave his order at lunch. On the other hand, George was a powerful theorist. His paper analyzing the resultant algorithm was quite deep, and in many ways set the tone for theoretical work in the field. I spoke to Bob Caviness of CMU at the meeting and he told me that his roommate, who had been asked to work on integration, had died some time after John Donovan visited CMU. Caviness could not have known what this meant to me, or of the sequence of emotions I experienced as he told his story: surprise, anxiety, relief, and then guilt because I was relieved that the other man’s death meant that my work was likely to be without competition.

Now reasonably secure that I would be awarded the Ph.D. on the basis of the work I had done, I spent the fall of 1966 writing the dissertation and finishing related research, which eventually wound up in appendices, one of which contained a proof that the problem of definite integration was unsolvable. Definite integrals are harder than indefinite ones since the technique of differentiation cannot be used in such problems to work backward from a proposed answer. Another appendix described a program, ITALU, which performed integration by looking the integrand up in a table. The technique I used for a rapid look-up was a numerical coding. I had not seen a follow-up study of this approach for thirty years, until my former student, Richard Fateman, created such a table look-up for the Internet.

The key chapter in the draft dissertation was Chapter 2, which contained an argument against heuristic search and for AI systems with expertise. I realized that this was an attack on Minsky's own position, but I figured that if I gave him the draft early on he could argue with me about it, and I would have plenty of time to modify it. I was sure my arguments for my position were powerful, if not unassailable.

My friends on the faculty, such as Joseph Weizenbaum and Marvin himself, were apparently giving my name out to prospective employers. I had not thought much about what I would do next, but I was willing to go on tour and make presentations on my symbolic integrator, SIN, and the AI philosophy behind it. In particular I agreed to talk at Bell Labs, Carnegie Mellon, and Stanford, three of the top computer science research institutions. My colleague, Bill Martin, had finished his dissertation on the Mathematical Assistant a few months earlier, and he accompanied me to Bell Labs.

I spoke at Carnegie Mellon in February of 1967. To say that it was a memorable event is to put it mildly. Earlier in the day I talked to Perlis, the department chair, and to Newell. Perlis was one of the great characters of Computer Science. He built the CMU CS Department into one of the top departments in the country.

On a less professional level, I note that Perlis was bald and even had no eyebrows. He was also a keen student of Chinese food. In fact his wife used to complain that visitors to Pittsburgh always expected him to take them on a round of the best Chinese restaurants. I am going to digress once again from my own story to tell one I heard about Professor Perlis:

During a mathematics convention in Missoula, Montana, one of the mathematicians went to a Chinese restaurant and asked the waiter for Fu Ye, a type of fermented food. The waiter appeared not to hear the order. The mathematician repeated his request, and this time the waiter said that they did not have any such dish. The mathematician said, “I know you have it. Please send the owner, and I’ll speak to him.” The owner came and the guest repeated his request. The owner said, “Do you know that this food is very stinky?” “I do. I have had it before, and please bring me some now.” The owner said, “This is very strange. You are only the second white man to order Fu Ye. First one, he was very bald. He had no eyebrows even.”

My lecture began at 4:00, lasted about 45 minutes, and then I asked for questions. I had described SIN as an example of a new approach to AI that concentrated on creating systems with enough knowledge that they could demonstrate expertise, and noted that Newell and Simon’s work simulated novices instead of experts. Newell began the question period, and he and I dueled for the next 45 minutes. Newell asked me questions like, “What would you do if you were lost in a forest and wanted to get out?” I did not know the usual heuristics, such as “find hills and climb to the top until you can see a way out.” My answer eventually was, “If you force me to search trees, then I will have to search trees, but if I know a better, more structured approach to solving the problem, I will surely want to use it.” Eventually Perlis stopped the questioning by Newell, and I received a nice round of applause for endurance if nothing else. As we walked away from the conference room, Perlis made me a verbal offer; I can’t recall my response at that time.

Bell Labs was not interested in AI. They cared about the symbolic mathematics underlying both my dissertation and Martin's. My host was Stan Brown, who had built the ALPAK system for manipulating polynomials and rational functions. The people I met were largely theorists, interested in communications and numerical techniques as well as software. Martin and I gave a talk that emphasized both the practical utility of our systems and the mathematics behind it. The lecture hall was quite large, but we were not given any microphones. The hall was so carefully designed that the speaker could be heard even if he were whispering. I was very impressed by these acoustics; I was also impressed by the staff members whom I met, such as Joe Traub who later replaced Perlis at CMU, and Elwyn Berlekamp, a fellow MIT Ph.D. Elwyn told me a story about his masters thesis. Believing that his supervisor would not read the final version very carefully, he wrote on page 36a that he would give $5.12 to the one who got to this page first. As expected, the supervisor did not claim the prize, but later readers won $5.12, then $2.56, etc.

The last talk in this series was at Stanford, where the computer science department had been established later than those at MIT and CMU (MIT's CS faculty were largely in the EE Department at the time). George Forsythe, its founder, was department chairman. It was about to become very successful, in large part because Don Knuth, who had begun writing a seven-volume series of books on computer algorithms, would arrive the next year. I recall that Forsythe called Minsky for help with the Stanford administration who were complaining about giving Knuth a full professorship at age 27. Marvin told him that Stanford would have been even better off if Knuth had accomplished as much and were just 17 years old. The books by Knuth set the tone for theoretical algorithmic research for the whole field of computer science. My host at Stanford was Anthony Hearn, who had developed the REDUCE system for algebraic formula manipulation and had an appointment in the Stanford Linear Accelerator Center (SLAC). Stanford was prepared to make me an offer jointly between the CS Department and SLAC. My talk concentrated on algorithms for the integration of rational functions. It was the least successful of the three talks. I got lost in the middle of describing the algorithm for integrating rational functions, and I had to ask Hearn to help me with it.

I spent a little time on a mini-vacation in San Francisco. San Francisco was well ahead of Boston in terms of the sexual and social revolutions that were the hallmark of the late 1960s. North Beach was in full swing at the time, as was Haight-Ashbury.

The MIT AI Group was having one of its peak moments at about the same time. Much of the systems development for the robot project was done by MIT dropouts and recent graduates who called themselves hackers. The best hackers were involved in the MIT Tech Model Railroad Club (TMRC). TMRC gave them the opportunity to play with electronics for controlling the railroad system as well as with software. The best known of the hackers of that generation was Richard Greenblatt. Rich developed a beautiful display of a chess-board over a weekend in November of 1966. The success of this endeavor led him to develop a program that actually played chess. Chess programs had been under development since the 1950s, but Greenblatt's program was better than any of those, after just a couple of months of effort. MIT had at the time a Professor of Philosophy named Hubert Dreyfus. Dreyfus had written polemics against AI, which led Minsky and Papert to write polemics against him. Dreyfus claimed that computers based on AI technology of tree search and the like could not play chess. A match was arranged for him with the Greenblatt chess program—named MacHack VI—in February of 1967. Dreyfus was not a good chess player, but the program was not yet well developed, either. Hubert's brother, Stuart, was a well-known researcher in applied mathematics. He gave Hubert the advice that he should get the program into the "end game," the part of the game where there are relatively few major pieces left on the board. Stuart claimed that the program would do poorly in the end game, and that Hubert could then beat it. This was sound advice. Unfortunately, the computer forced Hubert's king out in the center of the board, and he lost the game well before reaching the end game. We were very happy at this outcome, which reminded me of the subway sign of the times: “God is dead [signed] Nietzsche”; “Nietzsche is dead [signed] God.” We could now say: “Computers cannot play chess [signed] Dreyfus”; “Dreyfus cannot play chess [signed] MacHack VI.”

Greenblatt’s program began to play against humans in chess tournaments, and won some low-level prizes. Its ratings improved as Greenblatt added more and more chess knowledge into the system. I believe it peaked at a level that was approximately that of an average human chess player in a tournament. Needless to say, that level is far higher than the average chess player; unfortunately, it is also far lower than that of a chess grandmaster. I used to play the program from my home whenever I had difficulty sleeping at night. I must have lost several hundred times to MacHack; I may even have held the record during the 1970s for the most losses to a chess-playing computer.

My decision regarding a faculty position was made easy when I was approached one day in the eighth-floor corridor by Minsky and Bob Fano, director of Project MAC. Fano said, “I hope you will stay here, as an Assistant Professor.” I said, “Yes, of course.” And that was that; I could tell the other institutions that I would stay at MIT. CMU tried the hardest to get me, after all. Both Perlis and Newell wrote asking me to break the umbilical cord to MIT and come to CMU, which was certainly gratifying, but I had made my decision.

The rest of the academic year was spent writing the remaining chapters of my dissertation, and giving them to each of my committee members, Minsky, Papert, and Weizenbaum. I received relatively few comments from them. Toward the end of the academic year I was invited to meet with Louis Smullin, Head of the EE Department. I went into his office and for a while he looked at me and said nothing. Eventually I said “You called,” and the interview finally began. Louis was an electrical engineer, and computer science was an upstart branch of the department at the time. I may have been his first appointment of a computer scientist, and he was clearly not fully comfortable with such folks. He was a wise man, and he relied on people such as Bob Fano, who had made the transition from EE to CS, to advise him on such appointments. Following this meeting I received a formal written offer. I was to become an Assistant Professor on September 1, 1967, thirteen years to the day from our arrival in the United States. My salary for nine months was to be $8500, and I could earn two ninths extra by working on research for two additional months in the summer. There was no guarantee of support for the summer months, but it was informally understood that it was all but certain that the funds would be found. This was the same statement I would make verbally to candidates for assistant professor when I eventually became head of the department.

Louis Smullin was born in the Detroit area, and did his undergraduate work at Michigan. He came to MIT in the fall term of 1938 as a graduate student in the EE Department. Claude Shannon, the father of communication theory, had come from the University of Michigan a year earlier, and Louis was curious about what Shannon could tell him about MIT. He invited him to a pub in Boston's Back Bay one evening, and in those cozy surroundings they became lost in conversation. They emerged the following morning to discover that they had completely missed the great hurricane of 1938.

Shannon's Masters thesis in 1938 provided the mathematical foundations of modern digital circuits. His 1948 work on the foundations of the theory of information led to our ability to communicate with computers, for example through modems. He did this work at Bell Labs, but returned to MIT in the 1950s. Shannon also wrote one of the early papers on game-playing in an AI context. His basement in Winchester had several mechanical juggling machines that Claude had built by himself.

The University of Michigan gave MIT yet another star of that time period, Jerome Wiesner. Smullin recalls that he recommended Wiesner during the Second World War, when engineers were difficult to come by. Wiesner was hired by the Radiation Laboratory, where radar was developed during the war, and quickly rose to become one of its leaders. He later became President Kennedy's Science Advisor and MIT's dean of science, provost and president.

My parents invited me to join them and my brother Abe on a summer vacation at the Concord Hotel in New York State's "Borscht Belt." I enjoyed staying at the Concord because it offered plenty of good kosher food and many activities, as well as nightly shows. This time, something seemed different about my brother, who made a pest of himself all the time we were together. My parents had moved to Flushing, Queens in part to allow Abe to attend a better high school than he would have in Brooklyn, and he had just finished his freshman year with a 97 average, surely one of the highest, if not the highest, in the school. During the vacation he kept on telling me that he would do better in school than I did. I had not realized how much my experiences had influenced Abe. He also bragged that he would memorize the Encyclopedia Britannica by the end of the summer. I did not know that this was a warning sign of serious problems to come.
A few weeks later Abe called me in the middle of the night to tell me that he had been having a severe headache for several days. The headache simply would not go away, and the medication the doctor had prescribed did not help. My parents called Mr. Halpern for advice and he suggested that Abe see a psychiatrist he knew whose name was Dr. Moses. Abe went to see him and a few days later the headaches stopped. My parents thought it would be wise for me to take Abe on a trip to the World's Fair in Montreal. Abe was on medication and he was easier to get along with on this trip. I enjoyed Montreal and have visited it several times since. The most interesting part of the trip occurred when we went to the Israeli exhibit. One of the guides was a girl whom I had dated when she lived across the hall from David Smith and me. We went to the Israeli exhibit several times that week.

Abe's long-lasting headache was an early sign of a great tragedy—an illness ultimately diagnosed as schizophrenia. It not only devastated him eventually, but also caused great harm to my parents, who continued to take care of him. He was hospitalized nearly a year later, which began a long, hopeless series of releases and readmissions. The new antipsychotic drugs introduced in the 1950s had given the states the false hope that all mental hospital patients could be released to the community, and this change in mental hospital policy meant that my parents' lives were sadly disrupted and their finances depleted in their efforts to take care of Abe. My brother would improve during his hospital stays, but he refused to take his medication when he was outside the hospital setting; when he was readmitted to a hospital, the new patients'-rights movement allowed him to leave again after a relatively short period. My parents took him back each time, and he made their lives very difficult and spent all their available funds by the late 1980s, when it became necessary to rescue my mother from him.

My doctoral defense was scheduled for Monday, September 1, 1967, shortly after my return from Montreal. I needed to pass the defense that day in order to take up the position of Assistant Professor. Another graduate student, Carl Hewitt, asked me what reaction I expected to Chapter 2, in which I discussed the AI philosophy. I was not concerned about Chapter 2 since I had handed the chapter out eight months earlier, and I figured that if there was to be a major negative reaction to it I would have heard by now. Carl had pursued a thesis that was quite close to the general problem-solving approach that I attacked in Chapter 2.

The exam was to be held in Professor Weizenbaum's office. Normally the exam is given in the presence of the three examiners only, so I was surprised when Marvin's wife, Gloria, showed up carrying her doctor's black bag. In addition, Cynthia, Marvin's former secretary and Seymour's assistant at the time, was also there. Minsky also dragged in Pat Fischer, who was visiting MIT that day, and Robert Fenichel, who had been appointed an assistant professor the previous year. The eight of us could barely squeeze into Weizenbaum's office.

The exam began when Minsky asked me to describe my thesis. A minute or two into this description Minsky blurted, "But that's not AI." Weizenbaum and I were confused, and Joe said to Minsky, "Marvin, what are you talking about?" I was allowed to continue after this interruption, and a couple of minutes later Minsky said once again, "But that's not AI." Weizenbaum was getting more frantic by the minute, and the examination was halted shortly thereafter. All the guests and I were asked to leave, and a heated discussion appeared to have ensued among the three examiners. Approximately fifteen minutes later Marvin asked me to come in. He then told me that Chapter 2 had to be rewritten so that it was closer to current views of AI. I agreed to do so, and Minsky handed me a signed slip of paper saying that I passed the examination.

The defense was the easy part. I still had to convince the Mathematics Department to let me get on the doctoral degree list for that month. Normally this required handing in a bound version of one's dissertation; I did not have a fully typed version, let alone a bound one. Nor did I have a final version since I needed to rewrite Chapter 2. What I did have was a signed first page and a partially typed and partially hand-written version of the dissertation. The professor in charge of graduate students at that time was Norman Levinson. I had known of his famous text on differential equations while I was at Columbia. I don't think that Levinson enjoyed the position I put him in at all, but he agreed to accept what I had with the understanding that I would present a bound copy later on in the fall. I then walked over the papers to the EE Department and began my appointment as an assistant professor.

Levinson had a great career at MIT. He violated the supposed rule that mathematicians do their best work when they are young, often by the time they are twenty-five. Learning in his sixties that he had cancer, Levinson then worked on the Riemann hypothesis, and made great advances on this, the most important unsolved problem in mathematics. The story of how he came to MIT indicates the great changes in American higher education that had taken place, largely as a result of the Second World War.

Levinson applied for a faculty position at MIT in 1936. Vannevar Bush was then Dean of Engineering and Vice President of the Institute. Noting that MIT already had a Jew in the Mathematics Department, namely Norbert Wiener, he remarked, "We do not need another Jew in the Math Department." Levinson had worked for a while with G.H. Hardy. When Hardy heard this story, he said that he would set Bush straight, and that MIT would hire Levinson. People asked how he was going to accomplish this, and he pointed out that he was invited to Harvard's

Tercentenary to give a plenary talk, and that Dr. Bush was going to give him a tour of MIT. At the appointed time, Bush led Hardy on a tour of MIT. At every opportunity Hardy called MIT the Massachusetts Institute of Theology. Finally Bush burst out, “Sir, we are the Massachusetts Institute of *Technology*.” Hardy replied, “Perhaps, but how do you explain the reason for not hiring Dr. Levinson?” Levinson was hired soon thereafter.

MIT changed its hiring practices during the war, although Harvard took more time to get over its anti-Semitism. As a result, MIT gained a number of very great faculty members, such as Minsky, Noam Chomsky in linguistics, and Paul Samuelson in economics. These men created outstanding new departments and sections of departments for the Institute.

I found out the issues underlying Minsky’s behavior in the exam a few months later. Cynthia told me what had happened. As I should have realized, Minsky read the dissertation at the very last minute, and blew up at Chapter 2. Cynthia said that on Sunday, the day before my defense, Seymour and Marvin had a violent argument in which Seymour defended my position in Chapter 2. Gloria Minsky was at the examination just in case someone needed medical help; with Gloria presumably supporting Marvin, Seymour decided to invite Cynthia as well. Marvin invited the other two guests so that everyone, himself included, would remain calm. Joe Weizenbaum was unaware of all this, as were the two guests and I.

Chapter 6. 1967–1970

My student years had been so intense, the work so challenging and exciting, that I am not sure I gave much serious thought to life after the Ph.D. I knew I would continue my research, and now as an assistant professor at MIT I was fairly well assured of where this would take place for the next few years at least; what I could not envision, in 1967 at the age of 25, was that by the time I was 36 I would be not only a full professor and an academic administrator, had made significant technical contributions, but also a husband and a father.

As the fall term began, I worked hard to rewrite Chapter 2 while carrying a heavy teaching load. I showed my new version to Papert, who felt that I had given in too much to Minsky. I was tired of arguing, so I submitted that version and Minsky finally approved it. The final version was bound by Project MAC as a printed report, and several hundred copies were sent around the country. I gave one copy to the Mathematics Department in lieu of a bound copy of the formal dissertation. Once again they were not pleased, but they took the copy I handed them to be placed in MIT's library.

In the thirty-plus years since then I have had plenty of time to think about the ideas underlying the thesis. The computer-science implications of the work were well understood quite quickly; the AI implications took rather longer. The philosophical foundation for the knowledge-based systems approach in AI that I created emphasized the role of knowledge as opposed to search—not any particular kind of knowledge, certainly not "common-sense" knowledge, but *structured* knowledge. This was not fully understood since the revision of Chapter 2 did not fully explain the AI implications of the work. I did not realize that the importance of structure would not be nearly so well accepted as the importance of knowledge, however structured; to me they were one and the same thing. I have spent much of the past decades trying to understand why Americans in particular have difficulty with the issue of structure. I believe it has a great deal to do with America's profound belief in individualism and competition. Thus structures that are closely related to a sense of community and cooperation are not well understood. I cannot say that I have fully understood the problem, but I continue to work on it, as will be clear in the coming chapters.

> Minsky and I basically stopped interacting with each other after the doctoral exam. He has blamed me, and to a degree correctly so, for the Knowledge-Based Systems (KBS) movement in AI, which has not led to great progress on the fundamental issues in the field. He argues that knowledge-based systems have dealt with relatively easy problems of highly structured but narrow domains, such as calculus and chess. Minsky is currently espousing the need for AI programs to have common-sense knowledge in broad areas, although without the emphasis on logic that McCarthy uses. I basically stopped working in AI for some decades after the late 1960's. My current views on AI will be discussed in a later chapter.

Newell wrote a long paper in 1968 in which he created a straw-man view of my approach. He imagined an approach to AI called the Big Switch. When one is presented a problem, one determines which of many specialized experts can solve it (the big switch) and calls on the relevant one. He then gives several arguments as to why this is not the appropriate approach to AI. Edward Feigenbaum of Stanford, a former student of Herb Simon, visited MIT in early 1968 and congratulated me on my thesis, especially the highly revised Chapter 2. He was leading a project at Stanford, called Dendral, which analyzed chemical molecules. Although Dendral used a great deal of search, the key to the success of the project was the incorporation of specialized knowledge about the group structure of molecules. This was more than heuristic knowledge. I told Ed that if he were not careful he would fall into the trap that Newell was setting up for KBS systems, that there are just big switches among experts, rather than a new architecture for incorporating structured knowledge into complex systems. As events unfolded over the coming decade it became clear that Ed did not quite appreciate the distinction I was drawing. I should mention that it was Randy

Davis in Ed's group in the early 1970s who coined the term Knowledge-Based Systems for Ed's and my approaches to AI.

The normal teaching load in the EE Department was one course each term. I am not sure I was aware of this, but the Executive Officer of the Department, Professor James Bruce, got me to agree to teach two courses in the fall of 1967. The first course was an introductory programming course given largely to freshmen, of whom there were some sixty students in my two sections. The second course was McCarthy's original graduate course, which I had taken four years earlier, also with about sixty students. I asked Bill Martin, who was a research staff member at the time, to help me teach part of it.

> In the undergraduate course I taught Fortran to two eager classes, telling them jokes and getting a good response from them, a most enjoyable teaching experience. The mid-term student evaluations of my teaching, required of all freshman classes, were strongly positive.

The graduate course was more complex and required much more preparation. I started with an introduction to LISP. Martin then gave a section on parsing of grammars. This was quite difficult, and his homework assignment took many hours. The student who did best on it, Terry Winograd, would later write a famous doctoral dissertation on natural-language understanding by computer, which required a deep understanding of parsing algorithms. I followed this section up with a section on algebraic manipulation algorithms, such as differentiation and simplification.

> In the graduate course I once made an ill-considered comment about the course taught by John Donovan, who was notorious for starting off his undergraduate software-engineering course by telling the students that if they worked hard and obtained an A in his course he could guarantee that some company would offer them a summer job. Summer jobs were not as easy to find for MIT sophomores then as they were later to be. A number of the junior faculty felt that John should not be advertising his course in that manner. As a consequence of my comment, Professor Smullin asked to come and see me

in my office. He told me that a graduate student of Donovan's had complained about the comment I had made, and he asked about my teaching. I admitted to having made the comment and apologized. I also showed him the freshmen's mid-term evaluations, and he left in a somewhat better mood than he had arrived in.

A number of graduate students wanted me to supervise their Master's theses. I accepted five of them that year, a larger number of new students than I would accept thereafter in any given year. Moreover, they all finished within a calendar year, which is faster than any of my subsequent students. In fact one of my later students took five years to complete his Master's thesis! The length of the Master's programs would be an issue for me when I became head of the EECS Department, and was rectified only when the Master of Engineering degree was created during Paul Penfield's administration in the early 1990s. Although nominally there were five student theses, actually I supervised only three masters projects, since two of them were done with a pair of students each. One pair worked on extending Tom Evans's thesis on analogies. This required them to understand how the original system described in the doctoral thesis worked, to identify new cases of analogies, and to extend the system to incorporate such new types of analogies. A second project involving two students was to recognize Morse code as sent by humans. This had been done before, and it is most likely that the National Security Agency had done classified work in this area, but I wanted to tackle it again. The students were moderately successful in using statistical pattern-recognition techniques. Later on, Albert Vezza, a group leader in Project MAC, obtained funding for an extension of this work. His research was sponsored by ARPA, but I assume that NSA had indicated to ARPA an interest in the work. I do not recall the exact accuracy level of these programs, but I believe that they were fairly accurate in translating Morse Code generated by humans into English. The third Master's project was an extension of Dan Bobrow's work on algebra word problems, and tackled word problems in calculus. The student, Gene Charniak, used more powerful techniques than Bobrow, in particular he had access to a better parser of English grammar. The program did quite well, but Gene pointed out many semantic difficulties that he could not solve in only a year's work. I was quite pleased with the effort, and hoped that we could extend the work in the coming years. Charniak was the only one of the five students who continued as a doctoral student. He is now a well-known AI faculty member at Brown University.

In the spring of 1967–1968 I taught the standard course load, this time two sections of the introductory programming course. I guess Jim Bruce had heard of my unhappiness at being assigned double the normal load, and from that point on I taught no more than one subject per term. These students were not as excited by the material as those in the fall, which taught me that it takes only one term for MIT to remove the excitement that freshmen arrive with. I do not believe that we have ever learned how to undo this property of the Institute, but I am pleased to say that the MIT faculty has not stopped trying to improve the freshman year.

One evening while I was working at Project MAC, two people approached me and wanted me to help them settle a bet. One was Carl Engelman, developer of the Mathlab system. The other was Bill Henneman, an older graduate student working for Seymour Papert. Henneman was a very interesting fellow. He claimed that his IQ was over 200, and when he was in school many psychologists wanted to test him further. He stopped doing well in school, so they would stop pestering him; in fact, he dropped out of high school just before graduation, and ran away from home. He took courses on and off during the succeeding years, earned no degrees, and finally wound up doing a theoretical doctoral thesis under Seymour Papert.

The bet between Carl and Bill was whether I could name six different oyster dishes. Henneman knew that I was kosher, and thus would not eat oysters, but he felt that I was so broadly knowledgeable that I would at least have glanced at the rest of the menu. Engelman felt otherwise. Carl was right, I did not know any oyster dishes, not one, but I wanted to help Bill. So I used a heuristic—suppose that oyster dishes were like chicken dishes. Thus I came up with ideas such as Southern Fried Oysters, boiled oysters, and the like. Henneman paid off on the spot**.**

In the summer of 1968 I had my first opportunity to travel abroad, exciting for any young man but also part of my professional development. I made the most of it, traveling not only to a conference in Berlin, but also to a second one in Edinburgh, with brief stays in London and Paris. Perhaps in these days that is as close as one can get to the Wanderjahr of earlier times.

> Project MAC had funded my work for two summer months, as I had hoped, and had also enabled me to go to Berlin for two weeks as part of a group that would demonstrate our work. The Ford Foundation paid for the trip, with the goal of helping West Germany improve its computing capability. Our dean, Gordon Brown, had good connections to the Foundation, which had helped him in the 1960s to transform the School of Engineering and give it its present engineering-science orientation. Moreover, MIT used Ford Foundation funds to teach engineering faculty who later populated engineering schools around the world about this new approach to engineering education. In addition, the Foundation help fund MIT faculty in the 1960's to teach at the Indian Institute of Technology at Kanpur, which has helped build up India's ability to compete with us in recent years. This project led to a famous telegram by Gordon Brown. He wanted to let the Kanpur people know who was coming to teach, namely Paul Gray and David White, and that Professor Green could not make it. The telegram read as follows:
>
> Green can't come.
>
> Gray and White on the way.
>
> Brown

When I became dean of engineering in the 1990s I found myself trying to undo some of Gordon's work, largely because it had been perhaps too successful in turning engineering away from issues such as design, manufacturing, and the environment. I am not sure that Gordon meant to exclude these aspects of engineering, but one cannot fully control every movement one starts.

Our host institution was the Technical University of Berlin. In 1968, the TUB was not yet run by its students, but it was quite close to it. The lecture hall had a large banner behind the podium, saying in German, "Mao is our guiding light." The students met just before we began to speak and issued a declaration stating that the MIT professors are not responsible for their government's Vietnam policy, so that the TUB students would let them give their lectures. It did not surprise us to hear later that the students took over the running of the TUB sometime after we left for a period of several months.

The lectures were to be translated into German. They told us that when the light on the podium was on, we should slow down so that the translator could keep up with us. My lecture was on my work in integration, and the light was on nearly all the time—I guess I had not been able to outgrow my Brooklyn heritage of fast speaking. Just before my talk the senior TUB professor took me aside and asked me not to say anything negative about the computing situation at the TUB. I do not recall a similar request being made on any other occasion in the following decades.

Of course, as with most conferences, the side trips were at least as interesting as the meetings. Professor Frederic Hennie, our leading theoretical computer scientist at the time, and I visited the art museum in East Berlin. One entered East Berlin by taking the train on the West Berlin side. West Berliners were not allowed into East Berlin. Fred and I, with our American passports, had no difficulty. The museum tour was conducted by a nice young man who spoke in English, while I teased him with German questions. For example, I asked where the Soviets stored the art that they took after the war and never returned. Fred became increasingly nervous as I kept pestering the guide, and finally asked me to stop asking questions.

On another day I visited East Berlin alone, this time on behalf of my father. Recall that my father and his father had owned two apartment houses in East Berlin, each sold at far below its value in 1939, when Jews were no longer permitted to own real estate. He asked me to find out if they were still standing, and whether the East Germans acknowledged his ownership of them. He had been told in Israel that one of the buildings was still standing and that some of the tenants remembered that a Herr Moses had owned them before the war. I had the address of an office in East Berlin where I could ask such questions. The train stopped near the office, and I walked the rest of the way, a Kafkaesque experience, in the middle of the day among tall buildings, with no one else to be seen. I could hear each of my footsteps echoing in the street; I sensed people watching me from behind their window curtains. When I arrived at the office, they told me that East Germany was not paying reparations (which I knew, of course) and they refused to answer any of my questions. It is unfortunate that I have forgotten the details about the two buildings, because the united German government is now willing to pay some reparations, but neither my mother nor I remember their addresses.

One night Joe Weizenbaum, Bob Fenichel, and I went to a bar with some of the students. The students were pleasant, but took extreme left-wing positions. I felt that German students were most comfortable with extreme positions, on both the Right and the Left. One of the students was a young woman, and we had a good time talking in English and German that night.

The MIT faculty went out of its way to demonstrate the power of time-sharing. We managed to get access to time on a satellite so that we could log into Project MAC and show off the system in real time. This was a significant undertaking in 1968, and very impressive to our hosts.

The Berlin trip was scheduled to take place just before the international computer science meeting in Edinburgh, Scotland, a city that has preserved much of its picturesque ancient monuments while keeping abreast of the times.

Tony Hearn, guide-book in hand, led us to several of the best eating places, and we much enjoyed wandering the old streets, Edinburgh Castle glowing with lights above us.

In Edinburgh I met Allen Tritter, whose reputation as the biggest man in computer science stemmed not only from his expertise, but also from his size. I had heard of him before, since he worked at MIT at one time, when he had the reputation of tying up all the phone lines between Lincoln Lab and the campus, and then leaving the phone off the hook and going home, which disabled the phone connection between the Lab and the campus. Tritter was also said to have been hired by Bell Labs to help them find flaws in the underwater telephone cable between the United States and Europe, hiring him mostly because he was able to make free overseas calls. Tritter described himself as weighing thirty stone—four hundred and twenty pounds. At that time I was not exactly slim, but I still weighed one hundred and sixty pounds less than Tritter. Once when we got into one of the huge British cabs he took up the entire back seat, and I was relegated to a little jump-seat in the middle. There were no other passengers in the cab—there wasn't room for any. During one panel discussion at the conference, with the hall full and no place that would accommodate his bulk, he carried up a chair and placed it on the podium, albeit at a respectable distance from the panelists. Since meeting Tritter I have sometimes asked to be introduced as the second-biggest man in the field. Tritter was at that time working on his doctoral dissertation at Oxford. He was testing a mathematical conjecture called Goldbach's conjecture, using the Deuce computer built by Alan Turing around 1950. Deuce was supposed to be decommissioned about a year later, and Tritter had determined that he would not be able to finish his calculation by then. I can't remember the strategy he used to get around this obstacle, but he did eventually get his Ph.D. He later worked for IBM Research Laboratory on an extension to the APL language, and for the Hebrew University for a while. I have heard that he had an operation to remove a tumor weighing many pounds, which supposedly accounted for some of his great weight. He claimed that doctors would be able to use his tumor for research purposes for many years. Sadly, he died some years later as a relatively young man.

> My only contribution to the Edinburgh conference was to correct a rumor about Minsky that surfaced during one of the panels. Minsky supposedly had promised his sponsors that he would build a robot that would beat the Chinese in ping-pong, in which they were world champions. A moment's reflection at the time would have revealed that this must have been a joke, but since one or two people seemed to take it seriously, I got up and pointed out that he had not actually promised to do so. As I recall now, some of the staff did try to play the game with one of the lab's robots, but the system was so poor that one had to throw the ball perfectly straight in a plane so that the vision system could track it. When the system did manage to track the ball it would move a basket underneath it and try to catch it. Far removed from world champion or even novice-level play!

After the Edinburgh meeting I went to Paris by way of London. In London I saw the usual sights, such as the Parliament Building. I recall ordering ice-water at a London restaurant, and the waiter saying, "Ah, American champagne." He and I had a good time discussing eating patterns of Americans and Europeans. In Paris I stayed at a small, inexpensive hotel on the Left Bank. In the morning I was lying on my bed without a stitch of clothing on when a woman knocked and without waiting for an "Entrez" came in to leave a breakfast tray for me. I was shocked; she appeared not to notice.

Paris was enormous fun. I did the usual tourist sights, as in London, although Paris certainly had better shows for a young man, such as the Folies Bergère. I used the two weeks I stayed there to grow a beard. Thus when my colleagues saw me again I would have a fair-sized beard already. One day I went to Les Halles (the central meat and produce market in those days) and ordered onion soup. After fifteen minutes I glared at the waiter, and he said that they were still preparing it. Sure enough, when he delivered the soup I could see why it took so long to make. It took me over an hour to make a serious attempt to finish that soup, and I have never had one quite like it since. I also went to lunch at a one-star restaurant along the Champs Elysées. Lunch was good, and then they brought a box of cigars and I picked a Cuban one. I was still smoking it a couple of hours later. I have never had as good a cigar since, including all the Cuban ones I bought on trips outside the United States.

In 1968–1969, my second year on the faculty, I taught the McCarthy course again, and Bill Martin again helped me by teaching a portion of it. This gave me the opportunity to attend a conference in Milan with Minsky and Papert. The conference, part of the celebration of the Olivetti Company's centennial, was on the topic of language in man and machine. I gave a paper that discussed the structure of algorithmic layers in my integration program. I followed this with a short description of the Master's thesis by Charniak. A subsequent speaker got up and roundly criticized the Charniak work because it did not rely on the modern theory of parsing, originated by our MIT colleague Noam Chomsky. Minsky and Papert urged me to respond to this attack, but I did not do so. Over the years I have tended to agree with many of my critics, perhaps too much so. In this case, I felt that Charniak could be excused for using a heuristic parser, since this was only his Master's thesis, but that the linguist who complained was also right in that we should consider better parsers in the future.

We were assigned rooms in a hotel close to the La Scala Theater and the great Cathedral of Milan, referred to as il Duomo. La Scala was closed for the season, but as I walked around the district and crossed the avenue to enter the cathedral, I realized that the locals ignored the traffic lights and crossed whenever they felt like it, in spite of the constant stream of Fiats and motor-scooters, which never slackened their pace as they zipped along in front of or behind the pedestrian. I wondered how the drivers managed to avoid hitting anybody, and I realized that the drivers gauged the rate at which the pedestrians walked, driving past them accordingly. I mentioned this to Bob Fano when I returned from Europe, and he said that the process is called "the chicken game." Pedestrians are allowed to cross at any time, but they must keep a steady pace and not chicken out in the middle of the road.

For the conference we were given personal chauffeurs, since the Milanese taxi drivers were on strike. There was, of course, a language barrier for those of us who spoke no Italian; this became a problem for me only when we left the hotel, some minutes later than the bus taken by most of the others. Nearly halfway to the airport I realized that I had left a gift in the hotel safe, and by the time I'd made the driver understand what I wanted and we had gone back and retrieved the gift, we were clearly very late. I was sure I would miss the plane; the driver was not worried. When he got onto the highway he simply drove on the wrong side of the road with his hand on the horn the entire time. This being commonplace in Italy, we had no difficulty getting to the airport just as the bus was arriving.

A few days after I returned I was surprised to see, visiting Project MAC, one of the women whom we had met at the TUB. Boston was her last stop on a tour of the United States, and she would soon return to Germany. I asked her out, and we wound up having a very good two weeks together. I made the mistake of telling my parents that I was dating a German woman, and they were not pleased, to say the least. I was prepared to date non-Jewish women, since my experiences with Jewish women at that point had been mixed at best. Mr. Halpern had once arranged for me to take out a graduate student in sociology at Harvard, who came from a family whose name is well known even to non-Jews. After we had been out a few times, she said things would not work out between us since she could not bring me to meet her mother. I was surprised, and asked, "Why I can't I meet your mother?" She said, "Because you're not a real doctor."

A couple of weeks after my German girlfriend left I was invited to a party at the apartment of another Minsky graduate student, Arnold Griffith. Arnold and his girlfriend, Patty, were giving the party for a group of musicians who had just given a concert at Jordan Hall. The musicians were students of Ran Blake, an instructor of jazz music at the New England Conservatory. I went with Carl Hewitt, another graduate student in the AI Group. There I saw a fellow assistant professor, Adolfo Guzman, with his girlfriend, and standing next to her was this great-looking young woman in a brown and white dress. Carl and I made a beeline to talk to her, and learned that her name was Peggy Garvey. After several hours at the party we managed to convince Peggy that it would be a good idea for her to go with us to Project MAC, where we promised to show her some interesting things on a computer. We were both too taken with Peggy, I suppose, to consider how much the situation resembled the old line about showing a girl one's etchings! As it worked out, Hewitt drove his car, and I navigated for Peggy, who drove me to Project MAC in hers. At that point I still did not know how to drive. We went up to the ninth floor and Carl and I showed her the Doctor program that Weizenbaum had written. It was quite famous at that time. The "patient" typed English sentences into the program, and it replied in English. Many people were convinced that since the responses were in English there must be a human supplying them. The technique that Joe used came, he claimed, from Rogerian psychiatrists who parrot what people tell them, and thus get them to talk. In addition, Joe placed stock phrases and sentences in the program. Thus one could have a conversation like this:

Doctor: Hello, I am the doctor. Can I help you?

User: My mother hates me.

Doctor: What makes you think your mother hates you? (parroted sentence)

User: I just think she does.

Doctor: Why do you think she does? (parroted sentence)

User: I just do.

Doctor: Tell me more about your family. (stock sentence)

The Doctor demo seemed to have gone well, but I did not seem to be able to shake Hewitt off. Finally I managed to get a phone number from Peggy and she drove home. The next day I called her and we arranged to go out on Saturday night. We went to a French restaurant in Boston where she ate coq au vin and I had my usual fish dish. We then went to a play, and I realized she was nervous, because she laughed at the wrong places. She drove me home, and then went home herself. I found out that she was 21, five years younger than I, and that she was a third-year student at Northeastern University. She was an Irish-American and had gone to a Catholic high school. One of her sisters was a graduate student at Northeastern University, and she had two other sisters and a brother. I thought the evening out was a great success, and called her again. I was lucky to get a second date since, I found out much later, she felt I had been more than a bit fresh with her on the first one. But once we started dating we never stopped. We saw each other nearly every night during those months, beginning a love affair that has lasted for over thirty five years now.

In the fall of 1968 Bill Martin, Carl Engelman and I began work on a project to combine our three prior efforts to create a mathematical system that would be the best of its kind. Bill led the effort to design the front-end and overall architecture of the new system. Bill, who was very hard-working and tenacious, was a research associate and did not have the same teaching duties that I did. He also loved to model situations as well as people. Although he did not actually create this analogy, he could be viewed as a Belgian horse and me as an Arabian one: he would carry the load on a regular basis, and I would be expected to run very well, albeit only once every few weeks. Eventually I came up with the name of the new system, Macsyma. Macsyma stood for Project MAC's SYmbolic MAnipulator. It also had an obvious Latin meaning, and a Hebrew one related to the Persian word "kismet" and meaning "magical" and "wondrous."

Macsyma was to have improved versions of our prior work as well as the state-of-the-art algorithms that we had learned at the 1966 conference organized by Jean Sammet. Martin designed a new data structure for a rational function system, and we intended to implement algorithms of the type we heard about from the others. Little did we realize then the weaknesses in these algorithms. Design work for Macsyma took nearly a year, and the actual hiring of staff and the beginning of the software development was to take place beginning in July 1, 1969.

My parent's thirtieth anniversary was coming up in January of 1969. I wanted to buy them a nice present, and decided on silver place settings for eight from Tiffany's. My dates with Peggy were going well; I had met her parents and we seemed to like each other; and I had met her older sister, Jane, the graduate student. Jane was interested in philosophy, in particular Ayn Rand, in those days and she and I enjoyed many philosophical arguments, since I was no fan of Rand. I wanted to have Peggy with me in New York, but I was very uneasy about telling my parents about her, since she was not Jewish. I didn't want to upset them about my dating a non-Jewish girl once again. At that time the understanding between Peggy and me was that we would have a good time, but we would not get married because of the differences in our backgrounds.

Peggy's mother had bought her a handsome white coat, and she looked lovely when I picked her up at North Station. We flew to New York and stayed (in separate rooms) at the Sheraton Hotel. I took Peggy to Tiffany's, and we ordered the silver place settings with an embossed "M." I then took a cab alone to meet my parents in Brooklyn, where they had moved on the advice of psychiatrists who felt that if Abe were closer to his former friends he would do better. Abe attended the venerable Erasmus Hall High School. My mother worked very hard to keep him in school, given his mental illness. My parents appreciated the silverware, but my mother and brother were suspicious about hearing that I was dating a Jewish girl named Peggy. After a couple of days in New York, Peggy and I returned to Boston. I later learned that my mother was so suspicious that she called Tiffany's and asked them if they recalled seeing a person with my description who bought silverware. She then asked if this person was alone, and they said no, there was a young woman with him. Thus my mother obtained a description of Peggy, who is blond and decidedly not Jewish-looking.

However, it was not my family, but Peggy's, that caused our first major crisis. Her oldest sister, Mary, married for many years but still living near her parents' home in Melrose, decided that for Peggy to date a Jewish man, staying long hours in his apartment, was unwise. She was convinced that I would never marry Peggy, and one day in March told her not to see me again. I was to meet

Peggy the next afternoon outside Northeastern. When she saw me she burst into tears, and finally told me what Mary had said. Peggy and I decided that I should go to her parents and tell them that we wanted to keep on seeing each other. She disappeared into the kitchen with her mother; I sat down with her father, Frank. I told him that Peggy and I liked each other a lot, and that while we might not get married in the end, we wanted to continue seeing each other now. Her father was not a man of many words. He simply said, "I like you, too, and it's okay by me." Peggy told me that her father had wanted to attend MIT as a young man, but (just like my father) his father needed him in the firm, M.H. Garvey, an export-import firm in Boston. I wonder how this early, thwarted desire influenced his attitude toward me. In any case, Peggy and I now had a green light to continue seeing each other.

This happy development gave both of us the energy and optimism we needed to pursue the other parts of our lives, for Peggy her studies at Northeastern, and for me my research goals and the development of Macsyma. While Bill Martin was concerned with its architecture, I spent my research efforts on key algorithms. In particular, I was concerned with simplification of mathematical expressions in addition to integration. I received a very important paper by a Robert Risch, which was based on his Ph.D. dissertation at the University of California at Berkeley. Risch, who was hired by IBM Research, had greatly generalized Ritt's work on integration, and obtained an algorithm that could decide whether an integral could be expressed in closed form in many important cases. These cases involved rational functions, exponentials, and logarithms, as well as any combination of these, including trigonometric functions. A key to Risch's algorithm was an idea similar to that of the Edge heuristic, the Educated GuEss heuristic that formed the third stage of SIN: the integral, if it is expressible in closed form, has a structure that can be predicted up-front. One can then differentiate this generalized structure and see if it can be solved for the variable coefficients in it. In order to obtain the structure of the integral one needed to represent the integrand in a very careful fashion. For example, all trigonometric functions had to be represented in their complex exponential form. In a sense, the key to integration was simplification, and the key to simplification was representation. If you can properly represent the integrand then the integral has a form which is easy to specify as its anti-derivative. The missing piece in Risch's algorithm was the part that gave so much difficulty in the nineteenth century, namely integrands that contained roots of polynomials or even more complex algebraic functions. It was these functions that led to Hardy's conjecture that integration could not be solved in finite time and space.

I began work to implement Risch's algorithm, and spent much time analyzing its implications for simplification. I began to classify simplification algorithms into three basic approaches. The first approach I called the general simplifier. It was similar to the one in Formac: it knew all the rules regarding 0 and 1; it could add two similar expressions, such as 2 x and 3 x becoming 5 x. Yet it did not do very much to get an expression into a standard canonical form. For example, (x +1) * (x + 2) did not get multiplied out. On the other extreme is a canonical simplifier that represents all equivalent expressions in the same standard form. This is easy to do for polynomials and rational functions, but gets harder when one also permits logarithms and exponentials. Risch's algorithm relied on a simplifier that was intermediate in nature. It guaranteed that one could recognize when expressions are equivalent, thereby avoiding spurious expressions equivalent to 0. It did not guarantee that expressions would always be in the same canonical form, because this was not critical to the integration problem. I implemented such a simplifier as part of the integration package and later as a standalone simplifier for Macsyma.

At this time I was approached by graduate students to supervise their doctoral theses. The first of these was Richard Fateman, who was actually a graduate student at Harvard. I asked Fateman to work on algorithms for simplification. He also helped Martin on writing a new package for manipulating rational functions, replacing Engelman's Mathlab package. I was also approached by Paul Wang, a graduate student in Mathematics. I asked him to work on the integration of functions with definite limits. As noted earlier, definite integration is harder than indefinite integration because one cannot differentiate the result to obtain the integrand. I thus expected Paul to rely on special cases and heuristics. Paul also needed to work on a package for obtaining limits of functions as a key component to indefinite integration.

Work on Macsyma began in earnest in July of 1969. Bill hired staff members, such as Jeff Golden, to help maintain the system as it was evolving. Jeff was a graduate of Brandeis University. He helped maintain the system for the next thirty years! We also hired staff to improve the LISP system. LISP has historically been slow in performing arithmetic, certainly in comparison to Fortran. We could not expect to have a useful system if we had to accommodate such a disparity in speeds. We hired Jon White to work on the LISP system. Macsyma was developed on the DEC PDP-6, rather than the IBM 7094 or the new MULTICS system being developed at Project MAC. The PDP-6 ran the ITS system at that time, and had its own version of LISP, written largely by Greenblatt and his fellow hackers.

Peggy and I would sometimes go out to eat with the hackers. One of their favorite haunts was a middle-eastern restaurant in Boston's South End, the Red Fez. I liked it because of my Israeli background. I recall that Jon White liked to eat raw lamb, called kibby. Not only was this not kosher, but I could not see myself eating raw meat. One night we invited John McCarthy to eat with us around four in the morning. He seemed to enjoy it and the conversation about the history of LISP. There was a bordello on the top floor of the Red Fez, which added an exotic note; we stopped going to the restaurant, however, when we realized that the new hole in the front door was a bullet hole.

The evolving Macsyma system, even in its earliest days, was putting pressure on the PDP-6 used by the AI Group. Although the DEC machine limited us to 256K words of memory, far better than the IBM 7094, we needed more memory than that in order to use the machine in time-sharing mode. Fortunately one of the former hackers, Stuart Nelson, had formed a small company that built a memory extension to the PDP-6. We were its first customers. I recall the day that Bob Fano, the Director of Project MAC, asked me to help him justify the expenditure of about $400,000 for this add-on memory. Up until this point I thought of myself as simply having fun, but with such an expenditure, largely justified for our project, things became serious. As it turns out, this would be only the first major hardware expenditure for our system.

Fano was, like me, an immigrant and had come from Italy about the time of the Second World War. He had studied electrical engineering in Italy. This generally meant the study of electric power generation and distribution. Research at MIT during the war concentrated on issues related to radar, so Fano made a transition to communication theory. He was close on the heels of Shannon in formulating the foundations of information theory. By 1956 most of the leaders in information theory were at MIT with Fano: Peter Elias, Jack Wozencraft, Robert Gallager, and soon Claude Shannon would return from the Bell Laboratories to complete the monopoly. When J.C.R. Licklider at ARPA was looking for a university to do research on timesharing in 1962, Fano wrote the proposal, and thus Project MAC was born with Fano as its director. When Smullin needed an associate head for computer science and engineering a number of years later, Fano moved on to that post.

Now that Peggy's family had approved our seeing each other, I frequently went with her to her friends' parties and get-togethers. Sometimes I had the feeling of being out of my element, since they were based on a set of social customs far removed from my own. Peggy and I attended several parties given by her high-school friend Janet Sergeant, Janet's fiancé Paul, and Paul's family and friends. At one of these, a cook-out, the guys cooked the hamburgers and hot dogs (including kosher ones for me), while the women sat and talked. I had never learned this middle-class American male skill of grilling meat, so at first I stayed with the ladies, which made them uncomfortable. Then I went outside to join the guys, where the conversation was about cars, another area alien to me, and now *I* was uncomfortable.

> Janet's wedding was a grand affair. Peggy was a bridesmaid and in my eyes was more beautiful than the bride. When we came back to my apartment, caught up in the joyous emotions of the day, I asked Peggy to marry me. She accepted my proposal, in spite of the religious differences we had often discussed. She must have been thinking about this a lot, because she also agreed to convert to Judaism. Now the die was cast. I had not expected to marry Peggy throughout much of the previous year, but my attachment to her grew and grew, so that there really did not appear to be another choice. When Papert heard about the engagement he tried to dissuade me from marrying anyone. On the other hand, Bob Fenichel, who was at my thesis exam and was at the time my office-mate, thought Peggy would be good for me. I'm sure no one could have affected my decision at that point since, as they say, love is blind. My hope was that Peggy, with her interests in recreation, would transform me into a more well-rounded person mentally and a less well-rounded one physically.

Meanwhile, work went on. The 1969 fall computer science conference took place in Las Vegas. The algebraic manipulation group that Sammet created held a session on algebraic simplification, including were Brown, Caviness, and Risch. I gave the introductory talk. My talk described my classification of the various approaches to simplification and representation. As I was talking I noticed that George Collins, who was the secretary of the group, started writing feverishly. After I spoke the others said very little, since my introduction had unintentionally covered much of what they might have said. I believe we began planning for the second major conference of the group at that point. It was to take place in 1971, five years after the first conference.

I volunteered to become chairman of the program committee for the 1971 conference. Little did I realize then how much effort that conference would take. Since by now I knew most of the leading workers in the field, I intended to have them present their major accomplishments through invited talks. Given that the talks were invited, I had the opportunity to select the topics that each of these researchers was to write on. I picked topics that in toto would cover most of the key topics in the field. I think I selected about thirty invited papers, a huge number for such a small community. The remaining papers were to be obtained as submissions by the community.

I spent much of my time at MIT in 1970 dealing with the upcoming conference. I spoke on the phone with all the authors whose papers I had invited. I was told that I was on the phone so much that year that the MIT operator told someone who tried to reach me that I was on the phone the whole time, and that I would probably call him before he could manage to get through to me. It would have been great had e-mail been widely available in those days. Macsyma development was going very well, and Martin and I decided that we would submit a set of papers on it to the conference. I invited a paper by Bill on the display of expressions and a couple by me giving an overview of simplification and describing my work on integration. In addition the group submitted four other papers for a grand total of seven.

Bill Martin got married that year. He had a hard time convincing Susan to marry him. She was a summa cum laude graduate of Wellesley College, and had another beau from Germany. Eventually Bill broke through and won her hand. I was invited to the bachelor party attended by several of Bill's MIT fraternity brothers. A number of them founded the Index Corporation, and one, Tom Garrity, later became Dean of the Wharton Business School at the University of Pennsylvania. The wedding took place at the venerable Wayside Inn. It was a beautiful day, the flowers in the garden were lovely, and the food was good; Peggy was with me, and we happily anticipated our own wedding.

J.C.R. Licklider, "Lick" to everyone, replaced Bob Fano, who had been Director of Project MAC from its inception in 1963 to 1968, when he became associate head of the EE Department. Licklider was a great visionary in the computer field. His classic paper on man–machine symbiosis heralded the importance of graphics and other methods of making machines easy to use. Lick also was the first director of ARPA's computer science office, and he mentored the next several directors. This office funded the key research in computer systems, AI, and especially networking in the 1960s. I recall that Larry Roberts, an MIT Ph.D. who ran this office in the late 1960s, promoted the ARPANET, a precursor to today's Internet, largely because he thought it would permit better use of the computer resources by the many ARPA contractors. Frankly, no one realized then what the full impact of networking would be.

Lick had been a professor of psychology at MIT in the 1950s, but went to work at Bolt Beranek and Neuman after it was clear that MIT was not ready to form a psychology department at that time. BBN was founded by former MIT staffers. The original emphasis at BBN was on acoustics, both for military and civilian applications; their military work brought BBN close to ARPA. BBN developed timesharing on a PDP1 at about the same time as MIT, and in the early 1960s had one of the earliest experiments on using a telephone to remotely connect to a computer. BBN also performed research on AI, especially on language and speech. Dan Bobrow and others from MIT worked there, and they created a competing LISP environment to the one we created on campus, called INTERLISP. When ARPA was looking for a company that would develop the key hardware and software for the ARPANET they asked BBN to do so. MIT was connected to the fledgling net in 1969, and thus we were able to be one of the first people to use e-mail across the nation.

While Lick was a great visionary and a wonderful person, management was not his greatest strength. At a faculty meeting one Friday afternoon he told us that he was thinking of rearranging the space for better use. I was not concerned, expecting that we would have more discussions about it in the future. I was quite surprised, and some of the others were clearly shocked, when we came in on Monday and found our offices had been moved and that our keys no longer worked. Minsky used space issues as one of the arguments for splitting the lab some months later.

The student uprisings that had begun in 1968 over the war in Vietnam affected colleges across the United States during the following two to three years, and MIT was no exception. One day I read in the student paper, *The Tech*, that there would be a demonstration at noon in front of the president's office, on the second floor of Building 4, in protest of the Committee on Discipline's decision to expel a student leader. I decided—rather casually, it seems to me now—to spend part of my lunch hour seeing what the demonstration was like. I arrived at the spot and there were already dozens of students in the hall, standing in front of a closed door that gave access through an anteroom to the president's office. Suddenly four masked students got up from the floor where they had been hidden by their friends and attacked the door with a battering ram. On the third blow, the door splintered; the masked students dropped the battering ram and started running down the hall toward the main entrance. Too furious at the violence and destruction to consider what I was doing, I ran after them. Not surprisingly I couldn't catch up with them, although there was a split second where I could have tripped one of them. It's doubtless best that I didn't. They ran up the stairs in Building 7 which serves as the main entrance to MIT and I followed them, but at the top of the stairs I saw more students, apparently there to protect their colleagues. Out of breath, I chose a more prudent course, and returned to the president's office. When I returned students crowded the anteroom and the president's office itself. Soon the Provost, Jerry Wiesner, showed up with a bullhorn, through which he angrily demanded that they leave. The Associate Provost, Walter Rosenblith, tugged at Jerry's shoulder and tried to calm him down. I heard later that Jerry suffered from diabetes and Walter wanted him to take medication to prevent a collapse in the wake of so much emotion. The students wanted to be defended from the campus police who also accompanied Wiesner. Since I was the only faculty member they recognized in the crowd, someone asked me to stand at the entrance to the president's office to protect the students from "the fuzz." I agreed, and the police left me alone. At one o'clock I had a meeting, so I simply left.

President Howard Johnson handled the student uprising with admirable aplomb. He did not set the police on them, and after a few days' "occupation" the students left on their own. I heard later that there were some confidential files in the office that the administration wanted; Wiesner simply walked in past the students, took the files, and walked out again. Johnson's management of the student anti-war protest was in strong contrast to that of Grayson Kirk, president of Columbia, who handled the sit-in in his office by calling the NYPD and was asked to resign as a result. MIT did, however, sue the students responsible for the damage, in Cambridge District Court. I was subpoenaed on behalf of the students and asked to appear at 9:30 AM on a Monday morning for a 10 AM start to the trial. I showed up in time and was told by MIT Vice president Constantine Simonides at 9:45 that the suit had been settled. Apparently the students were placed on probation for a year.

> In retrospect my actions appear quite naïve. I should not have run after the students, and I probably should not have tried to block the door to the campus police and the administration. I was lucky that nothing serious occurred, and MIT was lucky that its president was a cool customer.

Another activity in Technology Square, the Cambridge Project, also had some activities related to Vietnam. The protesters planned a demonstration march from the student center to Project Cambridge. Since Lick was associated with that project he thought the students might also show up at Project MAC, although it was in a different building in Tech Square. He prepared for this by getting a loudspeaker and making small holes in the door of the director's office through which he could be heard in the corridor. He sent his staff assistant, Al Vezza, to the Student Center to see what the students were planning, and to give Lick an early warning. When the students began to march to Technology Square, Vezza ran to warn Lick, but tripped and hurt his ankle. Fortunately the students were only interested in the Cambridge Project and Lick was spared the need to address them.

Peggy and I went to Washington to join a march on the Capitol. The bus ride took all night and when we arrived there were hundreds of thousands of people already there. We heard great rock music and lots of speeches. Bob Fenichel was also there. He was surprised to see us, especially me, since I had not been active in the anti-Vietnam movement. Bob was helping students avoid the draft; the notebook where he kept tips on draft avoidance was entitled “Dodgers vs. Yankees.”

Chapter 7: 1970–1977

In early 1970 Peggy and I went to meet a conservative rabbi in order to get her admitted to the course of study needed for her conversion to Judaism. I did not know the rabbinical attitude toward conversion, so when the rabbi asked Peggy why she wanted to convert, where I would have answered "because I want to marry Joel," Peggy answered that she had been attracted to Judaism since her high school days. This was apparently what the rabbi wanted to hear. The conversion class was run by a retired rabbi from Leominster, Massachusetts, Rabbi Lowenthal. It involved the study of elementary Hebrew as well as basic tenets of Judaism. Peggy and I attended the class for about six months. I enjoyed it immensely, partly because I was arguably the best student in the class. Contrary to the impression given by the interview, most of the students in the class were there because they were going to marry a Jew.

Ever since we met, we had been using her father's car with Peggy at the wheel, or we used the train from North Station to Melrose. Now Peggy persuaded me to learn to drive. I studied the Massachusetts driver's rule-book, passed the written test without too much difficulty, and began taking driving lessons. The instructor used a car that was painted white, and had an extra brake pedal for him, as he sat in the front passenger seat. At the start of the second lesson, he asked me to move the car out of its parking space. I turned the wheel to the left when I should have turned it to the right, and dented this white car. The instructor kept his cool during the remainder of the lesson. He told me afterward that he personally fixed the dent. My guess is that if he had reported it to the driving school, it would not have looked good for him as a driving instructor. I had no other accidents, but I was a difficult student nonetheless. The basic problem was that I had a hard time following orders. For example, the instructor would say, "Please turn right," and I might respond with "Why?" After 28 lessons the instructor decided that I would be better off driving on my own than obeying his instructions. He helped me pass the driving test by having me practice following the actual route that the examiner would use. After a couple of practice runs we went into the office, got the examiner, and I went through the route a third time with no difficulty.

Peggy's conversion course was followed by a conversion ceremony. This took place in a *mikvah*, a ceremonial bathhouse in Brookline. Peggy had to get fully undressed, get under the water for a while, and a committee of three rabbis questioned her while standing behind a curtain. I was standing there, too. I don't recall the questions, but Peggy remembers being asked whether she renounced Jesus, the Catholic faith, and all its tenets. She said yes, but has since felt that this was an unnecessarily difficult question to have been asked.

A few weeks later, as the 1970 fall term got under way, Peggy pointed out that she had fulfilled her part of our bargain and asked when we were going to get married. I said after the end of the term, not realizing how little time this would give her family to rent a hall for the wedding reception, decide on the menu, order flowers, and hire a photographer and a band. Peggy managed to find a band, which showed up a week early! They apologized for the confusion, cancelled a gig, and showed up on our wedding day as well. Peggy had decided to make her own dress, and that added immeasurably to the rush. It had a great many buttons that had to be sewed on, and that took time; in fact, she finished it on the day of the wedding. The date of the wedding, decided by the availability of the hall, became December 27, by coincidence her father's birthday. We asked Rabbi Lowenthal to officiate. He was also a rabbi at the small central Boston synagogue then on Tremont Street in downtown Boston, and we were able to reserve it for the ceremony. The invitation list was small, about two dozen. Peggy's brother and sisters were all to come; my parents and my brother were to come from New York; we also invited Professor Weizenbaum and his wife, Ruth, and Bob Fenichel would escort Peggy's sister Jane.

My own preparations were relatively simple. Peggy and I went to the Louis store in downtown Boston and picked out a nice suit for me. I went to the Garber Travel Agency to get tickets for our honeymoon trip to the West Coast. On the day before the wedding I was called up to the Torah. This is the traditional *aufruf* prior to a wedding, in which the groom goes up on the podium and says blessings for the reading of a portion of the Torah. It took place in the Kehillat

Israel synagogue, on Harvard Street in Brookline. My parents were there, and I did the blessings quite well. Rabbi Lowenthal seemed pleased.

Even a small wedding seems to have the potential for problems. There was a snowstorm before ours, and traffic was tied up all over the city. The Garveys had rented a limousine to bring the bride to the synagogue, as well as the rabbi and his wife, who did not drive. Traffic came to a standstill a block from the synagogue, and the rabbi suggested that everyone get out and walk. His wife pointed out that the wedding dress would be ruined, and it was best simply to wait, even though they were late. When they arrived the rabbi took Peggy and me to a table and showed us the *ketubah*, the Jewish marriage certificate, to be signed with our Hebrew names. Joe Weizenbaum was our witness, and he had to practice his Hebrew name, which he obviously had not used in many years. Then we signed the Massachusetts marriage license.

The rabbi pointed out to everyone that since this was the fifth day of Hanukkah we should light five candles that night. From the back came his wife's voice: "Six." She was right, of course, since the Jewish day begins on the previous evening, and thus we were celebrating the sixth day of Hanukkah.

The ceremony itself takes place under a *huppah*, four slender poles holding the corners of a Jewish prayer shawl. My father, my brother, and Peggy's father and brother held the poles as Peggy and I stood beneath. In Israeli ceremonies the bride's mother and the bride circle the groom several times, but this was clearly inappropriate to this wedding. The rabbi, knowing that the wedding was attended by non-Jews, tried to explain every step of the ceremony. In the middle of all this my mind wandered, and I realized to my horror that I had forgotten where I had put the glass that is broken by the groom at the end of the ceremony. Panicked, I turned to Peggy's family and muttered, "Find the glass!" Her sisters and brother-in-law slipped out in a feverish search, even going to an upstairs bathroom to find another glass. The rabbi understood the problem and extended his remarks by many minutes. Finally the sisters returned bearing the glass and the ceremony continued. With a single stamp of my foot I broke the towel-covered glass, which went some way toward making amends for losing it in the first place, but not enough for the rabbi, who added as an American touch, “Do you take this woman to be your lawful married wife?” Surprised, I said “Yes,” but he said sternly, “Louder.”

We departed for the hall. The rabbi had been doubtful about how my father would behave at a reception that followed the wedding, but he need not have worried—my father was correct in his behavior, as a good German should be. The party went quite smoothly. The food was good, and the band was excellent. The only hitch came when Peggy and I were supposed to cut the cake. We tried and tried, but couldn’t do it. I finally realized that there was cardboard between the layers, for ease of serving.

At last it was time for Peggy and me to leave for our apartment, and from there to begin our honeymoon. I had recently bought a car from Adolfo Guzman when he left to go back to Mexico, and had managed to get it through the snowy streets without incident. But it refused to start. After numerous tries Bob Fenichel had to help me push it out of its parking spot and keep pushing until it finally started. Peggy, who had arrived in a limousine, left for her new life in a considerably lesser vehicle.

I had bought a package trip to San Francisco, Los Angeles, and Las Vegas. I had been to San Francisco and Las Vegas earlier and wanted to show them to Peggy, and I needed to do some business in Los Angeles. The 1971 conference I had been organizing was to be held at the airport hotel in Los Angeles and I wanted to see the accommodations. In San Francisco we stayed at the Sheraton Palace Hotel, right on Main Street in the heart of the city. This venerable hotel had a glass ceiling that reminded me of Milan. I had my first Spanish omelet there, and it was great. I managed to convince Peggy to take a bus tour of the city, the one that labels itself the greatest bus tour in America. She loved it. San Francisco was a great start to the honeymoon.

The travel package I had bought said that we would be met at the LA airport. We waited a long time and there did not appear to be anyone to meet us.

Finally I heard a page for Señor Mozes. On a hunch, we met the person who sent the message and he was indeed looking for us. I asked why he paged us in Spanish, and he said that the package deal I had bought was usually used by South Americans coming to the United States for the first time. In addition to driving us to the airport hotel he was obliged to give us a tour of Los Angeles, especially the homes of the Hollywood stars. We laughed at this, but he insisted that he would not get paid unless we took this tour, so we agreed to meet him the next day. We actually received a tour of Hollywood and homes of the stars: it was so ridiculous and campy that Peggy and I loved it. The guide understood and was relieved that we signed his form toward the end. I took a tour of the hotel facilities that would be used during the conference. All appeared in order, pretty much as I was told by the local arrangement committee.

Our next stop was Las Vegas. Peggy said that she was not interested in gambling, but when we got off the plane and she saw a one-armed bandit in the airport, she had to try it. We stayed at the Flamingo Hotel, one of the original hotels along the Strip, which served free drinks and relatively cheap food, since they made their profit in the casinos. The entertainment was excellent, too. Las Vegas ignores the clock; there were no clocks in the lobby, and no way except one's own wristwatch to keep track of time in the casinos, where even day and night blend together in a seductive way, as there are no windows. We saw Chubby Checkers playing in the lounge at 3AM; the Smothers Brothers headlined during the evening. Peggy liked to play Keno, a version of Bingo, which she played for small stakes. I was thrilled when she won $80 in one game, and other players must have thought she won thousands, given my reaction. Peggy played until she had gambled all the money her father gave her for the purpose when he heard that we were going to Las Vegas, and then she stopped.

We had arranged to move to an apartment in North Cambridge following the wedding, and to it we returned at the end of the honeymoon trip. We rented it from a construction worker named George, who had renovated a two-decker house. We had the lower floor plus the basement. The apartment was full of unexpected nooks and crannies, so we called it George's Folly. We had hardly any furniture; one of our first projects was making a table with two large concrete blocks for legs and a plywood top glued onto them. We made another small one out of five pieces of plastic. A few months later we bought a rug for the bedroom, and as I raised the bed to put the rug under it I severely strained my back, damaging the sciatic nerve. The doctors told me to rest and gave me some exercises to do in bed. Minsky heard about this and sent one of the lab's staff members to install a phone by my bed. I was wearing a Little Red Riding Hood hat when he arrived—Peggy and I love hats and our collection of them has been tried on by most of our guests over the years. The staffer tried not to stare at me in my strange headgear as he installed the phone. This happened toward the end of the spring term, and I had to juggle things to complete my graduate course. I continued to have sciatica in my left leg for several years.

Soon after Peggy and I returned from our honeymoon there was a meeting of the editorial committee for the upcoming conference, to make the final selection of the papers. Bob Caviness complained about errors in some of the invited papers, especially the one I wrote on simplification, and about my inviting so many papers. I weathered the storm, and the conference went ahead as scheduled.

I was told by Bob Fano that I would be promoted to Associate Professor in July. This was good news, but also something of a surprise, since I was not involved in the process. At the time, promotions to associate professor without tenure were largely done as internal promotions, with the letters of reference being largely written by MIT faculty. Unfortunately, by the time I was an academic administrator seven years later, things had tightened up considerably, largely I suspect because the MIT School of Science wanted the bar for promotion set quite high. In addition, all searches for faculty positions had to be national, and the rather loose process by which I was hired would no longer pass muster. I recall a discussion in the late 1960s at the Project MAC cafeteria involving Lotfi Zadeh, then head of Berkeley's EECS department, who was suggesting various appointments for CS professors around the country. He was so influential that a number of his suggestions were taken up by departments in schools such as RPI.

I had been elected treasurer of the Symbolic and Algebraic Manipulation group of the ACM a couple of years earlier. Jim Griesmer of IBM was chair of the nominating committee. He called me one day and said that the associate chair of the group, Bob Tobey, had refused to run for chair of the group and asked if I wanted to run for the office. I said yes.

Meanwhile, research and theoretical work continued. Bob Risch sent me a paper in which he solved the full integration problem. The missing piece of the puzzle was the integration of algebraic functions that had led to so much effort in the nineteenth century. The reason these integrals are difficult is that their derivative contains so little information about the integral, such as the degree of the function. What Risch discovered is that algebraic geometry, the field that Riemann essentially founded in the mid-nineteenth century to deal with these types of problems, had actually managed to solve the problem, but no one realized it. Sadly, it appeared that algebraic geometers did not care very much that the problem that led to the founding of their field was indeed solved.

The symbolic algebra conference took place in July of 1971, with about two hundred in attendance—a good turnout for our field. I hurried from one session to another to make sure things were going well, rarely staying for a full session or even a full invited talk. I did make an exception for the talk on factorization of polynomials by Elwyn Berlekamp of Berkeley, who was something of a showman. He made it appear that he did not know what he was doing, and all the while he pulled technical rabbits out of the air. I loved his talk. In fact, the invited talks overall were of very high quality, as were most of the submitted papers. As things went on, it was clear that the conference was a great success. Griesmer told me that Tobey, who was the general chair of the conference, had changed his mind and wanted to run for chair of the group. I told him that I did not intend to step down as candidate at that point. At the final session Caviness made a surprise announcement, awarding Tobey and me individual plaques for our work on the conference, which I felt made up for his outburst a few months earlier. The quality of the papers at the conference was so high that the ACM agreed to have two special issues devoted to a selection of these papers, probably a record for a single conference. Both my papers were selected for the August issue of the *Communications* of the ACM, although I did have to shorten them to meet the page restrictions on the journal.

When I returned I found Project MAC in crisis. As noted earlier, Licklider was a great visionary, although was not a particularly good manager; Minsky, feeling that his group was not getting proper recognition and support especially in the matter of space, wanted to split his group from the Project and create his own laboratory. To do this he needed two agreements, one from MIT and one from the main sponsor, ARPA. I attended a meeting in the Engineering Dean's Conference Room, run by Jerry Wiesner, who was now president. We went around the table hearing arguments for and against the split. Louis Smullin asked for my opinion, and I said that I was in favor of the split. Unfortunately, I was not involved in the discussions with Larry Roberts and others at ARPA in which it was agreed that the AI Group would indeed split off, but that the Mathlab Group—Bill Martin and myself—would stay with Project MAC. I was frankly surprised by this decision, since I still viewed us as doing AI, at least in part, but it was out of my hands. In retrospect, the decision was probably in our group's best interest, since Project MAC could obtain more support for us than the new AI Laboratory likely would have been able to garner.

Then Bill Martin surprised me by saying that he wanted out of our group. He felt that I would get much of the credit for our joint work, partly because I had the doctoral students and he did not. Besides, he was becoming interested in another topic, automatic programming. I was thus placed in charge of the Mathlab group, a position I held for the next dozen years. I sorely missed Bill's leadership and advice, but I had to make the best of it.

There was another international computer conference I wanted to attend in the summer of 1971, this time in Ljubljana, Yugoslavia, and I had also been asked to give a talk in Bonn. So Peggy and I arranged a European trip for ourselves. Peggy, who had never been abroad, was of course excited. We went to London for a few days to revisit many of the sites I had seen a few years earlier, and from there to Bonn. We arrived late at night and most of the restaurants were closed. The one in our hotel was still open, however. The waiter brought us the menu, and when we asked for one item or another the answer was always a polite "no." Finally, we caught on and asked what was indeed available, and he said "eggs." So we had eggs for dinner.

I gave a talk to a research institute near Bonn, while Peggy used the opportunity to explore the city. I taught her how to ask for directions in German. When I returned to the hotel she told me that she did fine in asking for directions, but then the Germans told her how to go in German, and she understood nothing of what they said. After a couple of days in Bonn we flew to the conference site in Ljubljana.

Ljubljana is a modern city in Slovenia, at that time part of Yugoslavia. In 1971 the cost of restaurant meals in this city was amazingly low. Patrick Fischer, who had moved to Cornell, was in town with his wife, and Peggy and I joined them for dinner at a restaurant, where we learned for the first time how trout should be served. We stayed at a third-class hotel in the nearby town of Bled, where Marshall Tito's summer home occupied an island in a lake. I recall a discussion about how Tito kept the country together and how it would break apart after he died. When he died in 1980 I expected the breakup to occur soon thereafter, but it did not, although the country became the site of terrible fighting just a few years later and broke up just as was predicted.

While we were in Bled, Peggy left our hotel room one day just as a man scrambled out of the hall bathroom, his pants almost around his ankles, screaming, "Fledermaus!" and a bird of some kind seemed to fly out with him. Peggy ran downstairs to report this situation, but the clerks were quite calm.

They said that one of the rooms in the hotel had been taken over by bats, “flying mice,” which is what the man had been shrieking in German.

The conference in Yugoslavia must have been uninteresting, since I remember nothing about it. Peggy used the time wisely to visit a Yugoslavian professor in Zagreb working in her area of recreation therapy. Afterward we flew on to Paris. I took Peggy to the one-star restaurant I had visited a few years earlier. We had no reservation, so they placed us against a wall rather than overlooking the Champs Elysées. The food was good, as was the rest of the service, but we were much disappointed by the seating arrangement, and the rich sauces left Peggy suffering from an upset stomach. We much enjoyed the tour of the Louvre and the walk leading to the Eiffel Tower, but we were pleased to get back home.

Shortly after we returned I learned that I had been elected chair of the Symbolic and Algebraic Manipulation Group. I had bet Caviness that I would buy him some lobster if I won. Now I owed him several pounds of lobster. It took me about a decade before I found the time to buy the lobster and bring it to him. There would be no other big conference during my two years as chair. Instead we had annual sessions during various ACM national conferences.

The Mathlab group obtained its own DEC10 computer at this time, freeing us from time-sharing on the AI machine and within a year allowing us to provide Macsyma service over the ARPANET. For a while our Mathlab computer was the second most popular site on the ARPANET, due to people using Macsyma. ARPA also put pressure on Project MAC to come up with a new project to replace MULTICS, which was due to be completed soon. Bill’s automatic programming project was our major candidate. Corby and I were on a committee that Bill chaired, to develop this concept. The basic idea behind automatic programming was to permit programming in a language close to the application, and have the underlying software system translate the high-level requests into lower-level ones, and even optimize the resulting program. Bill was interested in business data-processing issues; he had become a faculty member in the Sloan School of Management by that time. Thus he was interested in problems involving data bases and extensive searches. In fact he had done a thorough study of search algorithms, which was published as a tutorial survey in an ACM publication. I was hopeful for Bill's project and the overall goals, but did not spend much effort on it.

Licklider resigned as Director of Project MAC and was replaced by Edward Fredkin. Ed was an unusual academic. He never graduated from college, but dropped out of Cal Tech to enter the Air Force during the Korean War. He worked at Lincoln Lab during its heyday in the 1950s and then at BBN while Lick was there. Ed formed a company, Information International Incorporated (III), which developed a computer hardware and software system that allowed it to read information expressed in words in many different fonts and enter that information into a computer. The original information was usually stored on film in those days, and thus the III system was called a film reader. These systems cost several hundreds of thousands of dollars each, and sales were slow in the early days, so much so that the company nearly went under. Eventually sales picked up, and Ed formed a division of III in our building in Tech Square. The space he rented had previously been used by the Navy to debrief some of their spies returning from Europe, so it came complete with shower facilities. Jerry Wiesner was a friend of Ed's and helped appoint him as a full professor of Electrical Engineering in the early 1970s, strongly supported by both Minsky and Licklider.

There is no doubt that Ed is a brilliant man, albeit an unusual one, and certainly a wealthy one. The first time I recall seeing him was on the Johnny Carson show in the 1960s. Carson asked him what he thought computers would be able to do. When Ed replied that they would cut hair. Carson naturally asked, "How?" Ed said, "First you get a good haircut from a barber. Then when you go to sleep, they'll put a small robot on your head and it will measure each and every hair. When you want a haircut again, they'll put the robot back on your head and it will cut each hair at the length it was before." Lest one think this is a one-of-a-kind type of story, I recall Ed telling us how robots could be used to find out secrets in the Kremlin. Imagine, he said, a robot placed in the sewer system in Moscow. The robot has access to a map of the system, so it can find the pipes leading to the Kremlin. Inside the Kremlin it goes up to the bathroom of the Secretary General and listens to the conversations.

A few weeks after he was named director, Fredkin called a meeting of the Project MAC faculty and told us that ARPA was very unhappy with our progress. We needed a new project, and he wanted to suggest one. There were two recent dissertations in the AI Lab, one by Terry Winograd, the other by Carl Hewitt. He wanted the Project MAC faculty, most of whom knew little of AI, to re-implement these dissertations and make them into robust working systems. I don't remember what the others said, but I surprised Ed by making some rather negative remarks about his idea. He had expected that as a product of the AI Group I would surely support the concept. In fact, I was quite unhappy with Hewitt's language, called PLANNER, which was a variant of a General Problem Solver system of Newell and Simon, and I had had a major argument with Seymour Papert over Carl's dissertation. On the other hand, I found Winograd's work, although it used a version of PLANNER, to be of great interest. He built a system to understand sentences that described a visual scene involving children's blocks; his key conclusion was that the problem of natural language semantics was very hard. I did not feel that making this system robust, as Ed appeared to suggest, would solve the fundamental problem that Winograd pointed out, even if we used a better version of Hewitt's problem-solving machinery. I expressed my concerns to Weizenbaum, and he suggested that I talk to Smullin, the head of the department. Smullin was very attentive, but in the end could do very little about Ed's management of Project MAC. Nor did Ed pursue his original idea with vigor, and thus life went on as before.

Smullin felt the need of new ideas about the direction and processes of the department, and created an Advisory Committee, to which I was appointed. Michael Dertouzos was elected by the members to be its first chairman. Mike was a young full professor who had started out in the control side of electrical engineering, but had moved to Project MAC in recent years. One day I was giving a talk at the University of Michigan, and I received a call from him. He wanted me to chair an Advisory Committee subcommittee that would deal with the implications of the EE Department's inability to increase the size of its faculty any further. Little did either of us realize that this assignment would land me in over a quarter of a century of academic management-related work.

MIT, like many other research universities, had grown a great deal since the end of World War II, much of its growth fueled by research funded by the Department of Defense. This ended with the Mansfield Amendment of 1968, which said the DoD must justify its research projects based on military need. A joke in the 1960s was that the DoD could justify spending research funds on improved pencils, because the DoD was the world's largest purchaser of pencils. Research on pencils could not be justified under the Mansfield amendment, and neither could many other worthwhile projects.

By 1968 the EE Department had grown to about 125 faculty. A key reason for this growth, in addition to the research funding issue, was the money obtained by Gordon Brown from the Ford Foundation. Brown and others had promulgated the idea of engineering science, which the Foundation wanted to spread to other universities. We were encouraged to hire many post-doctoral fellows under the Ford Foundation grants, and to make them assistant professors, in the belief that they would then carry the engineering science approach on to other institutions. This program was coming to an end. Gordon Brown's process worked, although it did have a side-effect that caused some difficulties in later years. The key problem was that some of the Ford post-docs, usually MIT PhD's, stayed on at MIT, which made non-MIT PhD's who did not stay on very unhappy. As a result, some former Ford post-docs at other institutions tried to convince young PhD's not to accept faculty offers by MIT in the following decades.

The effect of these and other budgetary issues was that the Department's faculty size shrank: instead of having the fifty assistant professors of 1968, we had fewer than fifteen three years later. My committee was to examine the departmental implications of such changes. I tackled this largely by myself, creating a very simple model of the EE faculty. I assumed a fixed faculty size, say 100. I looked at the age distribution of the tenured faculty we had, and noticed that there were about two per year for ages 45 to 65, and four per year for ages 35 to 45. I called the growth in the average number of tenured faculty of ages 35–45 the “tenure bulge.” The tenure bulge was due to a doubling of the hiring rate at MIT in the mid 1950s. In addition we had quite a few untenured faculty in the pipeline, and unless we wanted the probability of tenure to greatly decline, with attendant morale problems, then the percentage of tenured faculty would grow to as much as 80% in a few years. This would greatly reduce the number of new hires we could make each year until the bulge disappeared decades hence. The model was not limited to our department, of course. Jim Bruce, who had become the associate dean of Engineering, sent my memo on the model around the Institute. I became a minor hero of the Advisory Committee as a result of this analysis.

The 70’s were a decade in which my trajectory was rather different from that of MIT. I started the decade as an assistant professor and ended it, as we shall see, one year away from becoming head of the largest department as MIT. MIT had serious financial difficulties in the 1970’s. The Mansfield amendment had an impact not only on research volume but also on recovery of overhead expenses. In addition, the separation of the Draper Laboratory from MIT, a result of the student uprisings in the late 60’s, had a serious impact on overhead recovery as well. The stock market decline in 1973-74 had an impact on MIT’s endowment and its income. One effect of these financial difficulties was the reduction in the rate of hires of new faculty. This was the reason for the study just described. This reduction in hires also had a long-term effect on my career when I became dean of engineering and provost in the 1990’s.

The Macsyma system grew in capabilities and became sufficiently robust that we wanted to try it out on some users. Fortunately, the ARPANET provided us with the opportunity to acquire users from around the country. As a result of talks I gave at various conferences, a number of scientists and engineers knew of the system and its availability on the Mathlab PDP10 at MIT. We began to have users from universities, NASA labs, Navy labs, DOE national labs, and even industrial labs. Many of the users were physicists and mathematicians, but we also had a scattering of engineers. In some periods in the early 1970s the Mathlab computer was one of the top two sites on the ARPANET in terms of usage by people coming from the network. This pleased ARPA, which viewed its mission as to create technology and spin it off after five years. Our five-year period would be over in 1973, and they expected us to fund the effort through other agencies and companies.

The ACM was celebrating its 25th anniversary in 1972 and wanted a special issue looking toward the next 25 years. I submitted an article, “Toward a General Theory of Special Functions.” Despite the pun embedded in the title, the paper was quite serious, arguing that with advanced results in mathematics we might be able to greatly generalize the concept of elementary functions found in the calculus. What was needed was a relatively automatic way of developing the knowledge we have with functions, such as the exponential function, with which we are most familiar. Such knowledge includes ways of differentiating, simplifying, and integrating the functions. If the new function is defined in terms of an ordinary differential equation, then derivatives are easy to compute; simplification is harder, but once new identities are obtained, integration might follow. So I issued a challenge to create a theory that would permit identities and other knowledge to be developed for functions defined in terms of differential equations. As a result, the whole notion of integration in closed form would take on a relativistic hue. If you can’t integrate it in terms of functions you already know and love, then how about calling the integral F(x), and developing the identities so that you feel almost as comfortable with F as you would be with the usual functions? Now the original integral is expressed in terms of “almost elementary” functions. I now realize I was too optimistic about the programme as outlined in the paper. One of my doctoral students, Jamil Baddoura, was able to generate identities for some new functions, but this was hardly done automatically; Professor Kolchin of Columbia (whom I will always remember as the one who once denied me access to the set theory course) wrote an important book developing a theory of identities about a class of functions. There is little more that I know of that can be viewed as progress toward the goal I outlined. It is still a worthwhile goal, but we will need a new generation of mathematicians to work on it.

One of our goals in the Mathlab group after the 1971 conference was to implement the new algorithms that were presented there, especially by Brown and Collins. These algorithms claimed to be optimal. The key one was the algorithm for computing the Greatest Common Divisor of two polynomials. The GCD algorithm for integers is credited to Euclid, and involves division. The polynomial GCD algorithm is critical in simplifying the result of adding two rational functions, that is ratios of polynomials, and it is used in many other situations as well. Fateman wrote a version of Brown's algorithm; one day we made it the default GCD algorithm in the system. We got calls immediately from users around the country claiming that the system had ground to a halt. So we changed back to our older algorithm the same day and began to analyze Brown's and Collins's approach, which was to plug in various integers for all but one variable and use a special GCD for the far simpler single-variable problem. The number of such integers that needed to be plugged in so that we had enough information for the full solution grows exponentially with the number of variables and their degrees. This approach is optimal, I later said, if the polynomials are as large as galaxies, but in practical situations we may have a polynomial with seven variables but only five terms. In such sparse situations the Brown/Collins approach is clearly not the algorithm of choice. We quickly began to look for alternative algorithms for computing the GCD.

At the same time, Paul Wang and a post-doc from MIT's Mathematics Department, Linda Preiss Rothschild, were working on extending Berlekamp's algorithm for factorization to polynomials with multiple variables. The technique described in van der Waerden and used in the old Mathlab system was similar to the one used in Brown/Collins for the GCD: plug in lots of integers, solve the resulting one-variable problem, and combine the results. Wang and Rothschild discovered another approach in algebra, called the Hensel Lemma, which is similar to Newton's method. It substitutes a set of integers for all but one variable and solves the factorization problem using Berlekamp's algorithm. Knowing the integer value that was substituted for the second variable and subtracting from the original problem allows creation of a factorization in two variables. By repeating the process one can generalize the one variable factorization to include all the original variables. The process is rapid and works in all but a few cases, those in which the original integer values cause the resulting problem to become too simple, such as when the degree of the polynomial in the single variable is too low. In many respects I view this algorithm for factorization to be Macsyma's greatest algorithmic achievement.

It was one of my mathematics graduate students, David Yun, working on improving the GCD algorithm, who pointed out to me that the problem of computing the GCD is similar in many respects to the factorization problem: the GCD of two polynomials is a factor of each of the two polynomials, so it would be possible to use the Hensel Lemma approach for the GCD algorithm. I encouraged Yun to go ahead with this approach. When it proved successful I pointed out the need to deal with those cases in which the original choices for integer values caused difficulty. I suggested a solution to one such problem, but much additional work on the other problem by both of us proved to be fruitless. A paper on this new approach to the GCD algorithm would be political dynamite because of claims by some of optimality for this approach, so I broke my rule and placed my name together with Yun's on the paper we submitted to the 1972 National ACM meeting. In some fields it is the custom to place the supervisor's name on every student paper; at Project MAC some groups did it and others did not. Since the earliest papers by Fateman and Wang our group never had, and I never did so again, partly because of the negative reaction from the rest of the group to this one instance. The community's reaction to the paper was better than I had expected. Brown claimed that he had never felt that the algorithm he described was optimal.

Gerry Sussman and I discussed Carl Hewitt's language, called PLANNER, which I didn't much like. We devised a new language, eventually called CONNIVER. Backtracking one's steps automatically was a key feature in PLANNER; CONNIVER had built-in pattern matching, and relied on programmer-controlled backtracking. Used for a few years at MIT's AI Lab, it was supplanted by Sussman's language, Scheme. Scheme, which is actually quite close to LISP, solves a technical problem in the latter, and is used in MIT's very popular introductory programming course, 6.001. Thus we went from planning to conniving to scheming in less than a decade.

Peggy graduated from Northeastern in June 1970 with a degree in recreation education, and spent the following year working for her department, on a slide/tape production that would promote her entire field. She worked with a Northeastern photographer, taping children and adults engaged in various recreational activities in the Boston area to provide the sound accompanying the photos. The Northeastern Recreation Department used the production for several years thereafter.

Peggy then founded a company, Insight Inc., to produce similar slide/tapes and TV productions, working out of our Massachusetts Avenue high-rise apartment, to which we had moved after a year in George's Folly. It was a modern apartment building that housed several other MIT faculty members and their families. Peggy had been offered opportunities to work for the Boston Parks and Recreation Department and for local TV stations, but she wanted to work for herself. She had a full-time partner, Alan Gleiner, and used the Northeastern photographer as needed. The firm made slide/tapes for the City of Waltham and others, but funding by recreation agencies was hard to come by. The firm did not have much income during the next few years and eventually was disbanded.

That year Dick Thornton and I were on a committee to consider our overall undergraduate offerings in computer science, and as a result we began teaching a new undergraduate course. It was on mathematical methods, largely numerical ones, but I managed to slip in some computer algebra methods, too. In prior years I had also taught sections of our system architecture course with Jack Dennis, and our abstract algebra course with Fred Hennie. I found both to be very good lecturers, although the students felt that Dennis's lectures were somewhat dull, nor did they much like the content in the abstract algebra course. I championed the algebra course for many years, and it stayed in our program until I stepped down as department head. A computer architecture course is a required subject for all our EECS undergraduate students to this day.

Ed Fredkin had sold his III stock at a neat profit, and as a result he owned—among other things—an island in the British Virgin Islands, called variously Drake's Anchorage or Mosquito Bay, to which he invited several of the faculty and their spouses in 1973 for a two-week stay. It was the only "working vacation" I had yet experienced, and I thoroughly enjoyed it, as did Peggy. In the afternoons we spent some time at the beaches around the island. There were wonderful opportunities for snorkeling. I was belatedly grateful to Columbia University for that swimming requirement, which must have been what enabled me to enjoy time in the water, watching the multicolored fish. Fredkin warned us about not staying in the sun too long, and we were indeed careful. The island had a number of bungalows and a kitchen. Peggy and I brought kosher food from Boston, and we ate some of the local fish as well. On the whole it was a glorious vacation, so much so that Peggy wept when we boarded the boat to return home.

In the mornings on the island we worked. Our goal was to learn enough about medicine so that we could begin a serious attempt to automate medical diagnosis. Tony Gorry, a friend of Bill Martin's who had worked on medical diagnosis for years, had created contacts with doctors at Tufts University's Medical School. However, he had just left MIT for the Baylor School of Medicine, and Fredkin wanted to move us back into this field. Minsky, Sussman, Martin, and I were the AI experts; our medical experts were Bill Schwartz, the chief of medicine at Tufts, Jerry Kassirer, his second in command and later editor of the *New England Journal of Medicine*, and Steve Pauker, a cardiologist at Tufts. We spent most mornings learning about a particular set of kidney diseases. Schwartz impressed on us the fact that kidney diseases are tricky, and that specialists who are outside of the major medical schools might not see enough cases to make good decisions. "They could easily kill you," he intoned. Thus it would be very helpful to have a program that could embody the knowledge that the best specialists had, and to make this available to all specialists.

The disease we learned most about was post-streptococcal acute glumerial nephritis, or AGN. This is a condition that sometimes follows a strep throat treated with penicillin, and it has several possible outcomes. In one you might indeed die of kidney failure; in a second your kidneys will not function well, but you will live; in a third you will recover from most of the symptoms. An advantage to making a correct diagnosis is that a patient on one of the poorer paths might be moved onto a better one. Sussman was quite excited by all this and promised that he would have a program to diagnose AGN and related malfunctions in a couple of weeks after returning to Cambridge; I was not so sanguine. I am sorry to say that I was right, and although we spent many years in this area and made significant progress, we cannot claim to have solved the problem that was posed on that island in 1973.

Louis Smullin announced that he would step down as Head of the EE Department as of January 1974. This led to a crisis because Ed Fredkin then mounted a campaign to split the department in two, one department for Electrical Engineering and one for Computer Science. Ed had even commissioned an architect to design a new, larger building for CS on the site of the venerable Building 20—a ramshackle frame structure much loved by its denizens, in which radar was developed during World War II. The architect's design was a torus so that all of us would be on one floor and thus there could not be a hierarchy based on floors, as there was in Project MAC. But the MIT administration did not pursue Ed's idea, Building 20 was ultimately dismantled to provide space for the Ray and Maria Stata Center. It took over forty years to obtain a new CS complex to replace the rented building in Technology Square that was initially occupied in 1963.

I was not happy with Ed's idea of splitting the department. I felt that CS had done rather well under EE administrations. Besides, the entire MIT upper administration, President, Provost, and Chancellor, were all EE faculty at the time. Ed and others felt that we needed to be represented by a department head who clearly understood us and also understood the tremendous growth potential of CS. Fredkin, learning that Japan wanted to greatly increase its research program in AI in a new five-year plan, had wanted us to write President Nixon a letter comparable to Einstein's letter to Roosevelt regarding the atom bomb. I do not recall that idea getting very far, but Ed had certainly created an internal crisis, although not a national one. Alfred Keil, then Dean of Engineering, created a search committee for the EE department head with Wilbur Davenport as its chair and Corby and Hermann Haus as the other senior faculty. In a surprise move they named three untenured associate professors as the other three members of the committee. I was one of them.

At the same time that the search committee was getting under way I was elected chair of the Advisory Committee, replacing Mike Dertouzos, who had lobbied for me. Given Fredkin's move I felt it was urgent to create three subcommittees, especially one to recommend the structure of the department going forward, including the possibility of a split into two departments. It was chaired by Rick Morgenthaler. A second, chaired by Dick Adler, would consider the department's educational program, and a third was chaired by Hermann Haus. All three were EE faculty. I played a strong role in all these committees, especially Rick's, and felt confident with these responsibilities, even though I was still untenured.

One day Bob Fano called me in and asked me for names of individuals outside of MIT who could write letters on my behalf for my tenure case, which he was preparing. I recall mentioning Brown, Griesmer, Hearn, and Collins, although I indicated some concern about the last name. Fano recognized most of the names I had mentioned. The Smullin administration was preparing seven tenure cases that fall, although the new administration would have to present the cases that would be brought forward to the Dean of Engineering during the winter. I was asked to write a short essay about my ideas for the future, and I believe I emphasized keeping the department together as one of my points. In future years candidates would have to fill out a much more extensive vita and offer many more outside names of possible letter writers.

The search committee first created a longish list of internal candidates for department head, and we split the department's faculty into six groups so that we could interview them all and ask their opinions about the candidates. I interviewed mostly CS faculty, mainly those in AI. I felt that our committee chair, Bill Davenport, had the best chance of keeping the department united. This was due partly to his wonderfully warm and caring personality, and partly to his research area, communication systems, an area with connections to both EE and CS. Thus I asked my interviewees what they thought about Davenport as candidate, although he was not on our official list of candidates. Not surprisingly, those who knew Bill felt that he would make a good department head.

When the committee got back together we reported on our interviews and created a short list to present to the dean. These were good people in my opinion, but they might not be able to deal with the issue of splitting the department that Fredkin had championed. I pulled Haus and Corby aside and said that we should tell the dean about how people felt about Davenport. Davenport had not wanted to be department head in part because his wife was not happy with the idea, otherwise he would not have been chosen as chair of the search committee. I believed that he saw the difficulty of keeping the department together and might relent if asked by the dean. When we met with

Dean Keil, Davenport presented the short list and Haus presented Davenport's name. Somehow Keil persuaded Davenport to agree to become the new head.

While the search was going on, the Morgenthaler subcommittee had created three options for the structure of the department: remain as it was, a single department, but with a name recognizing the presence of CS; split into two departments; or become a separate school as Bob Fano had suggested. I liked the idea of a name change, and suggested we send a survey to each faculty member asking his or her opinion of various names. I intentionally rigged the survey by not giving the option of keeping the old name of Electrical Engineering. Furthermore, the alternatives other than the favorite one of Electrical Engineering and Computer Science were pretty awful. For example, one was "Electrical Science and Engineering and Computer Science and Engineering," surely a mouthful. When we received the responses about three-quarters of the faculty opted for EECS. Only Louis Smullin asked, "What's wrong with the old name?" The Davenport administration was now in place, and it had scheduled a meeting of its Visiting Committee in the Spring of 1974. The issue of the structure of the department would be on the agenda.

The Visiting Committee concept is one of the great organizational tools at MIT. The visiting committee of each department is composed of trustees (known as members of the MIT Corporation), alumni, and other presidential appointees. They report to the Corporation, rather than to the department head, dean or president as in other universities. This gives them great authority. The President, Provost, Dean, and Department Head pay great attention to recommendations of the various committees. The committees meet with faculty and students of each department, usually without the presence of the department's administration, allowing the faculty to express their views freely. Likewise, undergraduate and graduate students can express their concerns with the educational program to the visiting committee in executive session.

I presented the Visiting Committee the Advisory Committee's conclusion that the name of the EE Department should be changed to EECS. Thereafter the

Davenport Administration approved this recommendation in discussions with Dean Keil, and finally the Corporation itself approved the name change. Davenport's style and the name change created an atmosphere of relative calm in the department. Within a few months, discussions of a split had stopped.

While the name change was being pursued there was another recommendation of the Advisory Committee which created a significant change in the department. This involved creating a more unified undergraduate program for the department. Previously the EE major did not require CS subjects, although the CS major required three core EE subjects. I wanted to create a "common stem" of four subjects required for all EECS undergraduates. The Davenport Administration approved this idea and created an Education Subcommittee (Associate Head Paul Penfield, chair; Mike Dertouzos; Jonathan Allen of the Research Laboratory of Electronics; Sanjoy Mitter of the Laboratory for Information and Decision Systems; and myself) to determine how to implement it. We discussed the need to modify the existing introductory subjects, but with the deadline for a new course catalog upon us, and with me out of town giving a talk, it was decided to use four existing subjects as a "common core" for the entire department, even though this was not an ideal solution. Penfield later headed a committee to suggest modification to the four subjects, and indeed two were modified significantly in the next few years. Unfortunately, the introductory EE subject on electrical circuits was not much modified in the following decades from the version that was recommended in 1974. This is an example, I believe, where hasty implementation sometimes leads to reduced pressure to do the right thing. The implementation of the EECS Masters of Engineering program in the 1990's has some similar characteristics.

The Smullin administration prepared the various tenure cases, and decided to bring all but one forward. The Davenport administration had to present these cases to an Engineering Council made up of the Dean, other department heads in engineering, and a few others. Dertouzos, who was on Engineering Council at the time, later told me that he had to make a special presentation about computer systems in order to help get Bill Martin's case through. My case went through without difficulty, I was told, since I had worked on mathematical algorithms in addition to building a system. My tenure would be effective a year later, in July of 1975.

Thus I had a role in deciding not to split the department, in deciding who should be its next head, in changing the name of the department, and in determining its core undergraduate educational program, all before I was tenured. I have since thought about how I came to be involved in all these things at such a relatively young age in academia. My theory is that as one whose background was strongly in computer science, while most of the other CS faculty had been electrical engineers, mathematicians, or physicists, there likely was a power vacuum while the EE Department was being transformed by the growth of CS. The older EE faculty members were and are a great group of people, and like most engineers put solving the problem, whether technical or administrative, ahead of most other considerations. So they allowed me to make suggestions, and implemented them when they felt they made sense. As a side- effect of these experiences, I was drawn into academic administration for the next 25 years.

Early in the spring of 1974, a couple of months after the Davenport administration took over, Ed Fredkin announced his resignation as director of Project MAC effective in July. This would allow him to spend a term at Cal Tech as a Fairchild Scholar. This announcement took us all off guard. Suddenly we needed to find a new director. After much discussion in which I was not involved Mike Dertouzos was chosen. He was the director for over twenty-five years until his untimely death in 2002

There was another international computer conference in the summer of 1974, this time in Stockholm. The European algebra community took advantage of this occasion to hold a conference of its own in Stockholm just before the international conference. I submitted papers to each, and would spend seventeen days in Stockholm and vicinity. The paper I gave at the algebra meeting described the Macsyma system five years after we started and gave an overview of the various components of the system; the paper for the international conference was an overview of various approaches to algorithms, such as GCD, that were in use in the field. This paper was later chosen as one of the best computer papers of 1974, and was probably the only one chosen from that stellar conference.

Dertouzos showed up in Stockholm just before the international conference and asked me to become the associate director of Project MAC. MAC had had an associate director in its early days, but not recently. I agreed to take on this added responsibility, although it would detract from some of my teaching and research. Mike recalled then that he had first met me at a Project MAC Christmas party in 1968. Mike had done research in control but recently shifted to computer science. He claims that I said, “You don’t know me now, but you will.” If I indeed said that, then I was prescient. I was also overly confident of myself in those years, just after getting my doctoral degree.

One of the first things that Mike did when he returned was create an Executive Committee for Project MAC. Bill Martin, Corby, and I were on it. We agreed that we needed to change our name from that of a project to that of a laboratory. Projects are supposed to be of limited duration, but we intended this one to just keep on going. Someone suggested we call it the MAC Laboratory for Computer Science. Martin said that pretty soon we’d get rid of the MAC label, so why not do so now, and thus Project MAC became the Laboratory for Computer Science. Mike came to my group’s help with ARPA, which wanted to stop funding us, but Mike convinced them to give us a goodbye present, a new, faster DEC10 computer, eventually called the MC Computer. MC stood for Macsyma Consortium, a consortium of Macsyma users composed of the DOE, NASA, Navy, and the Schlumberger Corporation, which we created in order to assure our funding. With Macsyma users in each one of these agencies, we had funding for another few years at least.

Our group had several new students. Rich Zippel had worked for us since his freshman year. As an undergraduate he worked on generating power series. His friend, Barry Trager, worked on aspects of integration that involved algebraic numbers, simpler than the full algebraic function case but difficult enough on its own. Guy Steele worked on our LISP system with Jon White throughout his undergraduate years at Harvard. Steele had taken a programming test that Bill Martin created, and was the first one to ace it. When Guy became a graduate student he joined Gerry Sussman's group and worked on Scheme, a variant of LISP that solved the technical problem with free variables that I had addressed in the "The Function of FUNCTION in LISP" paper.

As I think of the students who passed through our program, I am pleased by their subsequent achievements. My first graduate student, Richard Fateman, became an Assistant Professor of Computer Science at the University of California at Berkeley, where he is now a full professor and served for a while as chair of the CS faculty; Paul Wang, first an Assistant Professor at MIT's Mathematics Department, later joined the Mathematics and Computer Science Department at Kent State University, where he is now a full professor; David Yun was a staff member at IBM Research in Yorktown Heights, N.Y., moving on to Southern Methodist University to become their chair of CS, and later to Hawaii to run the East-West Institute; Mike Genesereth, who was interested in AI, has become a full professor at Stanford University; Rich Zippel has headed a research lab for Hewlett-Packard; and Barry Trager is a research staff member at IBM in Yorktown Heights, New York. Our staff was also impressive. My former secretary, Ellen Lewis, was so good at helping Macsyma users around the country that we made her a staff person in charge of this important support function. She also helped write some of the original Macsyma manuals. I had written a Macsyma Primer, but had little patience with a complete manual of all of Macsyma's features. Ellen was probably the best known of all our staff, and at future meetings she would be surrounded by her admirers from the Macsyma community. She later married Jeff Golden who was in charge of maintenance of Macsyma. She unfortunately died a few years ago at a relatively young age.

Ed Feigenbaum, who had been impressed with my dissertation in 1968, became quite famous with new work done in his group in the mid-1970s that led to the creation of expert systems. Unlike me, Ed equated expert systems with knowledge-based systems. Expert systems relied on a set of rules to analyze a task, such as making a medical diagnosis. The approach became famous when the Schlumberger Corporation, a company that analyzed potential oil wells, claimed to use expert systems to help it determine whether a hole is worth digging deeper. This led other firms to invest in the technology, and made a number of companies, including some of Ed's Stanford off-shoots, quite wealthy. I was immediately concerned about the approach. I felt that it would fail when the number of rules went beyond a certain level due to the complexity of interaction between the rules. A key advantage of rule-based expert systems is that the rules need not be structured, since the problem-solving mechanism will take care of making all appropriate logical inferences. I, of course, believed in placing expert knowledge in AI programs, but I wanted the knowledge to be structured. I was told, by Ed or by someone else, to keep quiet because expert systems were going to make AI into a great success. In fact, for several years expert systems became a major business success. Then in the mid-1980s people realized that the systems became hard to change after some number of rules, and the 'AI winter' set in. Thus, once again, my point about the need for structuring knowledge seemed overwhelmed by Americans' love of relatively unstructured, or at best tree-structured approaches to organization and problem solving. I would ponder this American tendency many years later.

As associate director of LCS I chaired a lab committee to consider how we could improve our computing facilities, in particular in obtaining graphics workstations. At the time the best hardware/software systems were being built at the Xerox Palo Alto Research Lab (PARC). We were able to obtain as gifts a few of their systems, called ALTOs, and especially a Xerox machine connected to a computer, called a Dover. These devices are commonplace now—in fact, one is next to me as I am writing this. But the Dover was a tremendous achievement in its time, together with the special character sets and editors. We could not get enough of these ALTO computers, and we wanted some of their capabilities. Thus we considered building some computers with a few of their capabilities on our own. Professor Stephen Ward and his students decided to take on this task, using an early microprocessor as a computing engine. Several of us were quite skeptical of this effort, especially since it was promised to be done over a summer, but they indeed succeeded in developing a computer with a nice graphical display. This success made them reach for higher goals, and over the next couple of years they developed one of the early prototypes of a microprocessor-based high-performance computer, called a NU machine, for the Greek letter ν. The Xerox ALTO was based on a special-purpose computer, and we were still years away from Apple's Lisa/Macintosh line of computers. Thus the NU machine was a clear leader in high-performance personal computing. Mike Dertouzos decided against forming a company to develop this concept. Instead he had MIT license the patents to Zenith Data Systems. When their design team left, MIT licensed Western Digital; when they nearly went bankrupt, MIT licensed Texas Instruments; ultimately all these efforts failed to produce a high-performance personal computer, although LCS wound up with fees as well as computers based on the contractual agreements with each firm. This situation created an open competitive position for SUN Microsystems, an offshoot of Stanford University, which is now a multibillion dollar company, and had started with a roughly similar product.

The NU machine saga brings up the question of the role of MIT, and especially EECS, in the build-up and decline of Route 128, the circumferential highway around Greater Boston that is thick with high-tech industry campuses. MIT's radar work helped Raytheon grow to the multibillion dollar defense-oriented firm it now is; Lincoln Lab spun off many firms, especially DEC. When MIT's EE Department decided to stop research on silicon in the late 1960s, Stanford's EE

Department stayed with it, through some lean times, and its influence on Silicon Valley was and is great. In part this is due to their unique Master's program, which in turn is probably due to Fred Terman, their great department chair, dean, and provost, who created a symbiotic relationship with Hewlett-Packard by allowing them to form their company on Stanford land. HP attracted many engineers, in part, by promising them an opportunity to get a Master's degree at Stanford while working at HP. HP paid Stanford double the usual tuition for students in "TV land." Eventually many other firms in Silicon Valley joined HP in this program, creating a relatively close relationship between Stanford and its local industry. The same could not be said of MIT and its local industry, until perhaps the 1990s. MIT is a national university, and increasingly an international one, but MIT needs to be a local university as well. My attempts to create a Stanford-like Master's in Industry program during the 1980s were not very successful, nor were we successful in creating a SUN-like firm to complement the minicomputer giants along Route 128.

The AT&T Corporation wanted to celebrate its centennial in 1976 near the place where Alexander Graham Bell created the first telephone, not far from MIT, and commissioned several studies for this celebration. One was on the impact and history of the telephone. Another was on the future of the computer. Mike Dertouzos headed that study, and he and I co-edited the book that grew out of it. I wrote an chapter on the computer in the home, and I am gratified that most of its predictions have come true. For example, I suggested the existence of specialized chat rooms, although I did not mention (nor indeed did I surmise) some of their negative consequences. The one area where I was a bit too optimistic was in broad-band communication to the home. I predicted that this would occur within twenty years or 1996. It has actually taken years longer for wide-spread availability of broad-band to the homes in the US. Dertouzos' predictions in his chapter had largely to do with tailor-made clothes. He predicted that there would be machines into which one would place one's feet, and a pair of shoes would come out of the other end in a short while. I don't think we are as yet close to having such machines. The volume of essays entitled *The Computer Age: A Twenty year View* was eventually published in 1979 and was for a year the best seller of the MIT Press.

We also held a conference in MIT's Kresge Auditorium in 1976, and Mike gave an overview of our study. This was to be followed by Arthur C. Clarke's plenary talk envisioning communication in 2076. Recall that Arthur C. Clarke was the author of the book *2001*. While Mike spoke, Clarke had a coughing spell so severe he had to be taken out of the hall. When he recovered and gave his predictions for 2076, they seemed mild in contrast to what we expected in 1996, twenty years after the conference. I have often wondered whether this was the cause of the coughing spell.

The third major computer algebra conference took place in New York State in 1976, five years after the conference I had organized in Los Angeles. My students submitted several papers, and I was on the Program Committee. I expressed surprise that Rich Zippel's paper on power series was chosen as an invited paper, since he had just obtained his degree. They said that was surely not unusual, that doctoral students liked the attention they get with an invited talk. I said that they didn't understand—Zippel had just obtained his *bachelor's* degree. I need not have worried. Zippel gave a good talk.

Risch created a sensation at the conference by describing a new integration algorithm that fundamentally reduced integration to the solution of a set of linear equations. I had implemented his earlier and far more complex algorithm, and did not like the new method, finding it partly incomplete since in certain cases the bound for the number of equations was not known. I also was interested in extending the old method to integrate special functions of the type I described in the ACM 25th Anniversary paper. I felt the new approach could not be easily extended to such functions. My guess is that new algebra systems all use Risch's new approach, although I have not heard of a solution of the bounding problem.

Rich Zippel and Barry Trager began work on new problems. Zippel began research on the GCD algorithm, with the goal of solving the missing case in the Yun approach; Trager started work on extending his integration work to the full case involving algebraic functions, using as a basis the pointers to algebraic geometry made in Risch's 1970 paper.

Dertouzos and I had an important meeting with a group from DEC, headed by their VP for Engineering, Gordon Bell. We described our requirements for a high-performance personal computer and said that if they met them we could get ARPA to buy a hundred for the Lab. Such a computer needed to be an extension of the DEC10 architecture which permitted programs with more than 256K words; it must have a graphics display, a mouse, a keyboard, and a disk. Bell rolled out graphs indicating the rate at which technology was moving, and he said "No problem. You can get all you want for $10,000 per computer in a couple of years," and off they went.

Peggy and I, now married for five years, had held off having children while she ran Insight Inc. The company went out of business in 1974, but Peggy had developed pains in her breasts at about that time. The MIT doctors could not figure out the cause, and one specialist at MGH infuriated her by saying, "Life is a vale of tears, dear." After a couple of years of this pain she went to her childhood doctor in Melrose, who encouraged her to start her family, pointing out that the hormones generated by her pregnancy would help a great deal. Peggy became pregnant soon thereafter.

One day, a few months into her pregnancy, she called me at my office to tell me that she was having twins. Stunned, I said nothing for a long time. Peggy was calling from her gynecologist's office in Stoneham, and when the other ladies realized that nothing was coming from the other end of the line, they started laughing. I called my mother in Brooklyn, and she too was astounded. Since no one on either side of my family had ever had twins, we hadn't considered the possibility; but of course on Peggy's side there were indeed twins.

When I saw her later in the day, I asked Peggy how she found out. She said that the doctor, guessing that there were twins because of Peggy's pattern of weight gain, ordered an ultrasound exam. During the exam the technician exclaimed, "Look at all those arms and legs!" She nearly fainted, but then he calmed her by saying that she was having twins, and not a monster.

In 1977 Peggy and I were living in a three-bedroom townhouse in Norwood from which I took a train to work. One day we were shopping at the local supermarket when we saw an ad for the Dydee company. It said that if you enroll with them prior to the birth of your child, they will give you an extra set of Dydees free of charge if you have twins. This sounded like a great deal to us, and so we enrolled. We also enrolled in a Lamaze course because Peggy wanted to have a natural childbirth. I oversaw her breathing and held her hand. She gained a lot of weight, around sixty pounds, and some people asked her if she were having triplets.

The first Macsyma Users' Conference in Berkeley, organized by Richard Fateman, was scheduled for the summer of 1977, about the time Peggy was supposed to deliver. Peggy encouraged me to attend it nevertheless, and I told people that I might have to leave at any moment. Most of the papers were on applications of Macsyma in a variety of fields. We also described some of our latest algorithms and packages. I recall introducing Bill Gosper who was our (and the world's) expert on summation in closed form. Doing summations in closed form is much harder than integration, in my opinion. Gosper learned some of his techniques from reading the manuals left by Ramanujan, the Indian mathematical prodigy; I introduced him as the only living eighteenth-century mathematician. The conference gave us an opportunity to meet many of our users, and gave them a chance to meet some of our team. Ellen Lewis was in many ways the star of our troupe.

Fortunately Peggy did not deliver during the conference, but one night soon after I returned the first signs that birth was imminent sent us to the hospital at 7AM.

When we arrived they tried to induce labor, but were unsuccessful. We spent an anxious day, Peggy in great discomfort, and I in spite of the Lamaze classes feeling completely helpless. In the middle of the night I dropped into an exhausted sleep in a chair by her bed, and that, of course, was when the amniotic sac suddenly broke. The nurses, who didn't see the need to wake me, could not believe that I slept through all the noise around me, and Peggy never forgave me for sleeping during such a critical moment. By the following morning labor still had not begun, and the doctor decided on a caesarian section. An x-ray was taken to determine the position of the babies, and while the doctors were having breakfast, the nurses looked at the result. They promptly told us that we were having triplets, not twins. This was, at that point, shocking news; Peggy and I were stunned. When one of the doctors came and looked at the x-ray he reassured us. There were just twins. Apparently the nurses mistook a baby's behind for a third head.

Although I had expected to be with Peggy at the delivery, I was not allowed to do so once they decided to do a caesarian. I had to stand behind a line on the floor, outside the room. The delivery went very fast once they made the incision. Each boy was cleaned and tested by a team of nurses and a doctor. A few minutes later they were ready to move the boys to the nursery. As they passed by, a nurse asked me to hold each one. I held each infant carefully, as if he were a Torah scroll, and then they put them back in a bassinet.

I had called my parents when Peggy entered the hospital, but forgot to do so in the following thirty hours. My parents were nervous, and they prayed much of the time. They called Peggy's mother, who visited us and saw the boys in the nursery, and thus was able to report to my parents that all was well. The boys became immediate stars in the hospital and other new parents as well as their visitors wanted to see them. Of course we thought they looked wonderful, the handsomest babies in the hospital.

One of our problems now was what were we going to call the boys. I wanted to call the second one David, after King David in the Bible, whose father was also named Jesse, my Hebrew name. David was Jesse's youngest son.

Peggy wanted to call the first one Jesse, a name which she liked, but Jews do not name their children after living relatives. I went to the synagogue in Norwood, and asked the rabbi what to do. I suggested to him that we use Jesse in English, and a different name in Hebrew, and the rabbi seemed to be pleased with such a Solomonic solution. So in Hebrew Jesse is called Shmuel, after my late uncle Samuel.

For Jewish boys the circumcision ceremony—the *bris*—occurs on their eighth day. Peggy was still in the hospital, with the boys, but the hospital allowed us to have the ceremony, even though it was a Seventh-Day Adventist hospital. I was able to find a *mohel* to perform this ceremony. He was a doctor in the Boston area. My parents came from New York for the *bris*, and Peggy's parents came from the next town, Melrose. The *mohel* had all his tools ready, and a couple of nurses brought in the boys and stayed to watch a ceremony they had never seen before. The *mohel* made the cut quickly and placed some wine on the lips of each child in turn. My mother was thrilled to hold her only grandchildren, and Mrs. Garvey did not mind holding them, either. Only later did we find out that someone had called the hospital and threatened to set off a bomb in the maternity ward, because a Jewish ceremony was being held there. When the *mohel* showed up they frisked him, and we were lucky he behaved as properly as he did after such a welcome. We never needed to use the hospital again; it has since gone out of business.

Twins are a lot of work. We were able to find an elderly woman, Ruth Neault, who lived near Norwood, to help Peggy take care of the boys and also to help around the house. Even so, Peggy had a difficult time, since she was trying to nurse both babies. I tried to help in any way I could, but Peggy and Mrs. Neault organized things between them, and I had to get back to teaching soon after the boys came home.

My promotion case to full professor was winding its way during the previous academic year. Fred Hennie, by then the Department's Executive Officer, called me while I was at a conference the previous fall to verify some pieces of information in my vita. I had to compose yet another essay describing my plans for the future. One senior EECS faculty member came by LCS to get a feel for our work. Other than that I had heard nothing until Corby told me that I had been promoted. It took ten years from the time I was appointed assistant professor, which is about average at MIT. On the other hand, I was only thirty-five years old, quite a bit younger than most newly promoted full professors in the department.

Chapter 8: Up the Administrative Staircase, 1978–1989

At the beginning of 1978, to nearly everyone's surprise, Bill Davenport resigned as department head, effective the following September, and his associate heads, Corby and Paul Penfield, resigned along with him. A search committee was quickly appointed, with Millie Dresselhaus its chair and Adler, Haus, Barbara Liskov of CS and me as some of the other members. Millie suggested that we find a department head who could use a different approach from Davenport's in securing resources, such as faculty positions. We had been following the typical School of Engineering pattern under Dean Keil of justifying requests for new faculty and additional space and equipment on the basis of increasing enrollments, whereas the science departments based their requests on exciting research ideas. President Jerry Wiesner was eager to fund promising science projects; Chancellor Paul Gray, whose responsibility was to keep the budget under control, was likely not thrilled with Jerry's approaches to spending.

Wiesner met with the search committee, and I was puzzled when he started by saying, "Ah, EECS, the paranoid department." I later learned that he and Bill Davenport had a disagreement, presumably about resources, which led to Bill's resignation.

The committee did the usual interviewing of all the department faculty members. We eventually had two candidates for the position: Mike Dertouzos, not a surprise candidate; and Gerald Wilson, whom few outside of his lab knew very well. Mike came across as someone who knew the people at the top and could get things done; Gerry's strongest suite was the set of letters from faculty members who knew him best. They felt that he had the energy and vision to lead the department, and strongly endorsed him. We presented this list to the dean. It was Mike who was asked to become head of the department, and this troubled him greatly. He liked the flexibility of being a lab director, and felt that the position of department head was quite constraining. He looked into the finances of the department, and found out that all but a few hundred thousand dollars were committed in the beginning of each year. Several times he expressed his concerns to me, about who would lead LCS and who would become associate head for computer science and engineering. He considered asking me to run the lab and be associate head at the same time, but I refused to tackle two full-time jobs simultaneously. He even came out to Norwood to discuss the issue with Peggy and me. Eventually Mike turned the job down, and it was offered to Gerry Wilson. I now waited for Gerry to call me, as he soon did, asking to see me in my office. He noted that although he knew very few on the CS side, he remembered me from the Advisory Committee days, and felt that I could do a good job. I told him that I knew all my colleagues in CS, and that there was, unfortunately, no other young faculty member who would do the job. I also told him of the discussions with Dertouzos. I am not sure I convinced him that there were no other young candidates for the post, but he left with my acceptance of the job. In retrospect I should have realized that the lack of competition for the post would not change quickly, and would cause difficulties in finding management talent among the young CS faculty in the coming years.

I thus became the second-youngest head or associate head in the history of the department; Peter Elias had been a few months younger than I when in 1960 he became department head at the age of 36. Dick Adler was to be the other associate head under Gerry, and I knew him also from the Advisory Committee. Dick was older than either of us. He was one of the "young Turks" who revolutionized the teaching of EE, making it into an engineering science education, in the 1950s. In the early 1960s he led the effort to develop educational materials for teaching integrated circuits. Dick also was largely responsible for the decision of the department to get out of research on silicon in the late sixties, a decision that gave Stanford's EE department a decided advantage in the following decade, and a decision that Davenport and Penfield began to reverse with Adler's blessing in the 1976–77 period.

In the spring of 1978 Hal Abelson and I had been discussing the creation of a new introductory CS subject. My idea was to concentrate on data representation, in particular abstract data structures, and to deemphasize control issues in programming. When I became Associate Head, and no longer able to pursue this project, Hal teamed with Gerry Sussman to develop an extremely successful course, Structure and Interpretation of Computer Programs, which is known at MIT by its number, 6.001. The first two chapters of the text are consistent with my theme, the last three are not since they do emphasize control issues. It is hard to argue with success, however. The course is now taken by about half of all MIT undergraduates, and by many in other institutions. My son David took a course using the text at Brandeis.

I had agreed to give a talk in southern France in October 1978. The boys were just over a year old, and we were able to leave them with our favorite baby sitter, Ruth Neault, while Peggy went with me to a resort near Nice. We had a large room and our veranda had a swimming pool seemingly just for us. Peggy and I thoroughly enjoyed this part of France, especially since most of the tourists were gone. The conference was for the Food Marketing Institute, an organization of supermarkets and chains of grocery stores. I gave a talk in which I described the future of microcomputers. At the time the Apple II barely existed, and most people were unaware of the explosion in computer power that would be unleashed as a result of Moore's Law. Moore's law states that every eighteen months to two years chips gain a factor of two in the number of memory bits they can hold, with a concomitant increase in speed and reduction in cost per bit of memory. In my own professional lifetime the cost of a bit of memory has decreased by a factor of over ten million. I am not sure the executives at the conference fully appreciated the implications of the computer revolution yet to hit them. They were pleased, as they should have been, with their creation of the bar-code scheme in supermarkets.

I undertook the position of Associate Head with the understanding that I would not teach regular subjects, but would continue my direction of the Mathlab Group and our Macsyma system. I felt that we needed to make the system widely available, and not just on one computer on the ARPANET, the forerunner of the Internet. Thus I asked our group to begin development of a LISP system that would run on the DEC VAX computer, and that would permit us to port our system to that machine. In response to our earlier request for a personal computer based on the PDP10 architecture, Gordon Bell, DEC's VP of Engineering, introduced Mike Dertouzos and me to the DEC VAX. We were disappointed that DEC did not choose to build a personal version of the PDP10, and Bell was displeased at our initial negative response to his creation. Clearly, the VAX looked like a winner in many situations, although it required the major academic CS centers, which had been using the DEC 10 architecture for many years, to reprogram much of their software. I asked Bell to donate a VAX to us with the notion that we would create a modern LISP system on it. I assigned Jon White the task of designing the compiler for this LISP. While I was concentrating on administrative matters, I kept hearing that things were not going well on the VAX LISP project, which we called NIL. NIL is the name given the empty list in LISP; we had it also stand for New Implementation of LISP. I paid insufficient attention to the warnings about lack of progress, and in the long run the NIL project was true to its name. All was not lost, however. Guy Steele, who had worked for us as an undergraduate at Harvard, made a major effort to standardize LISP, called COMMON LISP. He worked with a sizable group of LISP implementers, including Jon White, and their efforts were blessed by the funding agency ARPA. These standard LISPs became available on the VAX and many other computers in the early 1980s. As a result it was finally possible to run MACSYMA on widely available machines, and eventually on personal computers. There was some loss in performance by using LISP, however, and this became a significant issue in the coming years.

Gerry Wilson took on the position of department head with enormous energy. One of his goals was to build on the recent move by the department back to silicon-based integrated circuits. Dick Adler was assigned this task. We wanted to hire several new faculty members in this area, and to make the risky move of placing an IC fabrication facility on or near the campus. The three of us, like the Davenport administration before us, viewed the Very Large Scale Integration (VLSI) area as one area that would truly unite the department. The EE side was interested in circuit fabrication and design, and the CS side was interested in software for design, as well as certain applications of VLSI, such as chips that could be used in highly parallel computer architectures. MIT's department was one of the few that had an integrated EECS department, and we continually attempted to justify the wisdom of that decision.

I complained to Gerry about a number of things related to computer science and to computation in our undergraduate subjects. The department had just one DEC11 minicomputer at the time devoted to teaching. Even though we also bought time on the Institute's MULTICS computer, the total amount of computing power available to us for teaching was pitiful. That year—1978—Mike Dertouzos chaired an MIT committee that recommended that MIT add half-dozen computers on the scale of a DEC20, the computer on which Macsyma ran in the 1970s. I was on the committee, and wanted one for EECS. MIT's fundraising was largely centralized at the time in the President's Office. Jerry Wiesner gave us his wholehearted support, mailing dozens of letters to various companies asking for support of an EECS Computer Center and following up with phone calls. He raised $1.5M with which we built a computer center with all the required air conditioning. We also bought a DEC20 for this computer center. Although this did not fully solve our educational computing problems, by 1980 we were on our way.

Another issue that I pointed out to Gerry was the need for additional space for the CS faculty housed in Tech Square. Some of the early tenants, such as the CIA and Naval Intelligence, had moved out over time, and MIT leased some of the vacated space for us, but our needs were growing as both LCS and the AI Lab attracted increasing research funding. I wanted a new building for CS to be a major priority for the department. My reasons were not simply the need for space, but the importance of having this group of faculty on the main campus, rather than across the railroad tracks. The culture that was growing at Tech Square, insulated as it was from the culture on campus, concerned me greatly, although I must say that many of my CS faculty colleagues really enjoyed the separation.

I don't recall whether it was the space issue or some other one, but I felt early on that Gerry was not paying enough attention to me and my issues, so I asked him to meet with me to address this concern. Instead of getting angry with me, Gerry respected me more, and we had an excellent working relationship for the next 8-10 years as a result.

Gerry had projects and issues of his own. He wanted to finish the construction of the EECS complex of buildings on the main campus. Buildings 36 and 38 housed EECS headquarters and RLE; initially it was intended that there would be a building between them devoted to education, but Wiesner realized that he could not raise the full funding for the complex, and the education building had been deleted in the early 1970s. A related problem was that the department controlled no room large enough for the department's faculty as a whole to meet. It used to meet in the Bush Room, but control over the room reverted to the Alumni Association when EECS headquarters moved to Building 38. Gerry and the administration felt that we should build a larger building that would include classrooms and a lecture hall. It was estimated that this would cost $4.5M. Gerry talked to one of our most illustrious emeriti, Doc Edgerton, and Doc said in his usual style, "Sure, let's build a Bush Room replacement. But let's do it soon. I'm not getting any younger." Doc pledged $1.5M for the construction. By now Paul Gray was President of MIT, and Paul was going to ask Doc's partners for some of the needed additional funds.

One Monday afternoon in the middle of the annual MIT budget discussions, Paul called a meeting in Gerry's office. Gerry, Dick and I were concerned that such an unusual meeting could signal significant problems with our budget request. Paul walked in with a somber face, opened his briefcase, and with a broad smile pulled out a bottle of champagne. On Friday he had called

Doc's partner, Germeshausen, and he pledged to match Doc's gift. Over the weekend he met with Doc's other partner in the EG&G firm, Herbert Grier, and Grier also pledged $1.5M. So we had met our goal for the education building. Boy, were we happy! The champagne never tasted so good. Actually, Gerry decided a bit later that we needed more laboratory space for CS common core subjects, requiring an additional $500K for an additional floor. Doc was pleased to pledge that as well in order to get the project going.

Doc Edgerton was one of the people who defined MIT to the outside world as well as to itself. He improved flash photography in the early 1930s, developing strobe photography. In fact, his friend and collaborator, Jacques Cousteau, used to call him Papa Flash. Doc was child-like even in his 80s, and children loved him. One day one of our sons, David, who was five years old at the time, wanted to visit MIT and Peggy took him to see Doc's lab. Doc showed them demonstrations of flash photography, and David was quite impressed. A few days later we saw David emptying the shed in our back yard. We asked him why he was doing that. He said, "I want to have a lab just like Doc."

Later when the hole had been excavated for the education center, I was in the department head's office in EECS Headquarters when someone said that Doc was down in the hole and digging a deeper hole himself, in order to place a time capsule in it. I jumped up off my chair and ran downstairs to see this great event. Doc had finished making a place for the time capsule, and placed a number of Coca-Cola bottles in the sand. "Why Coke bottles?" I asked him. He said that when he and Cousteau explored the Aegean Sea they found many amphorae from early Greece, which were used to hold wine or oil. To him amphorae represented Greek civilization and Coke bottles represented American civilization. "A thousand years from now," he said, "that's how people will think of us."

Another project of Gerry's was the celebration of the department's centennial in 1982. Actually, there was no EE Department at that time; what happened in 1882 was that the Physics Department at MIT admitted their first students into an EE undergraduate program. Professor Emeritus Karl Wildes had been asked by Gordon Brown many years earlier to gather material for a history of the department. He had assembled voluminous material, but his writing was rather dry. With the help of the MIT Press, we hired Nilo Lindgren, a professional writer and as I recall an alumnus of the department, to make this material into a readable history. The goal was to finish the history in time for the celebration three years ahead.

Peggy and I began looking for a house of our own when the boys were born, because we knew that as they grew older we would need additional bedrooms. I had not lived in a house, as opposed to an apartment, since my family left our farm in Beit Yitzchak when I was five. It took us nearly two years to find a usable and affordable house, which was in Lexington. My father's financial situation had improved somewhat, largely as a result of the German reparations payments, and he gave us much of the down payment. We moved into the house in June of 1979, when the boys were barely two, and let each choose his bedroom. David worked his way upstairs first, climbing from step to step, and picked the first bedroom. It didn't concern him then that this was the smallest bedroom, but it would become an issue later. My parents visited the new house and liked it very much. They regretted not buying a house in New York when they had a chance, but felt that they were now too old (my father was 73) to become homeowners, especially since this would likely force them to buy a car. My father had let his driver's license expire, and my mother never learned to drive.

Early in November 1979 I received a call from my mother that my father was in the critical-care unit of a local hospital in Brooklyn. He had had a massive heart attack after eating dinner that evening. I took the next available flight to New York. By the time I arrived at the hospital my father's brain waves were flat, and it was just a matter of time before he was declared dead. My mother was distraught. She blamed herself for making a meal that was too rich; she blamed the 911 system for not sending anyone for over ten minutes; most of all she blamed the care my father received at this local hospital. She may have had a point in her concerns about the initial care my father received in the hospital, but we did not pursue the matter, except to find out that the doctor who initially treated my father was no longer working there a short time later.

The burial service was conducted by the rabbi and cantor of my parent's synagogue in Brooklyn. Many people who lived in their apartment building on Ocean Avenue attended. I recall one of them telling me how impressed he was with my father's bearing when he walked with Abe or with their dog, Charlie. My father was buried in the plot that he and my mother had previously chosen at Mount Lebanon Cemetery, the cemetery where my grandmother and uncle were buried. After the ceremony the family gathered in my parents' apartment. My aunt Frieda brought her children, Ruth and Robert. Ruth had become a model and later worked on the advertising side of major women's magazines. Robert worked for a company associated with his father's firm in Peru. My father's cousin, Mrs. Brieger, and her family were also there.

Jewish tradition is that the family sits for seven days of mourning, a period called Shivah in Hebrew, during which people visit and bring food. The mourning is interrupted by the Sabbath, when everyone is expected to go to the synagogue. My mother, Abe, and I sat shivah in Brooklyn; Peggy had to go back to take care of the boys. Another tradition is that the kaddish prayer is to be said every day for eleven months, usually until the gravestone is erected. I did this religiously, getting up to attend the 7AM prayer service each weekday at our synagogue in Lexington, and the somewhat later ones on Saturday. I had not gone very often before my father's death, but now not only did I go, but I would also often lead the prayers. I did not realize how much my father's death would affect me, and I am grateful for earlier generations having devised the traditions that help in the grieving process. At the end of the eleven-month period we had a small ceremony at the graveside for the laying of the stone.

My father was not the only one who died in that academic year. So did Bill Martin, my closest academic colleague. Bill had pulled me out of a meeting in September 1979 and told me that he had been diagnosed with cancer of the colon, and that he would begin chemotherapy soon. After the chemo treatment Bill seemed to be in remission, and he subsequently returned to work. But by the next spring the cancer recurred, and did so with a vengeance. He was dead soon after that. Susan and Bill's friends prepared a beautiful ceremony in Cambridge's Mount Auburn Cemetery. My only part was pouring some sand into the grave. Although we had not worked together for nearly a decade at the time of his death, I have sorely missed Bill's presence and advice over the years.

Dick Adler's efforts to rejuvenate the department's activities in integrated circuits involved many people in one way or another. In addition to hiring several new faculty members in the areas of fabrication and design of ICs, we very much wanted to have an IC fabrication facility on campus. Stanford, our main competitor, had such a facility, and it was very expensive to replicate and maintain. Since these facilities use poisonous chemicals, there was some concern of having one on the campus. The cost of the creating such a facility was in the many millions of dollars, partly because of the need for highly air conditioned rooms. We simply did not have that kind of money. The equipment would cost additional millions, although some of it could come from industry since we would lag industrial IC production standards by at least one generation. Finally, the annual maintenance and operation cost would be significant and could not be readily borne by the research projects in the facility. A 'minor' additional problem was that we did not have space for such a facility and its ancillary laboratories and offices, even if we could solve the other problems.

Paul Gray became MIT's president in 1980 and helped us a great deal in the IC area. Paul's research area was related to ICs, and he agreed with us that a modern EECS department that wanted to remain at the forefront of the field had to have an IC fabrication facility. Thus we were able to get the whole of Building 39 that housed the central MIT computer center for our IC facility. Jim Bruce had become director of the MIT information system organization, and he planned on moving the big mainframes to an old wind tunnel near our athletic facilities. We did vibration analyses on Building 39 and determined that it could indeed house such an IC facility. Dick began the process of negotiating with DoD's ARPA to allow us to fund the renovation and startup costs through an increase in MIT's overhead rate. Bob Kahn, then director of the ARPA office in charge of IC related work, was very much in favor of this idea. It did not hurt that he was a former MIT faculty member. Dick Adler and Paul Penfield made many visits to companies in the IC business or related businesses in order to enlist them in the Microsystems Industrial Group (MIG). This MIT organization was to help us with funds for the maintenance and operation. Industry would help oversee the research activities, and could donate old equipment to the center. MIG had a counterpart organization at Stanford that had similar goals. Getting a company to join the MIG was a struggle in a number of cases, but eventually we enlisted enough firms that the maintenance and operating costs for a facility could be assured.

Paul Penfield also played the key role in obtaining a significant research contract from ARPA in the general area of VLSI design. He did this with a novel organizational approach, later called a virtual center. The funds when they came in each year were sent to a number of different laboratories and centers, such as the Laboratory for Computer Science and the Research Laboratory of Electronics. This is contrast to other major contracts that remain in a given laboratory, and incur its overhead. The virtual-center concept for the VLSI contract made it easier for faculty, students, and staff in a variety of laboratories to collaborate on a single major contract. I used the concept several times when I became Dean of Engineering.

The dean of engineering, Alfred Keil, resigned a few months before Bill Davenport. The difficulty that Dean Keil had in obtaining resources for the school, and the imminent change of president for MIT, made the position of dean of engineering somewhat unattractive in 1978. There was a long search for a new dean while Jim Bruce held the fort as acting dean. Finally, the administration convinced Bob Seamans to become dean, which he apparently agreed to do for a short time only. Seamans, the second in command at NASA during the Apollo project and a perfect gentleman, was also a former president of the National Academy of Engineering. He then returned to his home department, Aeronautics and Astronautics, before becoming dean. He did not wish to start many new projects during his anticipated short tenure, although he was interested in increasing the number of faculty in the area of engineering and public policy. This seems like an occupational hazard of deans: nearly every dean since Gordon Brown has been interested in policy issues, more so than the majority of the engineering faculty. This hazard would affect me more than I could have imagined at the time.

Bob Seamans announced his intention to resign as dean as of September 1981. A search committee was formed and I testified before them, strongly urging them to recommend Gerry Wilson as the new dean, although I noted that the impact on EECS would be non-trivial. I believe they recommended Wilson and Herb Richardson, head of the Mechanical Engineering Department, probably in that order. Gerry accepted the deanship, and now we had a problem in EECS. I felt that the key issue for EECS was the need to complete the agenda that Gerry had begun, and if someone was chosen who could do that I was willing to remain as associate head. Hermann Haus chaired the search committee for the EECS head, and they interviewed the entire faculty. One day Seamans called me and showed me the key paragraph in the search committee report. It said that either I would accept the position as head of the department or the committee should be asked to reconvene and continue the search. There were no other names on the list. I was flattered and surprised, and I accepted the position. I did surprise

Seamans by asking for an increase in my salary, but he agreed to the request a few hours later.

I now needed to complete the team. Dick Adler agreed to continue as associate head. I was very pleased with that decision, and I knew the search committee would be, too. I now had to find someone to become associate head on the CS side, no mean feat, as it turns out. First I had to meet with the search committee so that they could give me their conclusions and advice. One committee member acknowledged that he did not vote for me, and I appreciated his candor. I don't recall the advice they gave me as a committee, but I do remember Haus's advice. Asking me to his office for a personal meeting, he told me, "Get a haircut!" At the time I had a beard as well as long hair; I did trim the hair. I just wished he had said instead, "Lose weight!" That would have made a bigger difference in my life.

I was to start as department head on September 1. This was not the first time that September 1 played an important role for me. I came to the United States on September 1, I had my doctoral examination on that date, and I became associate head on that day as well. Later I would become Institute Professor at MIT on September 1. Of course, September 1 is the beginning of the month that starts the academic year. My guess as to why so many administrative changes at MIT occur on September 1 is that it was financially advantageous for academic administrators to resign as of August 31, since MIT offered 'combat pay' of one extra month to administrators who complete the summer months.

Until I had accepted the position as head of EECS it did not strike me how much I had changed from the relatively poor immigrant boy of 27 years earlier. The responsibility of heading such a prestigious department in such an important institution finally hit me. But I still had to complete the task of getting an associate head for CS. I knew that the young CS faculty would not be interested, especially since I was the first CS faculty member to run the entire department. I asked a number of people, and finally Peter Elias agreed to do it for a while, a gracious gesture. Peter had been department head in 1960–1966. He tended to look at issues calmly, and from an optimistic perspective. The four of us, Peter, Dick, Fred Hennie, and myself, worked very well as a team. We would meet for three hours each week to deal with the issues we faced. In addition, we continued the habit started under Gerry Wilson of meeting for three days during the summer to plan the next year. I maintained a list of issues (usually around two dozen), and by the end of those three days we usually had discussed nearly twice as many. The first meeting with Gerry took place in Vermont, near Adler's winter home in Killington. From that point on we called the meeting the Vermont meeting, although it took place once at Martha's Vineyard near Adler's summer home, and once on Prince Edward Island. The 1981 meeting took place at MIT. This one was memorable mainly because I accidentally let Adler's dog out of the room. She was in heat then, and pregnant by the time we finally found her, it would turn out later.

One of the issues that was always on our minds at the Vermont meeting was the composition of the department's Personnel Committee, which reviewed all appointment and promotion cases. We would discuss at the Vermont meeting the areas in which we needed to do faculty searches, and present our findings to the Personnel Committee at one of its first meetings of the academic year. Much of the time of the Personnel Committee is spent reading letters of recommendation on faculty being considered for promotion or tenure; since letter writers know that under certain unusual circumstances, such as a lawsuit, these letters may become known to the professor being recommended, there is a tendency nowadays to write relatively positive letters. I became proficient at reading between the lines, to discover what the letter writer really meant. In fact, Wilson accused me of reading so much between the lines that I neglected to read the lines. While this was an exaggeration, there was an element of truth behind his remark.

MIT departments are run by department heads, not department chairmen, a distinction that gives the department head greater authority than a chairman, the usual departmental leader in most universities. On the other hand, the Visiting Committee system at MIT provides at least one mechanism for the faculty to indicate their concerns with a head who is abusing his or her authority. I felt that my greatest authority was in the choice of areas in which to search for new faculty. Young faculty members are the lifeblood of any department. While the number of hires permitted us by the dean would rarely exceed five percent of the total faculty size, a department head can appoint quite a few faculty members during his or her term, and thus greatly influence the department's direction for many years thereafter**.** As I rose in the MIT administration I have increasingly felt that this power of department heads may be too great, because they tend to optimize hires for the needs of their department and not for the larger needs of the Institute. This is more problematic today, when interdisciplinary research and education is increasingly important. I unfortunately did not realize this point at the time, yet I think we made outstanding appointments during my tenure as department head. At MIT promotions recommended by the department are carefully reviewed by school councils, chaired by the dean, and then by the Academic Council, chaired by the president. Appointments of new assistant professors, however, are usually just reviewed by the dean, and approved based largely on the very extensive national searches done by each department. While we had clearly identified needs in several areas, often related to VLSI, we also wanted to appoint new faculty who were outstanding in areas other than the ones of previously identified need. Thus we continued Gerry Wilson's practice of conducting a General Search. Some of the EECS faculty leaders of today came out of that general search.

One of the most difficult jobs that a department head has, at MIT and elsewhere, is telling a young faculty member that he or she has been denied tenure. The tenure process involves multiple committees, and can take nearly a year from start to finish. Many businessmen tell us that a basic problem with universities is tenure, that "dead wood" should be let go quickly; however, there are some built-in safeguards. First, peer pressure on faculty keeps the dead wood to a minimum. On the other hand, even one very low performer can create morale problems in a department. What I liked to point out to such businessmen is the very large number of American firms that have a process akin to tenure, and are clearly not universities. What I am thinking of are large partnerships, such as accounting or law firms. In these firms the granting of tenure is called "making partner." I liked to think of MIT as a very large partnership of the professors. I think the overall probability at MIT for an assistant professor's obtaining tenure is about one third. The figure varies from school to school at MIT and is somewhat higher than one third in the School of Engineering. I used to ask young faculty what they thought the figure was, and they often replied that it was about one in eight. Such erroneous guesses unfortunately add to the tension of the pre-tenure years. Overall, I think it is a good idea to have at least one tough decision point in a career. In my opinion, we in EECS have made very few mistakes, possibly once every half a dozen years, either in the granting of tenure or in denying it to a worthy individual. I used to make a pun saying that *tenure* ought to be replaced with renewable *ten-year* contracts. This may still be a wise course in the long run, but I think that other institutions will have to adopt significant changes to the tenure process before MIT would make any such changes.

Another practice that MIT's engineering departments followed and that I had to defend over the years was the practice of hiring some of our own Ph.D. candidates as junior faculty. Many universities have rules against the immediate hiring of one's own. One reason why the practice was common, especially in the 1950s and 1960s, was that MIT was quite a bit ahead of the other schools in the engineering science approach, and thus many of the best candidates in the nation graduated from here. In recent decades the practice of hiring one's own at MIT has greatly diminished, largely because excellent candidates are graduating from other schools.

The Dean of Engineering works closely with the Engineering Council, which was made up of department heads, laboratory directors, and associate and assistant deans of the school. Gerry, when he became dean, moved several laboratories into the school that had previously reported to the Provost. They were mostly EECS labs; in fact we used to joke that the EECS was an inter-laboratory department, since almost all its faculty resided in one interdepartmental laboratory or another. While I applauded this move as giving greater visibility to these labs, it unfortunately put some of our promotion and appointment cases in double jeopardy. Many of the EECS laboratory directors in Engineering Council were also on the EECS Personnel Committee, where they voted on cases; when they lost, they could reargue their position in Engineering Council. The case that brought the issue to a head was that of a woman whom I tried to hire. I complained to Gerry about this, and he eventually restructured the Council into two parts: one had only department heads and deans and dealt with personnel issues, and the other included the laboratory directors as well and dealt with all other matters. Gerry gave the lab directors the opportunity to argue special cases with the department heads, but I do not recall that they ever did once the split was made.

As department head I had even less time to devote to the Macsyma project than as associate head. I thought that I had prepared the project for such an eventuality by creating the NIL effort that would allow us to run the system on widely available computers. Although NIL was not successful, other approaches made LISP available on a VAX, and thus Macsyma could run on it. The next step was to move the project out of MIT and operate it as a business. I felt that a for-profit company was needed in order to continue to maintain and improve the system and get it to work on an ever larger range of computing platforms. The research on algorithms could continue at MIT and at other places, but continued maintenance, support, and development was not something that research universities ought to do beyond a point, and that point had certainly arrived for Macsyma.

The MIT Licensing Office had me meet with several venture capitalists. Each pointed out that MIT had to grant a start-up an exclusive license to the software, otherwise the venture might have too much competition from others with access to our system. The need for an exclusive license was great because we had spent so much time debugging and improving the system, so that it would be relatively easy for others to obtain a non-exclusive copy and make it readily available to others. The discussion over the exclusive license ran into several difficulties. For one, some of my former students, in particular Richard Fateman of Berkeley, felt that the system was not MIT's to give away. Berkeley's EECS faculty tended to make their software public-domain, and he wanted us to do the same. I disagreed, feeling that a public-domain system would not attract the resources to allow high-quality maintenance and evolution of our facilities. Discussion of our plans with our sponsors led the Department of Energy, largely at the instigation of some of our users at the DOE national laboratories, to claim that the software belongs to the Federal Government. MIT was prepared to argue otherwise, largely using the recently passed Bayh-Dole act that gave universities the right to license government-sponsored research.

In spite of these difficulties, we went ahead with a business plan, and we even interviewed potential presidents of a "Macsyma Inc." In my discussions with the Licensing Office I was led to believe that MIT would grant us an exclusive license. The complaints by our sponsors, I think, led MIT to convene a special meeting of the Research Committee chaired by President Gray. In an initial discussion of the committee from which Gray was unfortunately absent, the group leaned toward giving us an exclusive license; a subsequent meeting chaired by Gray, from which I was absent, led to a rather different conclusion. Gray was concerned that it would look bad, from a conflict of interest point of view, if MIT granted any of its faculty or staff the right to commercialize the products of their research. Recall that this was in the early days of Bayh-Dole act that gave universities the right to commercialize government-funded efforts, and MIT had relatively little experience with setting up new start-up firms. The Research Committee came up with a compromise. They would let the Arthur D.

Little Company decide who would get the license, in return for half the income. I was extremely unhappy with this outcome, and even considered suing MIT. I felt, however, that I owed too much to the department to do so just after I was appointed to head it.

Since we had little choice, we met with A.D. Little's vice president whose job it was to commercialize their own technology. He was looking for a firm with relatively deep pockets that could guarantee a stream of income regardless of how well the Macsyma system did in its early years. This led him to give the exclusive license to Symbolics, Inc., a firm created by former MIT AI Laboratory staff members to commercialize LISP machine hardware. I was certain that Symbolics was a poor choice: their main goal was to sell their specialized machines, and they would regard Macsyma running on a VAX as a competitor. I did not realize then that in the attempt to avoid conflict of interest at MIT, the administration permitted A.D. Little to have more than a bit of conflict of interest on their own: A.D. Little's pension fund, the Memorial Drive Trust, was a major investor in Symbolics!

I was right about Symbolics's conflict of interest relative to Macsyma on a VAX. Although they hired most of the staff who wanted to continue working on the system, they did not support its continued development on other platforms to the extent that was needed, and Macsyma was superseded by other systems, such as Mathematica, within the decade. Symbolics did pay out about $1 million in royalties, half to A.D. Little and half to MIT. The developers of the system, myself included, obtained about 40 percent of MIT's share, which was placed in an MIT fund for support of research on large-scale systems. The Department of Energy later got MIT to give them a copy of the system by threatening to stop all DOE support at MIT, which they then placed in the public domain, and this was used as an excuse by Symbolics for their inability to sell enough copies and adequately support the system's development. Symbolics itself went under as a result of the decline of commercial interest in AI following the expert-system debacle. Just before its demise it sold its rights to Macsyma to a company called, of all things, Macsyma, Inc. That company lasted about a decade before it sold the rights to a private company with connections to the National Security Agency. All in all, the effort to commercialize Macsyma, and MIT's role in it, was one of the most discouraging episodes of my career.

Ray Stata, co-founder and CEO of Analog Devices, Inc., a local firm that made analog chips, came to see me one day in the fall of 1981. I knew little about analog chips at the time, but Ray was interested in me because his firm was getting increasingly involved in software issues. Bob Fano had apparently recommended me to Ray, and Ray asked me to be on his board of directors. I thought about this for some time, and finally agreed to join the board. It was one of my best decisions. I learned a great deal about business issues, and also about technology issues of which I was not aware. Ray helped the department enormously, as a member of our visiting committee and later its chairman. He and his wife, Maria, contributed a chair to the department when we had an enrollment crisis; later they contributed $25 million toward a building complex, the Ray and Maria Stata Center, that would house the computer science faculty as well as others. My stock holdings and options in Analog Devices helped pay for my children's college expenses and much else. When I joined the board the company had just finished a year in which they did $176 million of business; I left the board twenty-one years later, when their annual volume was more than ten times that. I wish I could say that I was a prime cause for this expansion, but I was clearly not. Ray and his successor as CEO, Jerry Fishman, have been tremendous leaders in the semiconductor field.

The Athena Project

In my day job of running the department, one of the issues with which I was still concerned was that of educational computing. Gerry wanted to have a committee from the Engineering Council make a proposal in time for the Five Year Plan

Retreat for the School of Engineering in the fall of 1982. He knew that Mike Dertouzos, AI Lab Director Pat Winston, and I were all strong personalities. So he created a committee of the three of us, without picking a chairman. As a result the committee never met, and as the summer was ending I was getting concerned that we would have nothing to discuss at the retreat. I drafted some talking points indicating a need for one to two orders of magnitude greater computing power available to our students. I pointed out the value of numerical methods and graphics in engineering. I also noted that symbolic computing had a potential role, which could only be addressed when we had much more computing power. When Gerry announced the schedule for the planning meeting in October 1982 he had left out any discussion of educational computing. I urged him to give me a slot, so he created a new slot, from 8 to 8:30 on the second evening of the meeting that was to be held in Cape Cod.

I began the discussion that evening by summarizing the points mentioned above. I was startled by the response. Everybody wanted to talk, and all were in favor of a major project in educational computing. Gerry had to stop the discussion at 11PM, two and a half hours after it was due to end. The timing of the discussion could not have been better: a week earlier IBM and Carnegie-Mellon University had announced a major educational computing project in which CMU would build an operating system for IBM's future offerings in high-performance personal computers. Our engineering school's department heads and laboratory directors, having had a week to digest this development, were full of ideas. In addition, we guessed that the Digital Equipment Corporation, which had lost out in the bidding at CMU, was still looking for a partner for developing software for their versions of future high-performance personal computers. Sure enough, about a week after the Cape meeting Gordon Bell called Mike Dertouzos to discuss a possible DEC–MIT project.

We held a dinner meeting in late October at an inn in Concord, halfway between MIT and DEC's headquarters in Maynard, Massachusetts, with Gerry, Mike, Corby and me for MIT, and Gordon Bell leading a similar number from DEC. After the meeting Mike took me aside and said that the two of us would have to draft a document indicating the needs of the School of Engineering, otherwise nothing much would happen. We did just that in the next few days. We wanted pretty much the same things that CMU wanted—a megabyte of memory per computer, a processor speed of a million instructions per second, and a display with a million pixels. I think we asked for a thousand of these machines eventually. We realized that DEC did not have these new computers available, although they promised they'd have them in a couple of years. In the meantime they'd give us dozens of VAX computers of the kind that Bell had discussed with us in 1978. Mike sent the memo to Bell in early November of 1982. Due to the tremendous increase in computing power in the past two decades, our request may appear puny in current terms, but rest assured that it was not so in 1982. The hardware and software we requested in the memo were sure to transform the educational computing scene at any university in the country.

I don't recall the exact sequence of events, but there was a feeling that we should not limit the project to just DEC, although its chairman, Ken Olsen, was an alumnus and a great friend of MIT. Mike sent the bidding document to IBM and other firms. We soon heard that IBM was interested in participating with MIT as well as CMU. IBM had been a good friend of MIT going back to the early days of Lincoln Lab, and Paul Gray felt that we had to consider them as a partner. Corby, Gerry, Mike, Jerry Saltzer, and I went to Armonk, IBM's headquarters, in December. IBM was particularly interested in the concept of coherence that I was advocating for the project. That is, that we should develop software that runs on multiple hardware platforms, and even uses multiple programming languages, and that can communicate information readily over a network. The president of IBM pointed out that they were having difficulties with multiple platforms at IBM and he was skeptical that we could solve the problem. I was optimistic because I felt that the UNIX operating system could be ported from one hardware system to another. UNIX, developed at Bell Labs and available under license to educational institutions, was an operating system that was based in part on concepts derived from MULTICS. I was less sure that we could get languages, such as C and LISP, to communicate together, but then we would have no project if we knew everything beforehand. I was also optimistic that the MIT faculty and staff could make great strides in educational computing, because I felt that the lack of hardware was what had been holding it back. In retrospect, I was largely right about UNIX, but too optimistic about progress in educational computing.

My prior experience with educational computing suggested that Macsyma and related algebraic systems could certainly be used if their large memory requirement could be provided for. In 1968, at the instigation of Bob Fano, I had written a program called SARGE, to help tutor freshman in calculus integration problems. Its concept was that the students would be allowed to take any steps in the solution of a problem they wished, as long as they informed the program what steps they were taking. That is, the program behaved the opposite of a drill sergeant. For example, if the students were making a trigonometric substitution or expanding the integral of a sum into the sum of integrals, they would tell the program that they were doing so, and indicate the result of that step. SARGE would then check the correctness of the result, and possibly keep the information to itself. It could prompt a student who was lost to attempt to pursue a previous line of exploration. SARGE took me less than a week to develop, and I was confident that similar intelligent tutors could be written with little effort. However, access to such programs was denied to most of the students by the lack of computing resources.

Once we found out that IBM was interested in participating in the project we realized that we had a problem. Would we offend one friend, or the other? All of us felt that DEC had the better offer for the School of Engineering's needs; Paul Gray felt that we should work with IBM, if at all possible, and I agreed, partly because of my desire to explore the coherence issue, and thus the need for multiple hardware platforms. I suggested to Gerry that we use DEC's equipment and support in the School of Engineering, and use IBM's in the freshman year and in the other four schools. Gerry was reluctant to make such a complex deal, but eventually agreed to this apportionment of the equipment.

While negotiations with the vendors were going on, Gerry convened a committee of faculty to discuss the project, and he asked the committee members to keep it confidential—a difficult thing to ask for in a university setting, and one which led to the later feeling among some of those not included in the early discussions that they were second-class citizens. We needed a name for the project, and many were proposed. I liked Leonardo; Dertouzos championed Athena. Someone objected to Leonardo on the grounds that Leonardo da Vinci was gay; Athena won because she was supposedly the goddess of wisdom—I researched this derivation and found out that initially Athena was the goddess of womanly wiles in war. Over time "wiles" was transformed to "wisdom," I suppose.

Gerry also decided that we needed to raise money for internal research on educational computing. The goal was $20 million for this effort, of which we raised about two-thirds. I estimate that the initial five-year phase of Project Athena brought in $70 million from DEC, IBM, and other donors. The eventual eight-year effort probably brought in about $100 million in equipment, services, and donations toward staff support and faculty-led projects.

One signal that all would not go well with Athena as an MIT-wide project came when Dertouzos and I visited the MIT Science Council. Our reception was chilly. Department heads in the School of Science apparently did not feel that computers would help in their classes. Some of the science faculty who were interested in using computation in their subjects also demanded to be given time off their regular teaching, which our budget could not easily afford. Initially we probably made a mistake in making the pronouncement that we wanted to "let a thousand flowers bloom." Eventually the Athena committees in charge of funding faculty projects emphasized a few "flagship" projects as needing significant support.

From the earliest days it was clear that certain uses of Athena would succeed in an educational setting. These were applications that used numerical computation to solve problems or emphasized graphical displays of solutions, most of which fit in well in engineering subjects. I felt that we would know Athena was successful if we were surprised by some of the applications. It turned out that our surprises were largely in the humanities. One such project used multiple films, stored on a computer disk, of some of Shakespeare's plays, allowing the student to compare how different actors portrayed the same character in a given scene. Other projects were in foreign language courses. Students could watch and listen to a scene in French, and then respond to it with sentences typed in French. We wanted initially to use artificial intelligence techniques to understand the student's response, but this proved too difficult for our AI programs, so the project had to be simplified. A number of the applications of Athena won national awards. Yet our assumption that most of the educational software that we would use in Athena would be written by our faculty and staff proved incorrect. Educational software continued to be difficult to develop, and our faculty did not have sufficient time and interest in creating the bulk of the educational software that our courses could use. Eventually certain standard packages, such as the matrix-manipulation package Matlab, became workhorse applications in our subjects.

Gerry Wilson was adamant that Athena be used almost entirely for course-related work, and not for games or word processing or even e-mail. But the culture that developed around PCs and networks was as difficult to change as most of us had predicted: word processing and e-mail quickly became ubiquitous on the Athena network. Before we had the Athena hardware and software infusion, our students complained about the lack of computing in such a technology-centered institution; Athena turned MIT into one of the most computer-rich institutions in the country. Our initial plan was to eventually get students to buy an Athena-compatible computer as freshmen and take it with them when they graduated, thus renewing the hardware/software base regularly. We have not implemented this plan, and students seem to get enough computer access from the nearly one thousand Athena machines available on campus. Of course, many students bring their own computers. Yet Athena's computers may be needed in courses that require special graphics, for example.

Gerry also did not want us to do significant system development for Athena. I had hoped that UNIX would fill our needs, but I overestimated its capability in a large and complex environment such as Athena. We were saved by Jerry Saltzer, who became the technical director of the project. Under Jerry's supervision we introduced many improvements to UNIX that have become standards in the UNIX world. One was Kerberos, the software for authenticating users in the network. A key related addition was X-Windows, initiated by a graduate student in LCS, and championed by a staff member from DEC, and approved by me for continuing development by Athena. X-Windows permitted UNIX systems to emulate the windowing capability of a MAC computer. DEC felt that X-Windows, which became DEC Windows in their environment, justified their entire investment in Athena. Unfortunately, IBM did not make such an extensive use of Athena's capabilities. I should also note that leadership of the overall project passed to Professor Steven Lerman and later to Professor Earll Murman, who made the project into the success that it became.

My key disappointment with the systems aspects of Athena was that the staff gave up on the coherence goal relatively quickly. Coherence implied interoperability, which UNIX and C, its underlying language, helped create across multiple hardware platforms. Thus we were close to the goal. Coherence also meant the ease of interconnecting applications over a network. While it is a bit of a stretch, it is imaginable that if we had paid attention to the issue then, MIT rather than CERN would have invented a variant of the HyperText Markup Language, HTML, and the World Wide Web. In recent years the concept of middleware has arisen in computer science—software that resides on top of the basic operating systems and permits multiple platforms, thousands of distributed computers, security, flexibility, and other important properties. Project Athena was one of the first systems to emphasize this middle layer of software components and capabilities. Unfortunately, many operating systems practitioners have an all-encompassing view of operating systems, and thus miss the structure presented by middleware.

Athena's eight-year effort became institutionalized in the early 1990s when Mark Wrighton, who was MIT's provost at the time, created a significant budget line for the staff and the annual hardware and software upgrades. Nor did MIT rely entirely on Athena computers for educational activities. Early in 1982 I had presented the Athena concept to Hewlett-Packard's CEO, but HP was nervous about becoming a major contributor because their hardware was still being tested. Instead they provided the EECS Department with the computers on which to run the Scheme language in our introductory computing course, 6.001, and later for our common core subject in computer architecture, 6.004. Both of the labs for these courses were on the top floor of the new EG&G building. HP was later helpful in providing computers for our common core EE subject in signal processing, 6.003. We were so proud of our overall relationship with HP we later created a mini-symposium in their honor, describing the various activities we had in common.

The EG&G Education Center

While planning was underway for Project Athena, construction of the EG&G Education Center was also going on. I could see the site from my corner office in EECS Headquarters, and saw two events that delayed completion of the building, one of which could have been tragic. One morning shortly after Doc Edgerton had buried his Coca-Cola bottles, I believe it was in July 1982, there was a major downpour in Boston. There was so much rain that the Charles River locks were closed. The lowest point of the storm-drain system under the campus happened to be the excavation for the new building. In a period of about fifteen minutes, under my astonished gaze, the entire hole was filled with water. It took about a month for it to evaporate or seep underground.

My sons enjoyed watching the Micmac Indians put the steel girders in place. These men are proud of their skill and apparently have no fear of heights. Some months later the construction crew began pouring the concrete slabs between the floors. Early one morning as I watched, they began pouring the fourth-floor slab. They had previously built a matrix of steel wires between floors, and were now pouring concrete onto it. All was going well until suddenly part of the matrix gave way, and the concrete fell from the bottom of the fourth floor through the third, second, and first floors. The workers all leaped and scrambled out of the way, and no one was injured. I saw some of the workers in the elevator later that day, and asked how they were. They were clearly shaken by the experience and asked that I pray for them. Apparently the steel matrix was weakened the previous day when one of the workers walked on it, and this led to its failure.

These incidents clearly delayed construction, but the building was ready for the first day of classes in 1983. Doc Edgerton was also delighted to give an open lecture that evening. In those years a student group would show an X-rated film during the evening of the first day of classes; the films were well attended, but later, as the number of women on campus increased, they were thankfully able to end that custom. The practice was still going on in 1983, and Doc had much competition for his lecture. Nevertheless, Edgerton Hall, on the first floor of the new building, was full for Doc's slides on flash photography. Doc put on a great show for the audience.

Later that academic year we had an official celebration for the building. The three partners plus the CEO of the EG&G Corporation were speakers, both at a formal event in the building and at a dinner at the Faculty Club. Photographs of the three donors and their wives were unveiled in three different parts of the building. I was able to tell the donors that each floor was in full operation and all was going very well.

The Centennial of Electrical Engineering Education Celebration

Another of Gerry Wilson's projects we brought to fruition was the celebration of the centennial of the electrical engineering program at MIT, and the accompanying volume on its history. The celebration took place first, over an entire weekend. On Saturday there was a major presentation done by the department on the topic of Life-Long Education, the work of Professors Bruce, Fano, Siebert, and Smullin, who had spent a year analyzing the need for engineers to receive continuing education during their careers. They visited a number of companies and made recommendations for a major increase in the amount of time firms allowed for such studies in order for their engineers to keep up with the changing technology. Some 20,000 copies of their report were eventually distributed.

The first day ended with a dinner for a thousand people at the newly finished Johnson Athletic facility. Doc Edgerton was the master of ceremonies. We had a guest speaker from Cornell, who pointed out that while MIT's EE program was indeed the first in the nation in 1882, Cornell was the first to have EE graduates since they had admitted only juniors, whereas MIT admitted underclassmen.

Doc Edgerton had had a stroke a few months earlier, but carried on gamely. He gave a rendition of songs on his guitar that went well. However, when he started making additional remarks using cue cards, he got confused and began repeating himself as he reused the cards. Priscilla Gray had the presence of mind to take the cards away from him, and he completed his presentation with no more hitches. The star of the dinner, however, was Karl Wildes, by now in his eighties, who was wheeled into the room a bit late. Known to many there because of his long-time involvement with the EE Cooperative program with industry, he was surrounded by friends and former students as he was brought to his table.

On Sunday there was a celebration on the first floor of the newly built Johnson Athletic Facility. There were many booths, and hundreds of balloons in the air. At the center of this huge room was an enormous cake. The many former department heads and associate heads in attendance stood around the cake and cut it in unison. There was so much cake that we later called the dorms and fraternities to come and take wheelbarrows full of cake back to their living quarters.

The volume on the department's history was completed after the celebration, and thus the photo of the cake surrounded by department heads made it into the book. We mailed a copy to each faculty member in the department. The assignment Gordon Brown had given Karl Wildes thirty years earlier had been carried out. I learned a great deal from this book, and I still use some of the stories in it on occasion. One of my favorite stories involved Dugald Caleb Jackson. I am interested in Jackson in part because I later became the D.C. Jackson Professor. Jackson became head of the University of Wisconsin's EE department in 1890, and of MIT's department in 1907. He remained head until 1935—a total of 45 years as a department head, a record that will not be broken until such time that people live much longer than they do now. Jackson went on sabbatical only three times while he was at MIT, each time replacing himself with Vannevar Bush, who later became dean of engineering and as dean chose Jackson's replacement in 1935: Moreland, Jackson's partner in the consulting firm of Jackson and Moreland. This was not Jackson's choice; he had been grooming others for the post. Thus the man he had mentored, at the end of his 28 years at MIT had undermined him! Bush probably made a good decision after all. Moreland became dean of engineering when Bush left for Washington to be on the scene when the Second World War broke out and later headed the national wartime R&D effort.

Twenty years later the EECS Department celebrated its centennial as a separate department at MIT. What we celebrated in the early 1980s was the centennial of the EE program within the Physics Department. Within a decade the EE program had more majors than any other major at MIT, and it and its related departmental programs have continued that tradition to this day. It took another decade for the physicists to throw the electrical engineers out of the Physics Department, and this event was celebrated in 2003.

Gordon Brown used to visit me occasionally in the department headquarters. He would burst into my office: “Moses, what are you doing sitting down? Get up and do something!” Thus I was surprised when he said that he feared Jackson more than anyone. To find out Jackson’s mood on a given day, Brown said that he would toss his hat into Jackson’s office, and if it did not immediately get tossed back, then he would enter the office, retrieve his hat, and discuss his issues for that day.

My sons, Jesse and David, went to preschools in Lexington. They had some difficulty in the first preschool because some of the teachers made fun of the fact that they looked so much alike. So we transferred them to the Waldorf School. They liked the Waldorf School very much, and although they agreed to go to separate private elementary schools, neither wished to leave the Waldorf School. Jesse had described the Waldorf kindergarten as being like a garden, yet he was the one who agreed to go to the Montessori School in Lexington, where he wound up having a very good time. Several other MIT faculty members had their children in Montessori as well. Both David and Jesse stayed in their elementary schools for five years before transferring to separate Lexington Middle Schools.

Right after we moved to Lexington in 1979 Peggy became a real-estate broker. We had searched for a house for nearly two years, so I guess Peggy got to be pretty good at it. She worked in this field for a few years, until the boys and I complained that she was not home enough, and then she stayed at home while the boys were at school. Selling real estate was interesting work, and had its own kinds of problems. Once a couple came into her office and said that they were dissatisfied with their present broker and wanted her to help them buy a house. Their initial broker sued for part of the commission, the case went to the Greater Boston Real Estate Board, and Peggy won. It was a landmark case in the Boston area.

Peggy and I and our sons began to take relatively long summer vacations, usually in the mountains north of Boston. Peggy had suffered a sunstroke during a vacation in Bermuda in the seventies, so for a long time we avoided further vacations in the sun. We rented cottages in Vermont, New Hampshire, and Maine. Once we rented a canoe and paddled the Saco River in New Hampshire. I accidentally grounded the canoe, and then paddled so enthusiastically that I splashed David with each stroke. Peggy and David never let me forget it.

We also liked to spend a few days at the Mount Washington Hotel in Bretton Woods, New Hampshire. This grand old hotel—the site of the historic 1944 conference that created the world monetary system—was nearly impossible to heat, so it was only open summers until the hotel was renovated in the 1990s. It was next to a small river, and Peggy and I once went fishing in it with a guide named Woody. The river had recently been stocked, and the fish were relatively small and inexperienced. We thus had an easy time landing fish. When we brought our batch of fish proudly to the kitchen, they said it was too late in the day to cook them for that night's dinner. Next day we fished again, and this time brought in a batch of fish early enough for the kitchen to make an appetizer of them. Just as they were served I remembered that I had to make a phone call, and by the time I got back the boys had eaten all the fish. We were never that successful again. I guess one has to know when they stock the river.

A couple of years later Peggy and I took a week-long course in fly fishing from a world-famous maker of fishing tackle, the Orvis Company, near their headquarters in Vermont. They had a private pond where we rehearsed our casts: eleven o'clock; one o'clock; cast; repeat. They told us to spend our evenings practicing our technique in the nearby river. Peggy and I tried hard: eleven o'clock; one o'clock; cast; retrieve the fly from a tree; repeat. On the last day the instructors demonstrated their technique in the river, and wouldn't you know it, the fish knew that the instructors were there and got caught again and again.

Enrollment Problems

At EECS in the early 1980s all was not parties and dedications. We had numerous problems, the biggest of them related to enrollments. Since the low point in 1972 enrollments had doubled, from about 180 sophomores to about 360 by the fall of 1983. I predicted that the fall 1984 enrollments would rise to 380, but I was wrong: there were 381 sophomores in EECS that year. The Institute was facing continuing budget pressures throughout the 1970s and early 1980s, so EECS was not permitted to add faculty. Our faculty size had in fact declined about 20 percent from its high point in 1968; moreover, my 1972 prediction of an increased percentage of tenured faculty had come to pass, and the tenured faculty numbered nearly 80 percent of the total EECS faculty. New blood coming into the department was relatively scarce. I did not want to grow the size of the EECS faculty a great deal; it was already larger than many entire schools of engineering. In fact, I used to claim that Cal Tech was just twice the size of EECS. Paul Gray was heard to say that he did not wish to make MIT into the EECS Institute of Technology. On the other hand, the growth in enrollments put great pressure on our faculty. MIT prides itself on having its faculty do most of the undergraduate teaching, including most recitations, but we wanted our faculty to spend about half their time on graduate teaching. This is especially important for new assistant professors who need to build up a coterie of graduate students. To avoid significant growth in the department's faculty we had to reduce enrollments significantly.

Public engineering schools have traditionally capped enrollments. This was apparently not done in private engineering schools since the Depression. The Institute committees that examined the enrollment problem in EECS suggested that EECS offer an exam in the freshman year—when, it will be remembered, MIT does not allow departmental majors—to decide who could major in EECS as sophomores. I opposed this approach because it put all the responsibility on the department. Instead, I argued, MIT ought to use the student's responses on admission to limit the number who are admitted claiming to want to major in EECS. This was opposed by the Institute committees. There was a critical vote at a faculty meeting in the spring of 1984. All the proposals for limiting enrollments lost. Instead we obtained a motion that if enrollments did not go down over the next three years according to a schedule we had drafted, then the issue would be revisited. I was pessimistic that enrollments would go down—they had increased for a dozen years. But in fact, 1984 was a watershed. *TIME* magazine had chosen the computer for its Man of the Year cover in December 1982; the computer and IC industries were gearing up for a huge increase in sales in 1984, which did indeed increase markedly, but not at the 50-percent rate that many had assumed. A recession therefore hit the computer and IC industries. For the computer industry this was the first recession ever. The effect on enrollments was immediate and enrollments declined according to our schedule. Enrollments in computer science declined nationally until the mid 1990s, when the Internet growth led to a spike. The biggest decline in 1984/85 was in women EECS undergraduates, a great shame because we had made notable progress in increasing the number of women CS students at MIT and nationally until 1985.

The debate over enrollments was probably my biggest issue as department head. The overall effect was, I believe, quite positive. The MIT community realized that enrollments in EECS had not only become very high, but that over a third of all MIT sophomores, juniors, and seniors majored in EECS. The department's faculty felt that the department administration and dean were backing them in their time of stress. We were able to obtain some additional positions, but we were not able to fill them all because of the limited supply of high-quality candidates. Some years later I actually returned much of the money associated with the positions we were not able to fill—probably a first at MIT. I was able to develop a strategy whereby each term a seventh of our entire regular faculty, instead of doing front line teaching, were on leaves or sabbaticals, spending full time developing new subjects, or writing textbooks. With Gerry Wilson's help we were able to create a $1 million endowed fund to help fund the writing of textbooks. He convinced a donor, Bernard Gordon, to allow MIT to split his chair funds, which had grown substantially, into three parts, one of which would be the textbook endowment.

Textbook writing became a significant undertaking in the department in the 1980s. EECS was noted for developing a series of texts in the 1940s, and a new series on the engineering-science approach to EE in the 1950s and 1960s. The first had blue covers; the second, green. I thought we needed a new series, largely devoted to the common core subjects. In addition, the CS core subjects had never been developed as texts. We held a competition for a new EECS series among the various publishers. We had two finalists—MIT Press and McGraw-Hill. I liked the Press because they were local and good friends, but they had little experience in the textbook market, whereas McGraw-Hill had a great deal such experience. I was able to get both finalists to negotiate a joint deal. The Press would help with the editing of the texts, and would sell the books by mail in the United States and internationally, McGraw-Hill would sell the books in the series as texts in the United States. I was offered a position as chief editor of the series, but I declined due to the potential conflict of interest with my role as department head. My most obvious contribution was to suggest that the series be in red or maroon. The series was quite successful, I believe, for both publishers. The biggest success in the early years of the series was the text for our introductory CS course, 6.001, *Structure and Interpretation of Computer Programs*, by Abelson and Sussman. Gerry's wife, Julie, is also a co-author of this important and award-winning text; the course is now one of the most popular electives at MIT.

Once the undergraduate enrollments in EECS began to decline, I was able to devote myself to other issues. One was the humanities requirement for MIT undergraduates, in which I had a special interest partly because of my Columbia background. In addition, the discussion of the U.S. weakness in manufacturing at that time led me to believe it would be of great value to have our students gain a better understanding of the culture of the United States and more generally of the western world. In the 1950s MIT students had been required to take a number of broad subjects in the humanities not unlike Columbia's requirements, often labeled Introduction to Western Civilization and the like. By the 1970s many universities had veered away from such requirements and gave students more options in the humanities and social sciences. At MIT this revised requirement, called the Humanities Distribution requirement, required students to choose three subjects from a total of 168. As a result students could graduate with no courses in history, for example, let alone the old Western Civilization courses. I was appointed to the committee that considered changes to this system.

My initial view was that we should revert to a system in which students had no more than ten courses as options, and had to choose three among them. I saw this as a major shift from the old 1950s requirements, which had hardly any choices. I quickly learned that such a proposal had no chance of obtaining the support of the majority of the humanities and social science faculty. There had been a sea change in the education of Ph.D.'s in the humanities: older faculty members were willing and even eager to teach broad-based subjects, but younger faculty members had completed relatively narrower dissertations and expected to teach courses related to them, rather than the Western Civilization and Great Books courses. In addition, the phrase "dead white males" came to represent the movement away from the classical canon, at MIT and elsewhere in the United States.

There were a number of MIT-specific issues as well. The MIT humanities faculty tended not to have graduate programs or funded research programs so that the number of undergraduate students in their classes was a key measure of the health of each faculty, such as literature or music. The fewer the students, the weaker the case for the size of the faculty; it was important to preserve the number of humanities distribution courses in each section of the humanities. The humanities had also undergone a change in expectations. In the 1950s for example, the humanities faculty at MIT was primarily a teaching faculty, but in the 1980s the expectation of publications and research had risen markedly.

The committee that eventually made the recommendation on the change in requirements created five course categories, and students were basically asked to take one course in each of three of them. It was now less likely that a student could graduate without taking at least one course in history. The number of alternatives in the new Humanities, Arts and Social Science requirement was reduced from 168 to about 80, a reduction no doubt, but not nearly as much as I had initially hoped for.

An idea that germinated during these discussions and later became an important feature of MIT's undergraduate education was that of a minor. Relatively few MIT students majored in the humanities, arts, or the social sciences; they tended to take few courses in these fields other than the eight required for graduation. A declared minor comprising six subjects would increase the number of students taking advanced courses in these fields. Because the minor scheme permitted a great deal of double counting of required subjects, it was possible to obtain a minor with just one course above the minimum required of all students for graduation. I felt that these minors were a relatively small step in the direction of increasing our students' understanding of the humanities and social sciences, and argued for dual majors—nine subjects in a field of humanities and social sciences and nine in a field of science and engineering, for example. This proposal failed at the time, but has been discussed once again in recent years, in part because I argued for it during my term as provost. The concept of a minor has flourished, and has been extended to many other fields. In particular, minors in interdisciplinary fields, for example environmental studies and biomedical engineering, have been very important to faculty and students in both areas.

Efforts Toward Collegiality

The Keyser Random Faculty Dinners: I also became interested in furthering collegiality at MIT. I made a proposal to the key faculty committee at

MIT at the time, the Committee on Educational Policy, that we create twenty groups of fifty faculty each, and ask them to meet once a month for dinner, fellowship, and discussion at the Faculty Club, the fifty in each group to be chosen at random. Most on the committee thought this a somewhat wild proposal, but Jay Keyser, associate provost at the time, wanted to experiment with the idea by inviting one randomly chosen group of fifty faculty members to a dinner. A professor of mathematics on the committee, Alar Toomre, volunteered to generate the random numbers for the selection, and I volunteered the Grier Room in the EG&G building for the dinner.

Jay invited me to the first dinner, to which only a dozen other faculty members showed up—a great disappointment. I was particularly dismayed when I realized that most of those who came were educated in Europe, where collegial interaction among faculty is more common than in this country. Undaunted, Jay decided to invite a hundred faculty members next time, calculating that our initial 25-percent attendance rate might have indicated some interest, at least. For this session he managed to get twenty-five faculty members to attend—still just 25 percent. The dinners have now been running for over two decades, and are now known as the Keyser Dinners or the Random faculty Dinners. Jay is the perfect host for them. He now invites 140 faculty members and gets about 40 to attend. Their value is measured not in numbers, but in the quality of the discussions that spring up, and the interactions between people who might never meet otherwise. The faculty is still largely invited at random, and dinners were until recently still held in the Grier Room. They are funded at present by the provost and chancellor. Jay summarizes the discussion to them after the dinner.

The Symposium: Nan Friedlander, Dean of the School of Humanities, Arts, and Social Sciences (HASS) with a joint appointment in civil engineering and economics, wanted the faculty in the humanities to play a more central role in the Institute, and could see the difference between the roles played at MIT by engineering faculty and those in the humanities. She discussed this goal with a group of faculty members, and Bob Jaffe of Physics suggested the creation of a group of 25–30 faculty from throughout the Institute who would meet monthly and discuss each other's research at a series of dinners. Cynthia Wolff, a professor of literature, came to me with the idea, and I gladly agreed to help. Dean Friedlander and EECS paid for the first few dinners, with some initial support from other deans. I invited a number of distinguished engineering faculty members, and Cynthia did the same for the humanities. We managed to get additional faculty from elsewhere in the Institute, especially from the Department of Physics. The dinners were at first held in a lounge near the Dean of Humanities and Social Science's office, and the group came to be called the Symposium; these dinners, too, have continued since 1985. A member of the Symposium makes a presentation after each dinner on a topic related to his or her field, but at a level that can be understood by educated non-professionals in the field. The Symposium is not widely known because membership is by invitation only and relatively few can be added each year, as another cohort of faculty retires from it. Some of the discussions in the early years, when I was a member, were quite exciting, as specialists and non-specialists argued with each other. The Symposium is still in operation over two decades later, partly because I was able to find some funding for it when I was provost.

The Moses Seminar: A third group created in 1985 was a continuing faculty seminar that initially met in the Jackson Room near EECS headquarters every second Friday at noon during the academic year. The nominal topic for the past twenty plus years has been learning and complex systems. I ran the seminar for some years, trying to invite at least one faculty member from each of the five schools at MIT, but in recent years the attendance has been dominated by faculty in EECS and HASS. Jay Keyser and Arthur Steinberg have now taken over running this seminar with me; largely we help plan its speakers. Some of the seminar members claim that for them going to this seminar is the most enjoyable part of being at MIT. Jay began calling it the Moses Seminar, and the term has stuck. I have seen vitae of MIT faculty in which a talk at the seminar has been prominently featured.

In the early years we studied neural networks in the seminar, and several research projects ensued. The topics have ranged broadly in recent years, depending on the interests of the faculty. It is rare that a speaker manages to get through his or her formal presentation, but on the whole the faculty members behave very collegially.

The neural network research represents a transformation of classic AI research, which dealt with symbolic reasoning; neural networks work best with visual pattern-recognition. Minsky and Papert's 1969 book, *Perceptrons,* showed that a single layer of neuron-like perceptrons could not recognize whether or not an object was a connected figure. They suspected that multi-layer neuron models would have similar weaknesses. In the mid-1980s it was shown that three-layered neuron networks did have good performance on some problems. I believed for some years that three layers are needed for many tasks, and I was somewhat unhappy that people figured this out before I had a chance to publish my observations. I was pleased, however, when Steve Pinker then of MIT's Brain and Cognitive Science Department spoke at our seminar toward the end of the 1980s and discussed experiments by neural-net advocates on how the past tense of English words is learned. They required many thousands of experiments to arrive close to what a child can figure out with many fewer observations. That is one reason why I currently emphasize trying to understand how the mind processes linguistic, rather than visual, information. Of course, the two are related in several ways. Visual information provides semantic cues about the meaning of words, such as "ball" or "mama." In addition, it is likely that the language system uses mechanisms borrowed from the visual system that is developed earlier in the child. I am hopeful that the AI faculty in this seminar will devise a new approach to AI that is closer to what our colleagues in the neurosciences have been learning about how the mind processes information**.** The so-called "Modern AI" approach used by many of our colleagues in AI, but not by members of the seminar, is close to the rest of computer science in spirit. That is, there is an emphasis on creating algorithms and systems that work efficiently in areas, such as robotics, computer vision and machine learning. Modern AI is less concerned with understanding how the mind works than either Minsky or I.

The Moses Seminar and the Faculty Lunchroom that will be mentioned later are in retrospect influenced by Norbert Wiener. The Faculty Lunchroom was an attempt to model Weiner's interactions in the Walker Memorial during lunch. The original goal was to get faculty from different parts of the Institute to talk to each other over lunch. The Moses Seminar, although it too takes place at lunch, can be seen to be an attempt to recreate the dinners at Joyce Chen's Chinese restaurant along Memorial Drive that Wiener and his friends regularly held. Even the topics, often related to cognitive science, bear some similarity to one's discussed by Wiener.

I have gone into some detail on these three approaches to collegial interaction to demonstrate my great respect for my MIT colleagues. MIT is a unique institution in which the quality of the faculty is uniformly high, as demonstrated by many national rankings of the various academic departments and programs. For example, in the ranking of graduate programs conducted by the National Research Council in 1995, MIT was ranked in twenty-two fields, and was in the top three in seventeen of them. No other institution came close in terms of the number and percentage of top-three rankings. The high quality of the faculty clearly produces disciplinary high-quality research and education, but its relative uniformity also leads to high-quality interdisciplinary research, since one can usually rely on one's colleagues and their students to perform excellent work.

A key reason for the EG&G Building had been to permit the department to have meetings of its entire faculty in the Grier Room. I scheduled lunches to make announcements such as new faculty hires and major awards to our faculty, and to discuss other significant issues. One such announcement, that Jesus del Alamo would join the faculty, became notorious. Jesus had done his doctorate at Stanford on ICs, and then went to Japan to continue his research. We had been interested in him for some time, and finally convinced him to come to MIT. I told the faculty that I had written him this letter:

> Dear Jesus:
>
> Please come back to the Promised Land.
>
> (signed) Moses.

The laughter was so great that I decided not to tell them that I had made the letter up. That letter became the most famous story about me on campus—but when it was later announced that I was stepping down as department head, one of the EECS faculty members stopped me in the hall and said that he would remember me, not for the letter but for a four-letter word. I said, "Oh, no, what is it?" He said, "FOOD!" I have also become known for sayings that others had originated, but for which they might not want to claim authorship. I think that I heard that "MIT is a praise-free zone" from Art Smith, EECS's graduate officer. He claims he heard it from his colleague Paul Gray. Frankly, I tried to work against this line as an academic administrator by praising people, and that is why I used it so much. I did originate the line "At MIT there are only two times that you would be sure to hear good things about you. One, just before you retire and the other just after you have died. If you die before you retire you might never hear good things said about you at all."

I have had a number of secretaries and administrative assistants in my years at MIT. I believe that I got along with them very well, with one notable exception. Most of them complained about my handwriting, which I describe as being Israeli hieroglyphics. Yet they all managed to get used to it, and lately, due to computers, I have had to write relatively little by hand. The exception was a secretary I had soon after Ellen Lewis. She complained that I would talk to her from my office while she was on the phone in the outer office, and she could handle only one thing at a time. She later became a lawyer and married one of the professors at the lab.

When I was an administrator I scheduled many faculty functions, and my assistants began viewing themselves as arrangers of parties. (Hence the “food” comment above.) Other than that their primary role was keeping my calendar. They tended to stay at MIT even after I changed positions; one, Mary Haas, recently retired from MIT after nearly a decade as my dedicated and valued assistant. She still comes in part-time to lend a hand in the Provost’s Office.

In 1985 I was also on a committee that considered the need to change MIT’s limitations on the number of people in the various research staff classifications. In the 1970s a ladder for research staff had been devised. Its lowest rung, research associate, usually required a Ph.D. Next came principal research associate, and finally, senior research associate. Within a decade the laboratories and departments felt constrained by the limit on the number of principal research associates in a laboratory, 10 percent of the total research staff. Most of the EECS faculty members are in one major laboratory or another, and I found out about this problem from the lab directors. The committee that addressed the issue was chaired by John Deutch, Dean of Science and about to become Provost. John had an unusual style, denying most requests and expecting you to argue him out of his position. Jonathan Allen, by then Director of the Research Laboratory of Electronics, and I argued and argued with John. Eventually Deutch agreed at one of the meetings of the committee to raise the number. I quickly said 20 percent, and we compromised on 15. It is still 15 percent, and it still needs to be higher, in my opinion.

Although I was proud of MIT and pleased with my colleagues, I decided to test the waters and apply for positions as dean of engineering elsewhere. Gerry was doing a very good job as dean, and I did not see him resigning it any time soon. I interviewed in two universities, Princeton and Cornell, and several years later at the University of Pennsylvania. The Princeton situation was unusual. Their dean was named Dick Jahns, a well-known mechanical engineer who had decided to get into research on ESP. When he was questioned about this move, he said I was told that since he had tenure he felt he could work on whatever captured his interest. His department heads went over the dean's head and negotiated budgets with the provost directly. The dean of the faculty at Princeton asked me to interview for the job, and I came and met with the department chairs in engineering. It took me only a few minutes to realize that any incoming dean would have a difficult time with the current chairs, since they were so empowered by their present situation. I did go through with the rest of the interviews. I met the provost, Neil Rudenstine, who later became the president of Harvard. The president of Princeton in 1985 was the economist Bill Bowen. I much enjoyed my interview with him, as we began to discuss who the most arrogant faculty members were on our campuses. We thought that physicists, economists, and computer scientists were the most arrogant. We agreed that physicists might have a reason for being arrogant, but that the other two groups did not: although each of these fields was important in its impact on society, the foundations of both were suspect in our eyes.

The interview with the department chairs at Cornell was quite unpleasant. Later I learned they had an internal candidate, the associate dean, named Streett, whom many liked. They asked me to justify why I wanted to come to Cornell and be a dean. Was it because I wanted to hobnob with the president of Cornell and with rich benefactors? I felt that Cornell's engineering school was too theoretical, and not as much in contact with industry as MIT's. When I suggested that it would be wise to change this situation, I discovered that the engineering faculty members there enjoy being "centrally isolated." Cornell's rural isolation, far from a major city, means that the faculty members are not bothered by many others, including staff in industry, and feel they can get more scholarly work done. I later became a member of the Cornell engineering school's national advisory board, and this impression was corroborated. The dean, John Hopcroft, who invited me to join the advisory board, did try to broaden Cornell's engineering, but met with somewhat limited success.

The interview a few years later at the University of Pennsylvania went well. The search committee seemed to be impressed, although they wanted an internal candidate also, and wondered why the administration had recently chosen several new deans from outside the university, including Tom Garrity, a former Sloan School professor and a fraternity brother of Bill Martin. I likely would have turned down the offer, but it was never made. I stopped looking elsewhere at that point.

One day in February of 1986 Corby walked into my office and told me that I had been elected to the National Academy of Engineering. At that point Corby had replaced Peter Elias as the associate head for computer science, and he remained in this role until I stepped down as department head. I was pleasantly surprised by Corby's comment. I recall that when it was announced that I would become head of EECS in 1981 Fano came into my office and told me that he didn't think I would get into the NAE, as had previous EE heads. I did not much care about that issue then, but I have cared about the NAE since my election, and have succeeded in getting several others into it as well. Peggy and I were invited to the inauguration of new members in late September. My warmest memory is the ball at which Peggy and I danced better than at any other time in our lives.

The following spring Pete Elias told me that I had been elected a Fellow of the American Academy of Arts and Sciences. I was surprised by this election as well. It was harder for a computer scientist at the time to get into the American Academy, since it admits people in many categories, and hence relatively few in engineering, although I have been able to get a few of my colleagues elected as well. This has changed in recent years with the creation of a CS category, separate from engineering. I have found that the NAE, with its narrower agenda, to be more interesting to me and more useful to the nation.

In my role as member of the Board of Directors of Analog Devices, Ray Stata and I had several discussions. Ray played an important role as a founder of the Massachusetts High Technology Council. He pressed Gerry Wilson for MIT to adopt a program similar to Stanford's one-year master's program with no thesis requirement. Stanford admitted many of Silicon Valley's EE and CS employees. Many of the courses were taught via television, a fairly innovative concept in the 1970s. In return the companies paid twice the normal tuition. I was unhappy with EECS's master's program, which had become a sort of mini-Ph.D. program. It took over two years on the average to finish our SM program with its research-oriented thesis, and often nearly three years in CS. I railed against the length of the master's program in our departmental faculty lunches, but the average tenure in it seemed to creep up each year.

I asked our department's committees to examine the issue of the length of the master's program. In the meantime we created a program for MIT students who would work in local industry. This differed from the Stanford program in that most of the students would be MIT undergraduates who had been admitted to the master's program. We also required a thesis, albeit one done at the local company site, and under joint supervision by them and by an MIT faculty member. We asked local companies to sponsor students, and as at Stanford to pay approximately double the normal tuition, probably a too steep price given that the students were not the companies' employees. Frank Reintjes and I oversaw this program and tried to sell it to local industry. Frank had taught at MIT since the Second World War, and was officially retired. His health was so good that he was able to teach electronics courses into his eighties. We had difficulty convincing many companies to join the Master's In Industry (MII) program, but we were able to admit about half a dozen students a year. I supervised a student who did her thesis at Teradyne, for example; others worked at a Honeywell branch in the Boston area. I was somewhat surprised that Analog Devices did not join the program, but then they were members of the our Cooperative program and had a couple of dozen EECS students working in their plants each summer, with several doing their master's theses each year.

We knew that the MII program was a stop-gap solution until we figured out how to reduce the length of a master's program to about a year for most if not all of our students. One day Bill Siebert pointed out in a committee discussing the issue that MIT's coop students were taking a year to finish their theses at the various company plants around the country. Why not create a program like that, with our students doing the thesis on campus, he asked. Coop theses were developmental, rather than research-oriented. This is what a master's thesis was supposed to be about, we noted. However, the pressure on our faculty from many funding agencies was to get a published paper from nearly every thesis student, even from master's students. We needed to convince ourselves that it would not be a disaster to the department's research program for master's students to do a project-based thesis, rather than a research-oriented one.

Siebert's analogy to the cooperative program pointed the way to a solution, but there were additional complications. I was concerned that many of the master's students doing developmental theses on campus could not be hired as research assistants. Essentially all of our graduate students are funded through fellowships and research or teaching assistantships, with most of the support going to research assistantships. I felt that parents had decided to help their children for four years of college, but had not planned on paying for an extra year, nor did our students wish to add to their educational loans. This concern caused the discussion of the new master's to drag on for several years. My successor, Paul Penfield, finally took the bull by the horns and introduced a new program, called the Master's of Engineering, or MEng. He took the risk that students would be willing to join the program even given the likely funding situation, but it worked out for the best. Many of our faculty members were able to fund research assistantships, and many of the other students felt that a master's degree from MIT was worth the extra cost. The MEng program became Paul's greatest achievement as department head. Of course, most ideas are not pure successes, and the MEng program is no exception. Several years later I heard complaints that we admitted so many of our seniors to the MEng program that the content of the introductory graduate subjects had to be watered down somewhat to meet their capabilities. Thus the department had to create additional graduate subjects to meet the needs of its doctoral students. My other concern with the MEng program is that it did not meet a goal I and the committee I formed had for it, namely that the fifth year was to broaden the student as well as deepen them. By breadth I did not mean simply technical breadth. Coop students gained some implicit understanding regarding how firms worked, and how some non-MIT people behaved. The MEng program that was implemented in the early 90's lacked such experiences.

By the mid 1980s Gerry Wilson became increasingly agitated by the relatively low state of U.S. manufacturing. Japanese auto firms had been able to increase their world-wide market share in the 1970s and 1980s, partly because the quality of their products was clearly world-class, also because their manufacturing costs were lower than in the rest of the world. The Japanese were also able to meet changing consumer preferences in automobiles faster than anyone else, due to their ability to get new models out more quickly than anyone else. Representatives from U.S. manufacturing firms who were on MIT's board of trustees (the Corporation) or its visiting committees told Gerry that MIT needed to help U.S. industry by teaching new cadres of students how to improve our own manufacturing processes, and Gerry discussed the issue with the members of the Engineering Council. I agreed that the problem was serious, and strongly supported the notion that MIT, working with industry, attempt to improve U.S. manufacturing. Gerry appointed Kent Bowen, a professor of Materials Science and Engineering, to be the leader of this activity in the School of Engineering.

MIT was not the only place that was concerned about U.S. manufacturing; the National Academy of Engineering and the National Research Council were also concerned. A study in the early 1980s had recommended the creation of research centers in universities to deal with manufacturing, which had slipped in importance in engineering schools partly as a result of the Engineering Science revolution led by people such as Gordon Brown. Moreover, there was insufficient concern with design, engineering management, and the environment in schools of engineering in the 1980s. Gerry wanted to turn this around, at least at MIT.

Congress, prompted by this new concern, appropriated funds for new engineering centers, and NSF was asked to create and supervise them. Kent Bowen and others at MIT submitted a proposal to the NSF committee overseeing the selection of the first few such centers. We recognized that the manufacturing problem was only partly technical. There was a significant management component as well. The NSF committee at that time was relatively engineering science-oriented in its appraisal of proposals. Thus Bowen's proposal for a center was turned down, although many of us strongly believed that it was in the spirit of the original studies that led to the creation of the NSF engineering research centers. The centers that did succeed in getting funded tended to look at relatively technical aspects of manufacturing, which may have been helpful but really dealt with only a small piece of the puzzle.

Gerry Wilson and the rest of us were quite disappointed with NSF's conclusion. Gerry decided that if industry really wanted a significant MIT response to the manufacturing challenge, then they would have to ante up significant amounts of money, and he challenged the major manufacturers to fund MIT to the level of $50 million by the end of 1987, with five firms expected to pay $8 million each over a number of years, and ten others $1 million each. Kent Bowen and Tom Magnanti of the Sloan School were to help raise the funds and run the program, at least initially. At first it seemed no money was forthcoming, but then toward the end of the year (and a few months thereafter) a number of $8 million pledges were made by firms such as GM and DEC. The program that resulted is called the Leaders for Manufacturing (LFM) program, which admits fifty students a year to a two-year program whereby they obtain an MBA in MIT's Sloan School as well as a Master of Science degree in an engineering department. In the first few years all LFM students were given fellowships, attracting outstanding applicants. Students are no longer given full fellowships as a matter of course, although company-sponsored students usually receive significant support. The program has been going strong for over fifteen years now, and has been copied in one form or another by other universities. Project Athena and LFM were Gerry's major achievements as dean, and each had an impact well beyond the School of Engineering.

The Leaders for Manufacturing Program

I had also become interested in manufacturing, but in a round-about way. When I became associate head of EECS I had decided to read the literature on management, beginning with March and Simon's seminal 1958 book, *Organizations*. This is the same Herb Simon of AI fame. I was struck by the fact that Simon's approach to management, specifically his approach to the organization of firms, was so similar to his approach to problem solving. Simon's key insight into organization as well as problem solving had to do with tree structures. His Nobel Prize in Economics was given for the insight that decision trees can get so large that no human can analyze all the alternatives, and thus managers have to make decisions on the basis of incomplete information. I, on the other hand, was unhappy with tree structures when problems were large and complex. My mathematical background, in particular the power of abstract algebra in integration, led me to emphasize layered structures, and tree-structured hierarchies are not layered hierarchies. I later decided that my parents' German background also greatly contributed to my interest in relatively flat and layered structures. When I started reading the literature on Japanese organizations in the early 1980s I saw that they, too, liked layered organizations as well as abstractions.

Japanese manufacturing prowess was viewed by most American observers as a skill in designing high-quality goods, and the main stories about Japan emphasized how Americans, such as Deming, taught them how to improve the quality of their products. These same Americans were ignored by the

U.S. manufacturers in the 1940s and 1950s. What I saw, however, in Japan's production processes was an ability to introduce new products much more quickly than comparable U.S. firms. I wondered what gave the Japanese firms this flexibility. Books and articles that were written on Japanese firms in the early 1980s gave me enough clues that I was able to arrive at a theory. My theory was that U.S. firms were largely organized in a bureaucratic tree-structured hierarchy, just as Simon has assumed. Such hierarchies are inherently incapable of coping with rapid change because the hierarchy requires people to go up the chain of command, and this takes time. If they try to get around the chain of command for efficiency reasons, they greatly increase the complexity of the organization, making it difficult to make further changes readily after some point. Similarly, a bureaucratic hierarchy tends to create competition between different parts of the organization, which makes it difficult to get people from different divisions to cooperate and work in teams effectively and quickly. This also leads the design division to 'throw the design over the wall' to the manufacturing division, ignoring the possible difficulties thus created for the manufacture of the product.

I viewed the large Japanese firms as having an internal organizational structure based, in part, on layers, similar to the structures I created in software in Macsyma. The layered structure plays a key role in mentoring within these firms, where there is an overlay of a relatively classic bureaucratic hierarchy for project management. The flat, layered mentoring structures made it far easier for Japanese workers to work with others in different parts of the firm. It seems that Japanese mentors try to increase the knowledge and trust of the mentored workers in others in the firm with whom they might eventually have to work. Indeed, it took Japanese firms longer than U.S. ones to make the initial decisions, but the time was not wasted. The result was an implementation process that worked smoothly and quickly once it got started. In the United States, decisions were made relatively quickly, but then as problems and differences arose, much time was spent dealing with them. Moreover, when teams were created in the US of people from different parts of a firm, they tended to take much time getting to the point where they were effective.

I recalled reading the view that many modern issues have been encountered in past centuries and millennia. I recollected the debates in previous centuries that I learned about at Columbia, between Hume and Kant, and between Aristotle and Plato. It now seemed to me that the difference between the U.S. approach to management and manufacturing and that of the Japanese exhibited many similarities to these older sets of differences. Aristotle represented middle class values, and Plato represented aristocratic values; Aristotle tended to think in terms of tree-structured organizations (like Herb Simon); Plato tended to think in terms of layered organizations (like me).

I also began recalling stories in the Bible that alluded to such debates. The first written description of a bureaucratic hierarchy was in Jethro's proposal to his son-in-law, Moses, at the foot of Mount Sinai. Jethro urged Moses to create a tree structure of judges reporting to him—perhaps the only suggestion by a human to which Moses paid any attention. Moses created a rather different organization for the religious hierarchy of the Israelites. It had Aaron as the High Priest, Aaron's family as the remaining priests in the top layer, the rest of the Tribe of Levi in the middle layer, and the remaining tribes as the lower layer.

Tree-structured organizations are good for individualism and competition, as is usually emphasized in the United States, with its middle-class-oriented values. Layered organizations, with their emphasis on flatness, are best when the culture emphasizes teamwork or cooperation. Teamwork is in fact difficult to achieve in a pure tree-structured organization because the horizontal or nearly horizontal links are "not legal" in such a hierarchy. On the other hand, layered hierarchies have an overhead in crossing boundaries between layers. While large U.S. firms tended to be organized along Alfred P. Sloan's hierarchical model for General Motors, there are surprisingly many U.S. firms organized along layered lines and emphasizing cooperation. These are large partnerships, such as law firms (senior partners, junior partners, and associates) and accounting firms. Universities (full professors, associate professors, and assistant professors) have traditionally been based on the Catholic Church's layered hierarchy (archbishops, bishops, and priests). The tree-structured departmental overlay combining both research and teaching occurred first in Berlin in the Charité Hospital around 1800. Von Humboldt created it, I believe, because of the needs of the medical school for specialization. Departments that combined research and teaching were introduced into the United States around 1880 at Johns Hopkins, which is also heavily influenced by the needs of its medical school. When President Eliot of Harvard adopted the model, it took off in the rest of the country. Departments have been extremely useful in focusing on research. Administrators, such as myself, have decried their negative influence on interdisciplinary education and research, for which a flatter organizational structure might be better. In contrast, a well-organized law firm could in a fairly rapid order create a team composed of one senior partner, some junior ones, and several associates from a variety of specialties of the law in order to deal with a new case that has just been brought to the firm.

In the late eighties I gave a talk along these lines at an IBM conference in Boca Raton, Florida. After the talk a number of people, most of them women, wanted to continue the discussion. This should not have surprised me since competition is a sign of alpha males, and cooperation in our culture is considered a relatively feminine trait. One of the women, Kirsten Pollack, was the head of the staff of the Manufacturing Studies Board of the National Research Council. I was invited to join the Board soon thereafter. Its role was to propose and oversee studies related to manufacturing that the NRC would then conduct. My studies took me back to Japan in the next two years in order to contrast how manufacturing was done there and in the United States. I already knew that I was sympathetic with certain of the Japanese approaches to manufacturing and organization, particularly the relative flatness of their firms as well as apparent holistic aspects of their approach to problem-solving. On several occasions I have talked to Germans, and they have claimed that my views are obvious; Americans, usually tended to dismiss the same views. On the other hand, American firms applied some of the Japanese approaches in the 1990s, such as cross-functional teams and the Toyota manufacturing process. American manufacturing has improved significantly as a result. Japan has undergone a long recession since the bubble burst on its real estate values. Many Americans have viewed this as a sign that all was well in the competition between the US and Japan in manufacturing. This view ignores the fact that some Japanese firms, such as Toyota, still lead the world in their industries.

Another of Gerry Wilson's concerns was the financial health of MIT. Recall that research support from the DoD had been under pressure since 1968, and the funds that pay for some of the overhead expenses in universities, such as the cost of maintaining buildings, did not grow much. The endowment was largely invested in the stock market, and the Dow Jones average was nearly the same in 1981 as it was in 1968, thus the payout from the endowment could not grow significantly in the 1970s. Private universities began to increase tuition at above the rate of inflation in the 1980s, partly because faculty salaries had significantly declined in real terms during the 1970s and needed to be adjusted upward. Surely this tuition rise could not be sustained for long. MIT started a new fund-raising campaign in 1985. It was publicly announced in 1987, coincidentally during the week of the stock-market crash. The crash notwithstanding, the campaign by itself although eventually quite successful in meeting its overall goal, did not appear to significantly reduce the financial pressures on the Institute. Thus Gerry concluded that the key to the financial health of MIT was to reduce expenses. In particular, he felt that MIT had to reduce the size of its faculty as a key step in bringing costs under control. I tended to agree at the time with his general analysis. Other deans did not, and it did not appear that the President and Provost were going to pursue such a course for the Institute as a whole. Gerry seemed ready to reduce the size of the faculty in the School of Engineering regardless of what the rest of the Institute was prepared to do. The mechanism he used was to initially reduce the number of allowable faculty positions, which was usually greater than the actual number of faculty since we were constantly searching for new faculty to fill openings. Thus there would not be much of an impact until the reduced allowable staffing was close to the actual number of faculty. Nevertheless, I was opposed to a unilateral move by the

School of Engineering. The EECS Department was spared for a couple of years because of the continuing concern over its large enrollments. When 1989 began, Gerry reduced our allowable staffing, and I resigned as department head.

I have wondered many times why I disagreed with Gerry so strongly over the faculty staffing issue. Clearly the issue itself was a big part of it. On the other hand I also believe my rebellious streak showed up in this case. Why am I rebellious? I ascribe it, in part, to my left-handedness. The recent book *Born to Rebel* (Frank Sulloway, 1996) claims that intellectual rebels are usually not first-born children, and I am a first-born son. The book does give itself an out in the case of only children, which I was for a bit over a decade. Nevertheless, I am convinced that some left-handers tend to be rebellious by virtue of this aspect of their birth which tends to cause different neural wiring than for right-handers. In older times many left-handers were forced to write with their right hand, and thus there are relatively few records of left-handers in the arts and sciences. Einstein, Churchill, and Leonardo da Vinci are known to have been left-handed, and Churchill certainly had his rebellious streak. Left-handers make up about an eighth of the population, yet three of the past seven U.S. presidents have been left-handed, and four of the final eight candidates for President in 2000 in the two major parties were left-handed. My theory is that recent politics favored outsiders, and left-handed people are likely to feel like outsiders since their different neural wiring likely causes them to see things differently from right-handers. Outsiders could also be rebellious because what is obvious to them might not be obvious to others and vice versa; when they press their points others consider them rebellious.

A search committee formed soon after my resignation was announced, with Paul Penfield as chairman. Paul's wife had recently died and I felt that he would not be willing to become head of the department right at that time, although he was certainly qualified. I was not surprised that the committee had some difficulty arriving at a suitable list of names. Many faculty members of my generation had been unwilling to take on significant administrative roles. This may have been due to the fact that there were relatively few in my cohort. Significant hiring started in 1978 again, but these faculty members were too young to take on the department head's role in 1989. The committee had even suggested that I stay on as department head, and I was willing to do so for a while, but not surprisingly Gerry demurred. One day I received a call from a member of the search committee. He said that Penfield would be interested in becoming head after all. I passed this along to Jack Kerrebrock, the associate dean, and within a few weeks Paul was named head. Corby was retained as associate head in computer science, and Jeff Shapiro was appointed associate head on the electrical engineering side. Penfield was a productive department head for the next nine years, and then he stepped down and was replaced by John Guttag, who was one of the cohort of young faculty the department hired in 1978.

A Return to Israel

It was during this period of intense involvement in departmental matters that I was able to satisfy a long-felt personal desire—to return, now with my own family, to my childhood home. In May 1987 I went back to Israel, where I had not been for 33 years—partly because I still owed military service. People used to say, 'Don't worry about it: you're too old; you left at such a young age; you can't pass the medical.' None of this made me feel safe from being snatched up and put into an Israeli uniform and made to program some software system, since I had never renounced my Israeli citizenship. The Weizmann Institute was looking for a committee to advise them about their computer science activities, and I had been asked to join it. I asked them to address the military problem before I would be willing to go, and they did. I received a 60-day dispensation from military service, so Peggy, David, Jesse, and I went there for ten days, just before Israel celebrated its 39th Independence Day. Peggy, Jesse, and David went on their

American passports, and I obtained a Laissez-Passer from the Israeli consulate in Boston, which served as my passport.

We first stayed at a hotel in Netanya, where I had lived for seven years. When I left Netanya in 1954 I think it had a population of ten thousand; in 1987 the population was a hundred thousand. My cooperative apartment house was the largest house on the block when we left; now it was one of the smallest. I went to see it again, and met the ladies who now lived in our apartment. They spoke little English, and my Hebrew was not very good, so we spoke mostly in Yiddish, although I really spoke largely in German with a bit of a Yiddish accent. I found out that Mr. Perez, who had lived on the apartment above us, had died just in the past year; the apartment house's cesspool, which used to overflow annually, still did. The apartment itself was small—much smaller than I remembered, of course, but we lived in even smaller ones during our first years in the United States.

I telephoned the parents of my best friend in Netanya, Ze'ev Magidi, and told them that I wanted to come and visit them. His mother couldn't get over my being there after all those years when we had never communicated with each other. Ze'ev himself showed up. He runs a business where he helps create the sound and light shows for musical groups. He told us of his divorce, and also said that his teenage son helps in the setups he performs. Ze'ev looked very much as I remembered him at his Bar Mitzvah party; he kindly didn't mention my weight gain. He showed me his sister's graduation certificate and I recognized several of the teachers' names. This visit to his parents' home was a highlight of the trip for me.

Peggy remembers our brief visit to my school. She says I was so excited that I skipped along the way. The schoolyard where I sprained my left wrist and that used to be full of sand was completely paved, and a number of new structures had been built. The school itself was closed because it was a Saturday.

That Saturday afternoon we walked along Netanya's main street. The people had continued a tradition of strolling on Saturday afternoons, especially with their young children. My father had done so with my brother Abe. The contrast with a U.S. street was startling. There were a great many young children, for one thing: Israel encouraged a high birthrate among the Jews, since the Israeli Arabs had a high rate. We walked in the garden overlooking the Mediterranean Sea, the Garden of the King. It was lovely on that May day. The sea was too cold for swimming, but many people walked along the shore.

We took a cab to Beit Yitzchak the following day. Our first stop was the cemetery where my mother's father, Abraham Losner, is buried. The cab stopped along the way and waited while an air raid siren wailed. This was in memory of the many soldiers who died in the wars between Israel and the Arab nations. I found my grandfather's grave near the entrance. To my great discomfort I was roundly criticized by another visitor because neither my sons nor I wore yarmulkes, the traditional head-covering in a Jewish cemetery.

We then went to visit some of our former neighbors in Beit Yitzchak. I saw the farm belonging to Yitzchak Goldberg, the first child born in the village, about a year before me. I talked to him in German and he introduced me to his Arab workers and to his son. He had expanded the farm of his parents, and planted corn as well as produced milk on it. My guess is that German reparation payments played an important role in his success. His mother was not around, and so we crossed the street to talk to his neighbor. The neighbor was in his mid-eighties; he remembered two Moses families in the village, the "flying Moses" and the "regular Moses." I said that we must be the regular Moses family, but he corrected me later, saying that my father was the flying Moses. When I asked why, he said that in those years, no one else arrived in Palestine in a plane.

We were ready to leave the village, when a tractor stopped us. Mrs. Goldberg, now in her mid-eighties, descended from it. She had been an English teacher in the village school, which helped us immensely, since it permitted Peggy and the boys to join in the conversation at her house. One of the things she told about was my first birthday celebration, when her husband drank so much that she never forgave him. It took me several months to figure out why this birthday became such a big event in Beit Yitzchak. The battle of El Alamein took place in October 1942, a month before my first birthday. Fortunately, the British defeated the Germans, partly as we now know by having broken the German military codes. My guess is that the people in the village needed to have a party to celebrate the great defeat of the Germans. It would have appeared to these escapees from Germany that the Germans were going to get them again if they had defeated the British in Egypt. They were in sore need of a celebration when the Germans were defeated.

Our final stop was at our old farm, farm number 13 in the village. The owner was in, and he spoke English, too. He had been a cab driver in Brooklyn, earning money to buy the farm. He was proud of how he had enlarged the building, but when I entered it, it was tiny, much smaller than I recalled.

We finally left Netanya and took a cab to the Weizmann Institute, where we stayed in student dorms. While I was visiting the various labs and offices in Computer Science at the Weizmann, Peggy and the boys went to the Dead Sea, first taking a bus to Tel Aviv. There they found the bus station extremely busy, with many buses going in and out. They first went to Masada, where in 73 AD the Jews committed suicide rather than surrender to the Romans, then they went to the Dead Sea, where they experienced the strange phenomenon of floating like a cork in the saline water. Apparently Jesse was slow getting out of the water, and they nearly missed the bus to Tel Aviv. On the way back, Peggy asked the driver to stop at a bus stop nearest to the Institute, which she thought would be a short-cut, and he stopped in an Arab village, Ramallah. Fortunately there was an Israeli soldier there to protect the three of them; a few weeks after we left the country the bus station was blown up in one of the early incidents of what came to be known as the First Intifada. Peggy, who had once had a heat stroke in Bermuda, suffered a near-relapse at the Dead Sea, which is not too surprising, considering the temperature of the place. Israel has many microclimates for a place that is about the size of Massachusetts: Jerusalem, which we visited next, has snow in the winter; a few dozen miles away, the Dead Sea has a high temperature of 120ºF on many days; the Sharon Valley, where Netanya is located, has lush vegetation, but a few dozen miles east it is hilly, and farming is much harder.

After the visit to the Weizmann Institute we went to Jerusalem. Before 1948 it was possible to visit the Old City, but I never had; after 1948 we could not visit until it was recaptured in 1967, and by then my family had already emigrated. So this was my first visit. We went on a Friday morning, our main goal the Wailing Wall. Peggy was prepared with her cameras, and had brought a timer that allowed her to get a photo of all of us. Boldly, she tried to take a photo from the walkway leading to the Al Aqsa Mosque, but she was nearly shoved off it by Moslems celebrating their holy day.

We next went to the Church of the Holy Sepulcher, where Jesus is reputed to have been buried. There are many Christian sects here, and each has some claim to the place, claims that have been argued over for centuries. We went into the room where the burial place is supposed to be, and a monk there was prepared to give us a candle in return for a gift. I was surprised by this request, nor do I believe that most Christians would be happy with it, either.

Jerusalem is a remarkable city. The buildings of the Old City are largely of limestone, and take on a golden hue at sunset. One cannot escape the feeling that every building is hundreds of years old, and the archeological excavations indicate settlements several thousands of years ago. I have been to many of the great cities of the world, but Jerusalem is unique among them, with a remarkable tradition that exists nowhere else.

We were finally ready to return to the States, but there was another hitch. With my Israeli Laissez-Passer, I was prepared to pay the exit tax that any Israeli citizen would pay. The trouble was that they could not find a record of my arrival. When we arrived the computer broke down, so there was no computer entry. As I discussed this situation with the Immigration people, I could see out of the corner of my eye that Peggy, David, and Jesse were slowly edging away. I thought they were prepared to leave me in Israel. Fortunately I did not have to test this hypothesis, since the Immigration people were able to find some paper record that we had arrived when we said we did, and all of us returned to the States safely.

I asked for and received a sabbatical for the next academic year, 1989–1990. This was my first sabbatical since I became an assistant professor 22 years earlier. I did not feel I could leave or move Peggy and our twelve-year-old sons. David and Jesse had to prepare for their Bar Mitzvah, and this made it doubly difficult to move. So I considered alternatives in the Boston area. I called Bob Hayes, a professor at the Harvard Graduate School of Business Administration. I had read and liked his paper with Abernathy on how U.S. business schools were partly to blame for the manufacturing crisis. I told him I wanted to spend the sabbatical at Harvard, but that I needed to get half my salary from Harvard. He agreed to have me teach a seminar on AI the following spring. Since this occurred late in the academic year, he was concerned lest Harvard appear to violate an agreement between MIT and Harvard regarding raids after May 1 of any year, and asked for an appointment with MIT's Provost John Deutch to discuss the issue. What I did not know was that both Hayes and Deutch were on the board of directors of the Perkin Elmer company. At our meeting Deutch readily agreed to the sabbatical arrangement, and much of the remaining discussion was on issues at Perkin Elmer.

Just as I was about to leave for Harvard, I was informed that Gerry proposed to give me the D.C. Jackson chair in EECS, a chair first held by former department head Louis Smullin. After I became an Institute Professor in 1999, the chair was given to Paul Penfield as he stepped down from being department head.

One of the unfinished projects from my time as department head was the report of the committee I chaired on the MIT Lincoln Laboratory. In the late 1980s there was some discussion by faculty members, such as Louis Smullin, that it was time for MIT to divest itself of the Lincoln Lab, just as it had divested itself earlier of the Draper Laboratory. One argument was that there was little technical interaction between the campus and Lincoln, certainly in comparison to the early days in the 1950s. Second, there was a negative feeling on the campus toward the "Star Wars" concept that President Reagan was pushing; a significant fraction of Lincoln's work was being categorized by the DoD as Star Wars R&D. Finally, although it was understood that the campus was obtaining a financial benefit from overseeing Lincoln, the amount of the benefit was viewed by some as minimal.

John Deutch asked me to chair the committee to review the campus/Lincoln relationship and to propose what changes, if any, should be made. This made sense since EECS had the closest relationship to Lincoln of any MIT department. The committee was quite high-powered. On the campus side it had, among others, Francis Low, Deutch's predecessor as provost, Sheila Widnall, future Secretary of the Air Force, and Glen Urban, future dean of the Sloan School. Walter Morrow, director of the Laboratory, was one of the Lincoln members on the committee.

> One of the things we discovered was that the funds flowing to the campus, either through overhead payments or through employee benefits, were substantial, over $20 million per year. I made a tour with another committee member, Vincent Chan of Lincoln, of a number of similar government-funded laboratories. In particular we visited the University of California's office overseeing both the Livermore and the Los Alamos laboratories for the Department of Energy. These labs had a volume about six times larger than Lincoln's, yet the funds going to UC were about half those flowing to the MIT campus. Moreover, the University of California spent much of this money on joint R&D between its campuses and Livermore. The attitude at the University of California appeared to be that the existence of Livermore was good for the

state and that a state institution should support it. The situation at JPL and the Cal Tech was closer to that of Lincoln and the campus. Here we had greater difficulty getting accurate data, but from what I gathered in discussions with the provost of Cal Tech and the director of JPL, the funds flowing to the campus were lower than at MIT, although JPL's volume was three times that of Lincoln. All in all, it appeared that MIT enjoyed a far better financial relationship from its oversight of Lincoln that any comparable relationship among U.S. academic institutions, given the R&D volume of the various laboratories. A key reason for this difference was the technique, originally negotiated by Deutch, we used to pay the tuition of research and teaching assistants. Basically, their tuition was considered an employee benefit and was taxed as such on all MIT salaries, including salaries of all faculty and staff. Deutch's method actually benefited the U.S. government as a whole, although it cost Lincoln's sponsors quite a bit of money. The reason for the overall advantage to the U.S. government is that a majority of the salaries at MIT overall were not paid from Federal funds. The other academic institutions that used this method, Cal Tech, Stanford, and Columbia, also had a large federally funded laboratory associated with them. The effect was to have such laboratories pay a substantial amount of graduate tuition, and thus subsidize research contracts in the rest of the university. This method of funding tuition will be discussed again later because its removal would cause serious difficulties in the next decade. I estimated that in 1988 Lincoln paid the tuition of about 700 research assistants, nearly all of whom never set foot in the lab. I was not eager to publicize this result, feeling that once it was found out, there would be moves to change the accounting rules, which would significantly lower the level of funds flowing to the campus.

There was much discussion in the committee about the fundamental question of the nature of the future relationship between the campus and Lincoln. While I sensed that there was little enthusiasm in the committee for a significant change in the relationship, no one was willing to come out and say so. Toward the end of the academic year I pushed the point in the committee, and the conclusion was that we would recommend no significant change. We did feel the need for a closer involvement between the campus and Lincoln, and recommended a number of changes to that effect, such as including MIT faculty members on the Lincoln Laboratory Advisory Board.

The report of the committee was not finished until the fall of 1989, when I was on sabbatical, and I traveled to the campus a number of times to finish it. Deutch asked me to give a summary to the Lincoln Advisory Board, and eventually to the MIT faculty at its regular October meeting. I was nervous at that meeting, because I did not wish to discuss the finances. Sure enough, someone asked such a question. I mumbled some answer, and Deutch rescued me by getting up and claiming a number about half what I estimated it to be. I didn't argue with his analysis in public.

The most exciting thing at that faculty meeting was a report of a committee chaired by Molly Potter on housing for undergraduates. Traditionally MIT freshman chose between on campus dorm rooms and fraternities, some of which are not in Cambridge but in Boston. This choice was made in a hectic few days just before the start of the freshman year. Molly's committee recommended instead that all freshmen be housed on campus. Some faculty members who had lived in fraternities while undergraduates objected vehemently, arguing that it would kill fraternities, and Deutch, who favored the Potter proposal, did not go forward with it. It is unfortunate that the Potter proposal was not implemented when it was first presented, because the issue of undergraduate housing would become critical, under circumstances of great difficulty, during my term as provost nearly a decade later.

Chapter 9: 1989–1995

Sabbatical Year, 1989–1990

For my sabbatical, Harvard gave me a large office in the basement of the Baker Building at the Harvard Business School. I paid $300 for a parking sticker, which as academics know is as important as an office; this seemed exorbitant compared to MIT's parking charge, but MIT has since caught up. I filled my half of the office space with the books and computer I would need, and looked forward to a year of uninterrupted research and writing. The office would be shared with Eric Lander, at that time teaching at HBS. Lander is a pretty amazing fellow. He obtained his Ph.D. in mathematics, learned a great deal of computer science, and while at HBS became interested in modern biology. He is now a professor of biology at MIT and headed its human genome project. Not surprisingly, the genome effort relies on robotics to replicate biological assays, and uses mathematical algorithms to combine their results. I never saw him during the year, although I believe he taught all his HBS classes. I understand that Eric won the national high school mathematics contest in his senior year at Stuyvesant High School in New York, so we have some experiences in common.

In the office on my right was Michael Porter, whose book *The Competitive Advantage of Nations* (1990) has impressed me. I think I saw him twice, since he was traveling a great deal on this comparative project. In the office on my left was Steven Wheelwright, who had recently transferred from Stanford University—a move that created a sensation in the business press. He and Kim Clark, who was also in HBS's Production and Operations Management group headed by Bob Hayes, were working on books describing cases in which American companies had improved their manufacturing practices. I attended some of Steve's case-study lectures, and was extremely impressed by his devotion to teaching. The case method is quite different from the lecture method used in engineering courses at MIT and elsewhere, and HBS does it as well as anyone. The HBS faculty travels a great deal, interviewing managers in firms worldwide and creating case studies that are used to educate MBA students around the world. HBS publishes such cases and earns many millions from the enterprise.

In a lecture I attended, Steve discussed the case of an American firm, Ampex, which made decisions that eventually caused the United States to lose its dominant position in videotape technology. He posed questions to the class of about eighty students, and used their responses to build up his talk. It was a very impressive performance. I later asked George Lodge, another HBS professor whose work on competitiveness I admired a great deal, what it would take to make him miss one of his classes. He said that if his temperature were less than 103ºF he would come in to deliver the lecture; at MIT we would try to get a substitute or cancel the class if we had a temperature of 102°F. We pride ourselves in EECS on our devotion to teaching, but HBS provided me with another model. I think they cared more about teaching MBA students than we cared about undergraduate teaching. I would not have believed it unless I saw it with my own eyes. George Lodge was a member of the Lodge family of “Boston Brahmin” fame, and ran unsuccessfully against Ted Kennedy for the senate seat from Massachusetts in 1962. Ted ran on the slogan, “He can do more for Massachusetts,” which may have been true since his brother was President at the time.

At HBS I started using a Macintosh computer to do my writing. Until that point I had used a dumb terminal connected to a DEC 20 computer, so I needed some handholding while I learned the new system, at that time quite different from the environment I was used to. I have come to love the Mac; by now Digital has been sold and the DEC 20 line is non-existent. What I had intended to write about was organizational structures and their relation to national ideology. I eventually wrote about one hundred and fifty pages, but I have never published the paper, in part because I did not feel that I knew the exact audience for the work. It was too technical for most audiences concerned with cultural studies, and was far less technical than the usual CS or AI audiences I used to address. I set it aside, and turned to another interest, large-scale engineering systems, and their complexity and flexibility. I am still thinking about the first topic, and the two issues are connected in my mind.

MIT could not fully let go of me while I was at HBS. Jack Kerrebrock, the Associate Dean of Engineering, became acting dean while Gerry was on leave at United Technologies trying to improve their manufacturing processes. Jack asked Dan Roos and me to co-chair a committee looking into large-scale systems, a key result of the most recent Five Year Planning meeting in 1987. Some of the other members were Tom Hughes, a leading historian of technology who was visiting MIT from the University of Pennsylvania; Rosalind Williams, a professor of writing and also a historian of technology; and Tom Magnanti, one of the two initial co-directors of the Leaders for Manufacturing Program. Earll Murman, later to become head of the Aeronautics and Astronautics Department and Fred Moavenzadeh, the director of the Center for Technology, Policy and Industrial Development were also members of the committee. The committee's topic was related to the writing I was doing at HBS, although the breadth of our membership made the discussions veer in various directions. Many years later several members of the committee would tell me, without prompting, how much they enjoyed the discussions that took place in it.

Paul Gray had announced that year that he would step down as president of MIT at the end of the academic year, and a search committee composed of faculty members as well as members of the Corporation, MIT's trustees, was formed. In the middle of the fall term I received a call from the chair of the search committee, Carl Mueller, asking me if I would come and help them in their efforts. I was startled: this is often the way such committees ask candidates to be interviewed. I had never thought of myself as a university president, especially MIT's president. I could see myself as a dean, and possibly a provost, but I did not feel I had the fundraising ability required of a president. On the other hand, I heard rumblings that our key internal candidates were not doing well in their interview process. I asked for Peggy's advice, and she said she did not wish to play the role normally expected of a president's spouse. I would use the interview to tell the committee the directions I thought MIT needed to go in the coming decade, but I also wanted to tell them that I did not see myself as a candidate for president.

The interview took place in November. I talked about large-scale systems, the need for interdisciplinary research at MIT, and the continuing importance of undergraduate education in defining MIT. When I mentioned that I thought that the brain was the next frontier in science, and that understanding the brain would play a key role in the future of AI, Phil Sharp, who was the associate chair of the search committee, got very excited. He said that biologists at MIT also think that the understanding the brain was the next frontier. I was asked whether I would consider being president, and I said that I could see myself as provost but not president. That was at the end of the interview, and I went back to Harvard.

The committee was really having difficulty with its internal candidates, but was impressed with Phil Sharp, who was asked to remove himself from the committee. A couple of months later it was announced that Phil would become the next president of MIT. In the week that followed I received a number of calls at HBS telling me that the rumor on campus was that I would be asked to become provost. This I did not mind. With the president a scientist, it was natural for an engineer, especially one who graduated from the School of Science, to become provost. Many of us were shocked when Sharp announced toward the end of the week that he had changed his mind, and would not become president after all. Now the search committee had to go back to work and get another candidate. I had learned about the selection of Sharp in a rather unconventional manner—in a call from the chairman of the search committee, Carl Mueller, thanking me for my participation in their selection process, and telling me that Sharp would be announced soon. I heard that Gerry Wilson became very upset at this way in which he was informed that he would not be chosen; it did not bother me as much since I never considered myself a candidate for the position.

Large Scale Systems

The Large-Scale Systems committee found it hard to coalesce on a set of issues. I came up with a distinction that helped the process along. I distinguished between "open" and "closed" systems. Closed systems had relatively well-defined specifications as is usually assumed in engineering science, but the closed systems of interest to the committee were large, complex, and required the flexibility to change readily over time; examples were cars, planes, and large software systems. Open systems usually involved discussions with public bodies in order for the specifications to become clear; examples were transportation systems or large-scale construction projects. Members of the Civil Engineering department, such as Dan Roos, had a lot of experience with open systems. I felt that most of the other engineering departments would be more acquainted with closed systems, although even such systems as we were considering in the committee were not the usual ones discussed in our classrooms, where we tended to emphasize designing and understanding relatively small components, rather than systems. Of course, the distinction is not all black or white. I certainly felt that all the engineering departments would benefit from some emphasis on open as well as large-scale closed systems.

The distinction I drew between open and closed systems also permitted me to see a weakness of our major effort in manufacturing, the Leaders for Manufacturing program (LFM). I felt that LFM was indeed a great new educational and research program for MIT and the nation, yet most engineers were involved in designing new products and systems, rather than in manufacturing them. While this relative emphasis on design instead of manufacturing was indeed a weakness in many of our industries, I felt that the design of complex large-scale products and systems also needed more attention than it was getting in our educational programs. In contrast to manufacturing, where U.S. industry felt that it was in crisis and could readily see that it needed to improve its practices, design was an area where it felt relatively comfortable. I, on the other hand, felt that for large and complex systems the United States was not approaching the design process very well at all. For example, in my writing at HBS I emphasized the importance of designing large-scale systems with built-in flexibility so that their design could be easy changed even after the systems were already in use. I pointed out that it was often important to design a family of related products, rather than just one product at a time. For example, the Sony Walkman uses a basic design, a platform that is changed every two to three years with new technology. Each new platform is designed so that over a hundred different Walkman products can be created based on it. This greatly reduces the cost of designing each new product, albeit at some loss of efficiency. Americans had tended to emphasize the loss of efficiency in the use of platforms as bases for product design. I recall this argument being made about the high-level computer language FORTRAN in the 1950s. Now almost all software is written in one high-level language or another, partly because we have learned how to eliminate the loss in efficiency in most cases, and we recognize the need for flexibility, at least in such information-centered systems.

The automobile industry took nearly a century to recognize the value of platforms in car design, but it still has some way to go to fully buy into the concept. In particular, I find it hard to convince mechanical engineers of the value of a pyramid of platforms, or even just two platforms stacked together. Other industries still have not made the change to a single platform, let alone a hierarchy of them. Of course, our educational programs in engineering, either at the undergraduate or graduate levels, hardly dealt with these issues. An exception is Chapter 2 in our introductory CS course, 6.001, but then I had a bit of influence over the structure of that chapter.

I brought up these issues in my seminar at HBS in the spring of 1990. I asked some of my colleagues at MIT, such as Rod Brooks and Bob Berwick, to give guest lectures, but I led the discussion most of the time. The seminar had some non-HBS members. For example, Sarah Kuhn, daughter of Tom Kuhn, the philosopher of science who was a member of my seminar at MIT, attended quite often. At the time I was quite concerned over the success that Japan was having in manufacturing and product design. I felt that the United States would have a difficult time catching up because we tend to emphasize an ideology that prizes individualism, rather than teamwork, which is important in the modern manufacturing organization. Kim Clark came to the seminar one day and disagreed with me. I forget exactly why he felt that U.S. firms would change their practices, although it is likely he was relying on some of his consulting experience, and it turns out that he was right. (Kim later became the dean of HBS.) U.S. firms used their power to lay off workers especially in the recession of the early 1990's to cause the remaining workers to agree to cooperate in the new approaches to manufacturing. The elimination of the "lifetime" job was a tremendous change in attitude and practice in this country. I was surprised that it was carried out without too much labor unrest. Nor am I convinced that this is the best long-term approach to labor/management relations. Yet once the changes were made in the early nineties, the improved manufacturing and general management processes of U.S. firms gave them a boost that carried them to world leadership in many industries for the following decade. The Japanese, on the other hand, suffered greatly due to the sudden drop in their real estate valuations. Lester Thurow, who was dean of the Sloan School at the time, said that real estate in the center of Tokyo was so expensive that New Zealand was able to sell its embassy there at the height of the market in 1989 and nearly zero out its national debt as a result. Japanese banks had a great deal of their funds tied up in real estate, but since the Japanese did not believe in firms becoming bankrupt, hardly anyone sold their overvalued holdings, and the Japanese economy suffered as a result. I believe the Japanese economy will recover in the coming years, and that their continued advantages in manufacturing and product design will still have an impact on the world market. The United States has indeed learned from the Japanese, but I feel that we still have more to learn.

I was surprised to receive another call from the presidential search committee inviting me to meet with them again in the spring. Mistakenly thinking that they wanted to reconsider me as a candidate for president, I thought long and hard about it, and decided that if they did not have better candidates, then I would do it, although recognizing my weakness in fund raising, I wanted the committee to recommend recreating the post of chancellor. My notion was that the chancellor would be concerned with budget issues to some degree and with fundraising to some degree. It turned out that the committee had decided to look outside the Institute, since any insider would have been viewed, correctly, as a second choice to Phil Sharp. Thus I was probably being looked at for being on a short list for one of the other jobs, such as dean or provost. I should have understood this, since Carl Mueller was not present at the interview, and the committee reminded me that I was not interested in the job of president earlier in the year.

A few weeks later it was announced that Charles Vest, provost at the University of Michigan, would become the next president. I had met Chuck Vest at Stanford earlier in the academic year, since both of us were on the Advisory Committee to Jim Gibbons, Stanford's Dean of Engineering. I had heard that he had just become provost and asked him what it was like to be one. I do not recall his answer.

In 1989–90 HBS was undergoing major building construction and renovations under Dean MacArthur. The Baker Building was to be renovated, and the entire Production and Operations Group under Bob Hayes would move into one of the graduate student dorms in the meantime. Since I was to leave in July, I wasn't assigned another office. I decided to vacate my office a bit early. Hayes invited me to lunch at the HBS Faculty Club to review my year's work. I think he was annoyed that I did not stay the entire ten months, the normal academic year at HBS. I enjoyed the sabbatical, I told him, I appreciated his hospitality and support, but it was time to return to MIT full-time. I also explained the reason for leaving in mid-June, rather than in July.

David and Jesse's Bar Mitzvah

A major family event that began during my sabbatical and continued into the fall of 1990 was the preparation for our sons' Bar Mitzvah. We decided that Jesse and David would share in doing nearly the entire Saturday morning service. In Orthodox synagogues boys normally chant only the Haftorah, the reading from the prophets that follows the reading from the Torah; in Conservative synagogues boys can be expected to do much more. Our children had the advantage that they could split the workload. The Hebrew School they attended on two afternoons a week plus Sunday morning had prepared them for the usual prayers, now what they needed to learn was how to chant the Torah and the Haftorah. Since their birthdays occurred in August, when many people were away, we scheduled their Bar Mitzvah to be right after the Jewish New Year's holiday season. Their Torah portion would be some of the opening chapters of Genesis, including the story of the Garden of Eden.

Chanting the Torah is a nontrivial art. The Torah scroll is written without punctuation, and without indication of vowels or how one is to chant each word. Thus one has to memorize a great deal so as to avoid mistakes. A member of the congregation, Lowell Bensky, held a Torah chanting class on Saturday morning at 9AM, and Jesse, David, and I attended it faithfully for a year. I had not really learned how to chant the Torah in Israel, so I learned along with the boys. Bensky liked to encourage the boys and girls in the class; his method was if they did well, he would say so, if they did not, he would invariably say, "Coming along." For months Jesse, and especially David, would hear "Coming along." As the year wore on I became nervous that they could not read the Torah, so I began to practice at home with them. Toward the end of the year they learned how to say their Haftorah, a relatively easier task than the Torah.

We invited about fifty people to the service and to the lunch afterward. Jesse and David performed very well. I did not notice any mistakes at all, even though David was especially nervous and had to be given a drink of water. Some of our guests said that Peggy and I were beaming after the boys did their Torah reading. Later David said that I had put too much pressure on him; he would bring this up for years. The Bar Mitzvah celebration was the high point of the boys' Jewish education. They stopped going to Hebrew School after the school year ended the following June.

Lexington had two middle schools, Clark and Diamond. These covered the sixth, seventh and eighth grades. David was not happy with the Waldorf school, since it did not emphasize content to the extent that the Montessori school had. In particular, reading was not emphasized until the third grade. So Peggy and I decided to move them both to the middle schools after the fifth grades, and avoided issues related to their being identical twins by placing them in different middle schools. The Lexington school system understood our concerns, and let us do that. The Bar Mitzvah occurred when each of our sons was in a middle school. Since there was just one high school, a very good one in fact, we had a decision to make in a year or so later.

Back to the Classroom, 1990–1991

Chuck Vest had a temporary office near the main MIT library during the summer of 1990, prior to his official start date at MIT. Here he interviewed over a hundred people to familiarize himself with the issues and personalities at MIT. I was one of the people whom he interviewed during the summer. He reminded me of our meeting at Stanford, but I had not forgotten it either. I told him why I liked MIT so much, and what issues needed to be addressed, such as large-scale systems, communications systems, and other interdisciplinary activities. I also mentioned to him the need for MIT to have a greater presence in Washington. He took careful notes in a notebook, and I noticed that he had filled out quite a few of them already. Vest was a great listener, as I was to discover.

I have known four MIT presidents—Howard Johnson, Jerry Wiesner, Paul Gray, and Chuck Vest. Each was different, and each was good for MIT. Howard was just the right person to head the Institute in the difficult times of the late sixties, a time that also saw the end of a significant growth period for MIT and other research universities. Jerry was a great intellectual leader, but there would be little growth in the 1970s, which probably frustrated him greatly. Paul could manage an institution very well in an era of budgetary constraint. He was concerned with undergraduate education and the health of the overall MIT community. Chuck arrived at the end of three constrained decades. He appears to me to be a model of modern university presidents. In his era MIT has broadened in many incoming students' minds from the leading American engineering school to one of the top five or so American universities of any kind.

I was assigned to teach two recitation sections of 6.001 in the fall of 1990. I had not taught a regular subject since 1977, so I was a bit nervous. Although I viewed myself as an expert on LISP, the language on which Scheme, the 6.001 language, is based, I had not taught the course previously. I had sixty students and a TA to grade their homework. Hal Abelson and Eric Grimson gave the main lectures, which were basically performances in front of an audience of three hundred students. For example, in one of the lectures, Hal put on a cape because he was to play the role of a magician who handles the interpreter for Scheme. Eric told jokes, usually bad ones, which the students came to appreciate. Since I was, so to speak, the sole repository of early LISP history left on campus, I decided to spend a few minutes in each of my sections describing the history of the material that we were studying. This meant little to the students I found out, since today's MIT students care, not for history, but for what will be on the next test. The student evaluation at the end of the term gave me a 5.8 out of 7, good but not outstanding, and a number of students commented about the "irrelevant" historical remarks that I had made.

Early in the fall term I received a call from Chuck Vest, who wanted to interview me for the position of provost. I dutifully went and was interviewed. I don't recall the questions he asked, although he did tell me that Mark Wrighton, head of the Chemistry Department, Sheila Widnall of the Aero and Astro Department, and I were his finalists. Vest was to take over in mid-October. I had not heard anything by the weekend before this. I talked to Sheila, who had not heard either. We decided the choice would be Wrighton, and so it was, creating a balance between engineering and science at the top of the new administration. I was to learn that Vest tends to make such personnel decisions relatively close to the deadline.

Dean, School of Engineering, 1991–1995

Gerry Wilson announced during the fall term that he would resign as dean in mid-January of 1991. Mark Wrighton appointed Steve Lerman of the Civil Engineering Department as chairman of the search committee, and I was interviewed during the fall term. I must confess that I viewed myself as a natural candidate for the deanship, in contrast to the position of president or provost. Hearing a rumor that the committee viewed me as a "know-it-all," and wanting to demonstrate that I wasn't, I memorized a number of stories about real know-it-alls. One of the best ones was a story about three Oxford dons, one of whom was a know-it-all. They would all go to lunch together each day, and no matter the topic of conversation the know-it-all knew it all. One time the know-it-all went on vacation, and the other two decided to gang up on him. They studied a topic in the *Encyclopedia Britannica*, read the references, and when the know-it-all returned the discussion at lunch soon turned to that topic. The banter went back and forth, one of the pair showing off a bit of knowledge and being trumped by the next one, and so forth. The know-it-all sat through all this and said nothing. Finally, one of the pair turned to him and said, "Normally you are the life of the party, and know more than anyone else about the topic at hand, but today you have said nothing. How come?" The know-it-all replied, "I have been trying to figure out all this time why you two spent so much time analyzing *my* article in the *Encyclopedia Britannica*." Now *that* is a know-it-all, and I am not in that league at all. Carl Mueller had been quoted earlier as saying that he thought I was a bull in a china shop. I guess being a know-it-all is better. Actually, there is some truth to all these characterizations of me. I do seem to know a bit about many subjects, and don't make any great effort to conceal it. This has not helped my sons over the years, since I have not learned to avoid showing my knowledge, and their self-confidence has sometimes suffered, as their mother has remarked on occasion. On the other hand, such a failing probably would not have affected my performance as dean very much.

Of course, during the interview I was not asked in so many words if I was a know-it-all. I recall being asked whether I thought it was a good idea to begin the fall term prior to Labor Day, as many universities had done. I thought that it was a good idea because the fall term was so cramped, but one of the committee members got very excited and said it would destroy family life to begin the term before Labor Day. In any case, I felt that I had made a poor showing, and told Peggy so. We went out to dinner, and she said that I was being too negative, and that I would get the position after all. I was so sure that I would not that I signed a check saying, “If I become dean, I will diet every day.” Peggy has kept the check, and used it to taunt me for years.

Apparently I did alright after all, because Wrighton called me for an interview a few weeks later. He was under the gun to make a decision since Gerry’s deadline of January 15, 1991 was quite close; I was finally offered the position about January 12. They must have obtained approval from the Executive Committee of the trustees via fax. The offer was to be effective on the 16th, which I think was a Tuesday. Over the weekend I called Donna Savicki, the Assistant Dean, who had been informed also, probably by Doreen Morris, the Assistant Provost. I asked her how I could get into the office that day. She said to come after 9AM and the doors would be open. This is how everybody else in the dean’s office found out who the new dean was. Wrighton was at the meeting of the Academic Council, chaired by the president, which is held in the dean of engineering’s conference room. I was told to stay in my new office until 10:15 because Mark wanted to announce my appointment. Then I could walk into their meeting and be seated at the table. Thus in a period of 13 years I sat in three different chairs in that room, one for associate department head, one for department head, and now one for dean. It was not to be the last chair either. Interestingly, all three chairs were on just one side of the table.

I learned what the reason was for the January 15 date. Gerry had a created a couple of committees to investigate a tenure case that was turned down and in which the decision was challenged. The deadline for submitting the reports on the case was January 15, 1991 and he did not want to saddle his successor with the issue, although it did not go away entirely with those reports.

One of the things I wanted to accomplish right away was the creation of a faculty lunchroom. The Faculty Club, on the top floor of the Sloan School, was quite far from the buildings in which most of the engineering departments were housed. In addition, Wrighton closed down the Club for lunch for individuals or small groups of faculty members because it was constantly losing money. There was some space near the main engineering buildings that had just been vacated when the LFM program was moved by Wrighton to a building near the Sloan School. It was an interior room, but it was large enough, about a thousand square feet, for fifty or so people at one time, including buffet tables. I made a deal with the MIT food services. It was going to cost us about seven dollars a person for the food and a server to stock the buffet. I felt that we should charge no more than two dollars per faculty member, in order to attract them to the lunchroom. I asked for the tables to be long ones, which would make each person sit next to several others. One of my goals was to increase collegiality across the Institute, and I felt that long tables would help. I was willing to spend some of the dean's discretionary funds on the lunchroom, and I asked the other deans to contribute a bit also. We sent a letter to the entire faculty, and managed to get started that spring. The lunchroom has since moved to a larger facility, one more central to the Institute's faculty, and most recently found a home in the new Stata Center. It has been quite successful from the start, often attracting 15 percent of the MIT faculty on a given day. It is likely that creating the Faculty Lunch Room will be my most lasting legacy at MIT. For a while some departments, such as Chemistry, Mathematics, and Mechanical Engineering had captured a table largely for themselves and their colleagues, thus reducing the random interaction aspects of the previous lunchroom. Some women faculty members have found the lunchroom atmosphere not to their liking, but this is an issue that I believe is no longer the case in the Stata Center. The price of a meal briefly went up to four dollars, and when the attendance dropped markedly, was reduced to the three dollars, where it stayed for a number of years. It has risen to six dollars in the Stata Center, but the Institute still subsidizes much of the cost.

I had a number of visitors in my first week as dean. Louis Smullin came, but I could not figure out his agenda. It seemed that he was there in part to show his pleasure at one of his faculty hires becoming dean. Kent Bowen, co-founder of the LFM program, and another candidate for the dean's post, also came. He was surprised that I felt that LFM needed to be supplemented by a program that emphasized design, and invited me to meet with Tom Magnanti, from the Sloan School, the other co-director. We met in the LFM "War Room," where their goals were pasted on the walls. They explained what they were doing and I reiterated my position. Kent eventually took a position at HBS with the same POM group with which I had spent the sabbatical. Some of my friends in the school gave me a party that first week. Hal Abelson gave me a book, *Management Secrets of Attila the Hun*. The book claims that Attila was not all that bad, but I do not recall what his secrets were.

Jimmy Wei also came that week. Jimmy was the head of the chemical engineering department. He told me that one of my goals as dean was to foster education and research related to the environment. He noted that Harvard was getting into this area, in part through their Kennedy School of Government, and that while Princeton University had an edge in global climate modeling systems, MIT had an edge based on the strength of our engineering programs, if only we would get our act together. I did not realize until that point that the School of Engineering had a mission in the environment area, but I took on the task. One of the problems MIT had is that the faculty and programs related to the environment, just in the School of Engineering, were widely dispersed in several departments and centers. How could we pull it all together? My idea was similar to what Paul Penfield did in the VLSI program: a virtual center. A virtual center has an education and research mission, but does not own significant space or control faculty positions. In a sense a virtual center is an overlay on the matrix of departments and centers, and is in a third dimension of organizational structure in a university. Virtual centers provide a flexibility lacking in department or center structures. MIT has found it difficult to eliminate departments, partly because of the complaints by alumni that their degrees are made less valuable when their departments no longer exist. Centers at MIT have been difficult to eliminate since they control space and usually manage to continue to get a stream of funding from outside sponsors. Virtual centers are intended to be temporary organizations, but a number of them have been so successful that they have become permanent features of the MIT landscape. In short order we created PEEER, the Program in Environmental Engineering Education and Research, and Dave Marks, Head of the Civil Engineering Department, was made its director. Dave was an outstanding choice, because he was on friendly terms with all the various parties that compose the community of MIT faculty interested in the environment. Dave is also a great raconteur, so I asked him to begin meetings of the Engineering Council with a joke. He used to say that this request made him very nervous, but he came through with good jokes every time, and as a result we began meetings in a good mood. Dave's virtual center, PEEER, was so successful that we later transformed it to the Center for Environmental Initiatives. Even later CEI was combined with the Energy Laboratory to form a single center at MIT on energy and the environment.

Speaking of Dave Marks reminds me of some jokes about deans, although he is not their source. Gerry Wilson used to tell the following variation of a lawyer joke: "What is the difference between a dead dean on the road and a dead deer on the road? There are skid marks in front of the deer." Then there is the old chestnut: "Old deans never die, they just lose their faculties."

The dean of engineering has traditionally had an associate dean. I knew whom I wanted for associate dean—Dave Wormley, head of the mechanical engineering department, who had also been a candidate for the dean's position. I was grateful when Dave accepted the position of associate dean. Unfortunately, he needed to remain as head of ME while a search for his replacement took place. Dave was very helpful to me in various ways, not least in explaining the intricacies of promotion and tenure cases in technical areas that I did not understand very well. We created a search committee for the head of ME under the chairmanship of Bora Mikic.

One of the things that surprised me as dean is that search committees are often split along personality or subfield lines. The people who would naturally have been candidates for department head in the early 1990s would likely have been hired in the 1970s, but we hired relatively few faculty members in engineering from 1970 until 1978. In any case, there was a bit of a split in the ME search committee, but I went with the choice of the majority of the committee and proposed Nam Suh to Mark Wrighton. Nam had made a reputation by creating a consortium of companies in the area of manufacturing related to plastics and similar products. He had also been assistant director of the National Science Foundation, in charge of engineering. The ME department was composed of a dozen groups of faculty that tended to hire and promote faculty as a unit, an approach that made it difficult to hire faculty outside the various groups. In addition, it was not easy to hire and retain women and minority faculty under such an arrangement. The following year Nam proposed a restructuring of the department, giving more power to relatively young full professors than had been the practice. Such a proposal did not sit well with a number of the ME faculty and, with Nam's tenure as department head in doubt after a scheduled meeting of the ME Visiting Committee, Dave Wormley and I interviewed most of the faculty in ME. In the end we retained Nam as department head, and I am glad we did. He became a creative fundraiser, enabling the department to refurbish several of its laboratories. He has led a major change in the undergraduate program, the first in twenty-five years. The department has appointed a number of faculty members with experience in non-traditional ME areas, which will, I believe, continue to keep the department in the forefront of ME departments around the world.

Just as Wormley and I were interviewing the ME faculty in the spring of 1992 he informed me that he was leaving MIT to become dean of engineering at Pennsylvania State University. Dave claimed that he hadn't intended to leave, but that significant efforts had been made to get him to go to Penn State. I wished him well, and he has indeed done well at Penn State, where he is still the dean and in charge of engineering programs in a number of campuses around the state. I have been told that his office complex is the size of a football field, an apt remark at Penn State. I now needed another associate dean. Mert Flemings, the head of the Department of Materials Science and Engineering, recommended that I appoint John Vander Sande. I took his advice and John was an outstanding associate dean and even an acting dean on two occasions. John led the dean's office activities in education, and very much like Wormley helped me understand research in areas far removed from my expertise.

Budgeting for the Early Nineties

University finances, especially MIT's, had been constrained since 1968, the year Congress passed the Mansfield Amendment. During the early 1990s MIT's budget had a deficit (known as a shortfall in our parlance) of about $10 million a year out of a total annual budget (including the Lincoln Laboratory) of about a billion dollars. This shortfall was made up using invested funds and in rare cases from funds that were functioning as endowment. Tne latter are funds that were unrestricted when initially given to the Institute, but were treated as endowed funds following that point. Mark Wrighton and his budget analysts felt that the annual deficit might rise and in any case needed to be reduced if not eliminated. He thus proposed a three-year effort to reduce the MIT budget by about six percent, or two percent in each of three years. While these reductions were not to be taken equally from each unit, the School of Engineering, being a large unit at MIT, needed to reduce its total budget by six percent. Since I agreed with Gerry Wilson's earlier conclusion that MIT needed to reduce its expenses, I acceded to this reduction. EECS had a novel way of dealing with their own reduction. They negotiated a deal whereby the expected growth in income from the new Master of Engineering program would compensate for almost all the decline. Most department heads avoided reducing the faculty headcount in this reduction process, and the cuts were made in the area of teaching assistants, secretaries, in other staff categories and materials and services.

For years people felt that one way of reducing the budget in the School of Engineering was to eliminate departments, by which they usually meant nuclear engineering and ocean engineering. Nuclear engineering was associated with an industry with significant problems: no new nuclear power plants had been ordered in this country in some time, and a number of older plants, even ones partly constructed, were being decommissioned. There were only three or four ocean engineering departments in the United States, and the number of new undergraduate majors in Ocean Engineering was usually in the single digits. Thus there were good reasons why people talked informally about eliminating these two departments. One problem was that there was little to be gained financially in the short run if the departments were to close down, largely because MIT policy held that tenured faculty—and most of the faculty in these departments were tenured—had to be retained by the Institute. Wrighton and I discussed this issue and agreed that if a change were to take place, it would be best to attempt to change the Nuclear Engineering Department’s structure. We both knew that recent attempts to close down departments had run into serious difficulties. In particular, Wrighton’s predecessor, John Deutch and his Dean of Science Gene Brown, set out to close down the Department of Applied Biological Sciences and ran into a buzz-saw, although they did succeed in closing it down. A faculty committee, under the chairmanship of Sheila Widnall, created a complex process that a provost had to follow thereafter if he or she really wanted to make an organizational change, such as closing down a department. Nevertheless, we wanted to consider a significant change in the structure of the Department of Nuclear Engineering.

I thought that the best idea would be to make the Nuclear Engineering faculty a subunit of the Mechanical Engineering department, to which Nam Suh, head of ME, agreed. In fact, a number of similar mergers had taken place at other universities. Then I met with the Nuclear Engineering faculty at one of their monthly dinners to propose such a merger. It was quickly pointed out to me that nuclear engineering disappeared a few years after each such merger in other universities, and if MIT closed down its Nuclear Engineering Department this would send a very negative signal to academia and industry. It was also argued that the health-related activities in NED would not fit in well in an ME department and that there was opportunity for growth in such areas in an independent department. I noted that it was not likely that future deans of engineering would hire new young faculty to replace ones that retired, and total faculty size in an independent NED would seriously decline over the years. My feeling at the end of this dinner meeting was that the faculty was willing to take the risk of a decline in its faculty size. We also discussed these issues with the NED Visiting Committee, and many of the points that the faculty expressed arose at that meeting, especially the signal that would be sent outside MIT if we went ahead with the proposed closing of the department. As a result, we decided to not close the department or merge it with ME, but to reduce its size by attrition. A somewhat similar arrangement was later made with the Department of Ocean Engineering, and these changes contributed somewhat to the six percent budget cut for the School.

Accreditation and its Discontents

Gerry Wilson used to complain about all the invitations he received to meetings of deans of engineering, especially when they were to be held in places like Hawaii. He simply refused to go. I took the position that MIT ought to attend a number of such meetings and attempt to lead the greater community in new directions in engineering education. One such meeting of deans is called the Big Ten Plus group. This group began with the deans of the Big Ten schools in the Midwest and then added a few others, such as Stanford, CalTech, and MIT. We would meet about twice each year, with the newest dean acting as host and chairman. One issue that was critical at that period had to do with accreditation. For many years there were complaints about the nature of accreditation in engineering by the accrediting body, ABET, the Accreditation Board for Engineering and Technology. The complaints often had to do with the bean-counting aspects of the process. For example, members of the accrediting team would study transcripts of some former students to see if they had taken enough units in engineering design. Accreditation is supposed to make sure that all schools meet minimum standards. This is clearly important in many of the 300-plus schools of engineering. The top two dozen such schools, including the Big Ten Plus schools, did not feel that they needed to be accredited in this fashion. What was galling to some of the deans was when the visiting teams decided that some departments in major schools did not adequately meet the standards. Gerry Wilson took on this accreditation issue, and was surprisingly successful. Gerry met with officials from ABET at LaGuardia Airport shortly after a visit by ABET to MIT in the late 1980s. He threatened to have MIT leave the ABET system unless ABET changed its procedures radically. The MIT engineering department heads wished him well, but expected little action from such a conservative body. We were wrong. ABET decided to undertake experimental site visits in which the team would try to gauge whether the school being visited met its stated educational goals, and in return the team would reduce its bean-counting. One of the first such visits took place shortly after I became dean, and it was nearly a love-in. The chair of the visiting team told his team members what they were to look for, and even the Civil Engineering department, usually the most difficult to accredit, passed with ease.

The Big Ten Plus deans were quite skeptical of ABET's claims that it was willing to consider changing its methodology. Given my recent experience, I was relatively optimistic, taking the position that we ought to give ABET a chance to change its approach. ABET had formed committees to recommend a change in its processes and goals, and I knew some of the members, in particular Joel Spira, CEO of Lutron Corporation, which makes light dimmers. The two Joels would discuss in longish telephone conversations what the deans really wanted, and what ABET's councils were likely to buy. ABET's councils were dominated by representatives of professional societies, such as the American Society of

Mechanical Engineers. They were often from industry, and had less sympathy for academic needs than might be expected. On the other hand, these industry representatives had just been undergoing great changes in their firms as a result of competition from other countries, especially Japan, so they were sympathetic to the notion that auditing organizations—and ABET could be viewed in that light—should help the firms being audited or accredited, using best-practice information, for example, and de-emphasize looking for departments or students who did not meet every specification in exact detail. Analog Devices' auditors had just agreed to such a transformation in their approach, relying on the internal processes to check for errors. In the proposed changes in ABET's processes there would still be some checking of standards, but less than in the past. Mostly the emphasis would be on letting each university decide what it felt it wanted to impart to its students, and check to see that this was in fact done. This process of checking led to the major controversy with the proposed ABET changes. ABET let the institutions decide how to determine whether they succeeded in achieving their goals. One way would be to have students keep a portfolio of their exams and projects over four years, but this is not the only way. I was concerned about this aspect of the revised ABET standards, which led to a semi-comical situation. Chuck Vest was co-chair of the committee that was to recommend the final ABET changes. He called me from Washington while he was in the middle of the final committee meeting, to ask my opinion of the changes, but I was also in Washington, at another meeting. When my assistant called me there to tell me what Vest wanted, I tried to reach him and tell him to hold up on the portfolio-like recommendation, but I got through to him too late, and the full set of changes was forwarded to the ABET councils. Needless to say this aspect of the ABET changes has caused the most concern over the past few years. However, I believe that on the whole the engineering schools have been well served by the ABET changes. Given recent events in industry where auditors were in conflict in some cases with the firms being audited, there may need to be in the coming years another look at the university accreditation practices, although the two processes are surely not identical.

The Engineering Systems Movement

The changes in ABET presaged a larger transformation, which I had seen coming. Engineering education underwent a major transformation as a result of the Second World War. Some engineers noticed that scientists appeared more capable than they in dealing with new problems, such as radar. At MIT, for example, the radar work was done in the Radiation Laboratory, and its leaders were largely physicists, rather than electrical engineers. Gordon Brown, when he became head of the Electrical Engineering Department, decided that a major change was needed in EE education and asked a number of younger faculty members, including Bob Fano and Dick Adler, to lead the effort to rewrite the undergraduate textbooks in EE. This led to the green-book series of texts mentioned earlier. Brown’s changes were part of a national pattern that came to be called Engineering Science, an increase in the amount of science, especially physics, and mathematics that was required of all undergraduate engineering students. The Engineering Science transformation was largely in place by 1960. By 1980 we began to recognize some of the weaknesses of the approach. In particular, in order to make room for increased mathematics and science, some topics had to be eliminated or reduced from the prewar programs. These included emphasis on design, manufacturing, or engineering management. Policy studies or research and education related to the environment probably were not taught at all before the Second World War. A movement, let me call it the Engineering Systems movement, began to deal with these issues, but the movement encountered strong resistance from the adherents to engineering science. Engineering Science is a reductionistic approach that fit in nicely with the positivistic attitudes prevalent in the post-war years. Moreover, engineering science was very successful in many cases. For example, much of the progress in electronics and computing can be said to be due to science or engineering science. Many of the criticisms of engineering science came from civil engineers, members of a field that did not flourish in the US in the engineering science era.

As we have seen, the Japanese inroads in manufacturing, especially in automobile manufacturing, demonstrated some of the weaknesses of the engineering science movement, as did the environmental movement. Other complaints were that engineering schools had permitted business schools to take over education in engineering management, and my own claim that engineering schools did not pay enough attention to the design of large-scale systems and products. All these issues came to a head in the early 1990s, at MIT and elsewhere. At MIT we dealt with engineering policy issues through the Center for Technology and Policy Alternatives, founded in the early 1970s by Herb Holloman. Herb was Undersecretary of Commerce and a former president of the University of Oklahoma. His center was formed when Alfred Keil became dean of engineering, succeeding Gordon Brown. (Actually, before Keil Paul Gray had served a term as dean when Jerry Wiesner appointed him chancellor.) It is fair to say that nearly every dean since Brown has tried to deal with the successes as well as the excesses of the engineering science movement. Keil was previously director of the David Taylor Model Basin of the Naval Research Laboratory, which I once toured. The Navy uses the enormous basin, holding hundreds of millions of gallons of water, to test models of ship designs. When I toured the facility it was lunchtime. Nothing was going on, and we were the only people around. In the dim light my depth perception became distorted, and I couldn't see that that was water below me. I asked where all the water was. I was much embarrassed to be told that all the water was just beneath me in the basin, where it was supposed to be.

How his experiences with the Taylor Model Basin affected Keil I do not know. But he had an attitude that was quite different from that of classic engineering science; for example, he talked of creating departments of transportation, energy, computation, and the like. Of course, he was going to eliminate the traditional departments in order to create the new ones, and this did not make him many friends in the faculty and among the alumni, and the idea did not go far. Bob Siemens as dean also supported policy-oriented work, and so did Gerry Wilson, to a degree. Gerry, of course, championed the manufacturing initiative, and I championed initiatives in the environment and large-scale design. Thus there was quite a bit of activity at MIT that was not classic engineering science. Now it was time to consolidate these changes at MIT if not elsewhere.

American Society of Engineering Education

One of the meetings of deans that I attended took place at the Illinois Institute of Technology. The IIT buildings were examples of modern architecture initiated by the Bauhaus movement. They had flat roofs, for example. This in a region with lots of snow each winter, so I suspect that the flat roofs are not ideal, but that is what happens when ideologists get their way. I spoke up at the meeting, and as a result I was asked whether I wanted to become a member of the Board of the Engineering Deans' Council of the American Society of Engineering Education. I felt that the MIT dean ought to participate in such activities, so I agreed to be nominated. I eventually was elected Vice Chair of the Deans' Council, but did not serve since I became provost at just that time.

One of the activities of the ASEE was a meeting sponsored by the National Science Foundation regarding the future of engineering education. I spoke at this meeting on the need to move away from a pure adherence to engineering science. Many others said similar things. I believe that the report of this meeting actually had a somewhat negative effect on MIT. A couple of years after the meeting, it was announced that the Olin Foundation would create a school in Needham, Massachusetts to teach engineering in the manner that the NSF meeting had suggested. The Foundation felt that MIT was too set in its ways, and could not make the appropriate changes, and a new institution had to be created for over $200 million. Vest and I were not thrilled with this news, especially since we felt that MIT was leading in this approach to engineering. In fact, I wrote a report based on the School's Planning Meeting in 1993 that clearly emphasized the new direction. It was entitled “Engineering with a Big E,” a title stolen from LFM, which used the phrase “Manufacturing with a Big M.” By this LFM meant that they were teaching about not just manufacturing, but about the whole enterprise, including manufacturing. I wanted MIT to consider engineering from a broad perspective too, that is, not just engineering science, but also issues such as manufacturing, design, the environment, policy and the management of engineering. I was now ready to consider various ways in which these new directions in engineering education could be coalesced.

The System Design and Management Program and Distance Learning

One of my goals as dean was to develop a program operating in parallel with the Leaders for Manufacturing Program which would emphasize the design of large-scale systems. Lester Thurow, Dean of the Sloan School, was interested is cooperating, in a manner similar to the way Sloan cooperated with the Engineering School in the LFM program. He asked Tom Magnanti to help with the design of this program and its marketing. Tom was of enormous value in getting the new program, eventually called the System Design and Management (SDM) program, off the ground. We held many meetings with industrial colleagues to discuss the need for an SDM-like program. One of the points that were made over and over again was that design engineers with 5–10 years experience, who we all felt were the students we wanted for SDM, could not leave their firms for more than a few weeks at a time. MIT's minimum residency requirement of one semester for any degree recipient seemed like a deal-breaker to industry leaders. Tom devised a plan in which students would spend six weeks on campus on a couple of occasions plus a week here and there, thus getting the equivalent of over one semester on campus. This would not have been satisfactory to our colleagues at MIT, so he added a one-semester-in-residence requirement at the end of the program. This was finally acceptable to industry. Now we had to sell it to the various committees on campus.

If students were to spend only one full semester on campus, how were they to take all the subjects in engineering and management we would require? We decided that they would do so via distance education. SDM thus became the first MIT program that relied on distance learning. We needed a number of classrooms that were connected to satellites to provide the real-time feedback from students located in plants around the country. Fortunately, the Center for Advanced Educational Services was interested in converting their outmoded lecture hall into two levels of classrooms. An investment of $2 million, largely from provost's funds and partly from the Ford Motor Corporation and the Kauffman Fund, eventually paid for these renovations.

The SDM core courses emphasized issues such as system architecture, system engineering, and project management. The latter two courses have been taught elsewhere, but the former course was relatively new. As far as we could tell the only place a similar course was taught was at USC. Since the instructors of the system architecture course, initially Ed Crawley and I, did not know exactly what should be taught, our hope was that we would learn as we went, partly from our students. The average age of the students in SDM was 35, and thus they were relatively experienced in product design, although they had probably never sat down and tried to abstract the issues in this field. One way to gauge the initial success of SDM is to note that its original co-directors, Magnanti and Crawley, became dean of engineering and head of the aero and astro department, respectively. Magnanti was also appointed Institute Professor, in part because of his tremendous contributions in founding LFM and SDM.

Our introductory class had about a dozen students. The classes in the next few years had around fifty students. Since the program lasts two years, there are around one hundred SDM students at any one time, a similar number to the number of LFM students. An advisory committee to our new Center for Integrated Product Development told us circa 1995 that fifty SDM students a year is simply too small a number, given the needs of American industry for engineers with a broad view of product design, and suggested that the country might need many times as many graduates of SDM-like programs. MIT would never attempt to teach that many students in our degree programs, of course, so we were asked to work with other universities that could offer an SDM-like program. NSF gave us seed funds so that we could teach faculty in other institutions how to offer these courses locally, and we proceeded to do so. MIT has undertaken to teach faculty in other U.S. universities at various times in the past few decades. While I was head of the EECS department, we offered our 6.001 course to other MIT professors, as well as professors from around the country. The first time we did this we arranged for the professors to stay in a nearby motel for a week. A while later one of the student-professors gave me a check for $100,000, saying that MIT had never done anything as good for him before. The money was to be spent on similar programs by the department. I was able to convince the donor to allow us to spend the income on annual gatherings of the department as well. Given the success of the 6.001 offering, we also offered other subjects, such as signal processing. The technique we eventually began to use was TVI, tutored video instruction. This technique, developed by Jim Gibbons at Stanford, relies on tapes of lectures. The tutor plays about ten minutes worth of tapes, stops the tape and gets a discussion going about the material The interaction leads to a very good education. Professors do not mind learning new material, as long as there are no young students around who might embarrass them.

Adding Divisions to the School of Engineering

Several committees were created in the School in 1994 after the Five-Year Planning meeting. I chaired one that dealt with the structure of the engineering school. Tom Eagar, the new head of the Department of Materials Science and Engineering, chaired one that dealt with the new directions in engineering that we have been discussing. My committee on structure met even after I became provost in June 1995. I had concluded that it was too difficult to get rid of structures, such as departments and even laboratories, in a modern university. If you wanted something that crossed existing boundaries you might have to create a structure in a new dimension, and so we came up with the notion of a division—neither a department nor a center, intended to cross departmental boundaries, and with faculty appointments, usually joint appointments with departments. Research centers did not usually have faculty appointments, and thus did not receive much “hard” money from the Institute.

The Eagar committee recommended a divisional structure to deal with issues of public policy, manufacturing, environment, and the like. That is, they wanted an organizational structure for faculty with a Big E orientation. This was eventually implemented in 1998 (universities are quite slow to act on structural change). It was called the Engineering Systems Division. At that point Bob Brown had replaced me as dean. He was also interested in a division in bioengineering that cut across the engineering departments, and was connected to our biology department. Brown also created the dual-faculty concept. This concept dealt with the issue that a joint appointment tends over time to become unequal as the faculty member becomes much closer to one department than the other. A dual appointment essentially guarantees that a faculty member gives approximately half of his or her effort to each of the parties, and that each party expects no more. Actually, the Division of Bioengineering and Environmental Health was the first one to be formed, and the Engineering Systems Division followed it a few months later. Dan Roos became the founding director of ESD. I believe Alfred Keil and Bob Seamans would have been pleased with the ESD concept. On my return from sabbatical in 1999 I would become a dual member of ESD and EECS.

Reengineering

MIT had a Senior Vice President, Bill Dickson, to whom the other vice presidents reported. Bill was a wonderful man. He had been at MIT for about forty years and knew very many people. He was great at solving problems, so much so that I felt his direct reports relied overly much on his problem-solving ability. Bill and Gerry

Wilson were quite close, and both Bill and I, agreeing with Gerry's views on controlling expenditures, were concerned about MIT's financial future. Dickson became interested in using the new management concept of reengineering as a way of reducing MIT's costs. The authors of the first book on reengineering, *Reengineering the Corporation* (Hammer and Champy, 1993), both had MIT connections; Mike Hammer was a former CS professor who left his tenured post in the early 80's. I should have known that the reengineering effort would not be easy when Hammer gave a one-day seminar at MIT on the topic and told us to "shoot the stragglers," that is firing staff members who did not go along with the reengineering process he was describing. I felt then that Mike was just not sufficiently concerned with the way people were treated in the reengineering process. Years later I read an interview with Hammer in which he admitted to such a weakness in the approach. He noted that many people equated reengineering with downsizing, which was not his intention. I felt that the problems were even deeper. Reengineering required a cultural change, one where people were respected more highly than is often the case in a classical hierarchical structure. It seemed to me that it was not consistent to shoot the stragglers in such a culture, or even in the transition to such a culture. Besides, MIT was an institution which tended to respect its staff, and not a run-of-the-mill corporation.

Dickson chose Jim Bruce, an EE professor and Vice President for Information Systems, as the leader of MIT's reengineering activities. He asked me later to join the key oversight committee on reengineering. Believing that MIT could certainly achieve some savings by reengineering its administrative functions, and that if reengineering were done right many staff members would find themselves more empowered in their jobs, I agreed to join the committee. This was at about the time that a subcommittee, chaired by Katie Cochrane, was to report on the potential savings to be had from reengineering. The goal was to achieve a $40 million dollar per year savings at an initial one-time cost of $40 million. Katie's committee and their consultants from a local firm, Index Systems, thought this could be done, largely via a process of reengineering management reporting. By management reporting Cochrane's committee meant the various activities at MIT that included financial bookkeeping in central units as well as academic ones, appointment of students as research assistants, and the like. It was shown that it took as many as nineteen pieces of paper to appoint a student as a research assistant. What surprised me was that the bulk of the savings were to come, in their analysis, not from staff reductions in central administrative functions, but through an accumulation of many small reductions in time spent in departmental administrative activities. In addition, it was felt that additional significant savings could be achieved by combining the administrative functions in several academic departments, laboratories, and centers. There were two key problems with this analysis. One was that many administrative functions, including keeping track of research accounts, were being done in departments by secretaries. The number of secretaries at MIT and elsewhere was declining, partly because word processing had become so easy that faculty members typed their own letters and papers. In addition, Mark Wrighton's plan to reduce budgets by six percent over three years was implemented by many departments by reducing the number of support staff. This meant there were relatively few secretarial positions still available to be reduced by reengineering. The second issue was that academic units were proud of their autonomy and did not wish to have their administrative functions combined. The School of Architecture and Planning had undergone such a combination with some difficulty, but there were not many others lining up to undergo such changes. I therefore felt that while reengineering was a worthwhile goal, the total expected savings would likely not materialize.

At the same time, it became increasingly clear that the software underlying MIT's central accounting systems needed a major overhaul. Because the systems were outdated, and their data not easily available in departments, it was estimated that the academic units had created over 150 separate local accounting systems to keep track of research expenditures, for example. If MIT could create a central system that provided timely information we could save a great deal of money for the Institute. The reengineering activities were combined with the updating of the MIT accounting system and with the six-percent budget cuts. MIT was thus undergoing three related and significant changes simultaneously. As some people say, one ought not do two experiments simultaneously. We were attempting to do three at one time.

We analyzed all the available Enterprise Reporting Systems, as such systems are now called, and chose one called SAP. SAP had been successful in manufacturing organizations, but we would be their first American university. They were willing to make a financial deal with us for being first, and we apparently were willing to be early adapters, with all the advantages and disadvantages of that decision. A key reason for the choice of SAP was that they were willing to modify their system to use our security subsystem, Kerberos, a key result of the Project Athena system development. I recall telling Dickson that adopting a system of such magnitude with so little experience with our type of institution is akin to a merger, where most of the surprises are likely to be negative ones. Unfortunately, there were surprises along the way, and I think I was right about the sign.

Eventually it became clear that while there were significant savings in some central administrative areas, the majority of the savings in the departmental areas would not materialize. Moreover, the generality of the SAP software means it often took longer to enter new information than under the old specialized systems. Thus while there was quite a bit more information available to departments and account supervisors, the extra work that many staff have had to perform left many of them (and their faculty supervisors) with a mixed feeling about the overall system. I certainly hope that with time the software will be modified to give the same ease of use as in the past, while maintaining the complex data bases that a modern university needs. Our experience with such systems has been similar to that of numerous other research universities. Each has had to spend on the order of $50 million to modernize their systems, and the process of change has not been easy for anyone, as far as I can tell.

The RA/TA Issue

MIT is a great research university. In a number of research fields MIT has been able to garner 10 percent of the national R&D budget for universities. Research support from the DoD became increasingly difficult to obtain beginning with the Mansfield Amendment of 1968. By 1994 I believe that overall federal support for MIT research declined in real terms by about 25 percent from its level just a decade earlier. In part, this was due to some agencies' desire to "spread the wealth." Moreover, the big growth in funding was in the National Institutes of Health (NIH) for medical research; with no medical school, MIT did not qualify for many of the large NIH research grants. MIT was about to be hit even harder than is implied by the above because of proposed changes in the rules governing overhead and employee benefits. Overhead expenses are real costs incurred by universities in support of research and education. Some costs, such as the cost of the Office of Sponsored Programs, are clearly related solely to research; some costs are intended for both education and research, such as the libraries. A principle created during the Second World War at the Radiation Laboratory is that the government ought to reimburse universities for their direct costs as well as for a percentage of the jointly incurred indirect costs. It is the job of government auditors to make sure that the percentage charged the government is no higher than it should be.

When Reagan became president in 1981 he promised to reduce the cost of the federal government. He did not succeed in this goal, but the growing deficits during his administration created pressure to reduce costs. One of the areas where the Office of Management and Budget thought it could reduce costs was in overhead payments to universities. Two events at Stanford in the late 1980s made this a bigger issue than before. For one, major construction activities on the Near West Campus at Stanford threatened to raise the overhead rate on research contracts to over 80 percent, a record for non-medical activities. The faculty had a mini-revolt when they eventually realized this, and campus construction subsequently slowed. The second event, more critical to MIT, was that an auditor alleged that Stanford had requested reimbursement for some things that should not be reimbursed by the federal government, including satin sheets in the bedroom of Stanford's president. This led the auditors to examine other institutions quite carefully, and since MIT was one of the largest recipients of federal research support, it was high on the list. While MIT had to retract relatively little from its overhead reimbursements, various agencies got into the act, and their overall impact on MIT was decidedly nontrivial.

We estimated that the changes in the rules for overhead recovery and especially employee benefits would result in a loss of over $50 million per year, a huge impact on MIT, at a time when the net undergraduate tuition was only about $75 million a year. The biggest change was in how MIT paid the tuition for graduate teaching and research assistants. As noted earlier, John Deutch managed in the early 1980s, while he was Dean of Science, to get an agreement with the Office of Naval Research that MIT and a few other universities (Caltech, Columbia, and Stanford) could charge these RA/TA tuitions as an employee benefit on all salaries at the institution. The effect was to reduce the cost of research contracts on the main campus and to increase it at Lincoln Laboratory, which was sponsored by DoD. The federal government as a whole did financially well in this change since many of the salaries at MIT are not research-related. In a sense the provost lost some money in order to greatly reduce the cost of research assistantships to the faculty. The military generals overseeing Lincoln Lab and the Lincoln administration did not mind very much paying the extra cost because they understood the importance of the MIT connection to the effectiveness of the Lab. A key problem was that most universities were not permitted to make this change in RA and TA charges, even though some could prove that the federal government would financially benefit from such a change at their institutions, as is the case at MIT. Thus the OMB decided for the sake of financial simplicity to disallow the RA/TA mechanism. I doubt they would have made this decision in an earlier climate for research in universities. The impact of this change alone was on the order of $27 million per year for the main campus, and rising along with the tuition rate. Mark Wrighton convened a committee to study the issue, with Bob Weinberg of the Biology Department as chair and many of the deans as members. At the same time the administration tried, as best it could, to reverse the proposed change in Washington.

The Weinberg committee spent much time analyzing the financial implications of the RA/TA change. If MIT's administration did nothing to mitigate the effect of the change, the cost of an RA would rise to about $50,000 for a 12-month appointment, a level that we all felt would be absolutely uncompetitive at that time. The cost of an RA at MIT with the employee benefit mechanism in place was in the early 1990s already one of the highest in the nation—about $33,000. We estimated that our principal investigators might be able to handle an increase of 10 percent in cost, but no more. How then to handle the shift of funds, which was in the tens of millions of dollars a year? Since Lincoln Lab would obtain a windfall out of the change, we wanted to know whether there was some other change that would keep Lincoln as the bearer of a significant portion of RA tuition costs. The answer according to our internal auditors was basically "no." We also discussed the proposition of whether different parts of the campus should bear equally in overhead costs. Our conclusion was that MIT was one campus with one set of overhead rates. If MIT had had a medical school and its associated hospitals, we might have come up with a different conclusion.

After much discussion in the Weinberg Committee regarding various schemes for handling the RA/TA change, Mark Wrighton came into our meeting and made a proposal that appeared to handle the overall financial problem. His proposal was quite complex, but since we had over three years before the change would finally take effect, the change in any one year did not appear very great. For example, the MIT budget gave the provost $1.5 million each year, intended for new programs, hiring additional faculty, and the like. Mark proposed to devote a third of this, $500,000, for each of the next five years, building up to a $2.5 million-a-year line item in the budget. There were half a dozen such mechanisms. In effect, the provost took over a significant portion of the cost of an RA and continued paying the full cost of TAs. Departments and individual principal investigators were to be affected through a mechanism such as a tax on all expenditures through fund accounts. Fund accounts are distinct from educational or research accounts. They usually arise from faculty chairs, gifts from individuals or corporations toward research, and the like. I would say the faculty did not mind shifting most of the burden toward the provost, but was not thrilled with any mechanism that shifted some of the burden toward them. On the other hand, we were still hoping that the OMB would change its mind regarding the overall issue. We received support from the other universities affected by the change. However, the effect on MIT if the change went into effect would be far greater than on anyone else.

Just as Wrighton announced to the entire community his plan for dealing with the RA/TA changes, he also announced that he would leave MIT to become chancellor of the Washington University at St. Louis. Mark was recently divorced, and this plus the opportunity to head a major university were probably key reasons for his leaving MIT. Wrighton was good for the School of Engineering and has since done very well at Washington University. I was now concerned that the next provost, whoever he or she might be, would not be as accommodating to the Engineering School as Mark was.

Wrighton was due to leave MIT the day after graduation, on a Saturday in early June 1995, but on the Monday before there was still no announcement of a successor. I was called into Vest's office on Tuesday morning, and I expected him to tell me who would be the next provost and why he hadn't asked me. Much to my surprise, he asked whether I would accept the post—I was surprised that Vest broke the implicit balancing scheme between engineers and scientists. I accepted, and returned to my office for a scheduled meeting with a couple of other deans. I had to keep the offer to myself for an hour, so Vest could announce it to Academic Council at its regularly scheduled meeting. Chuck Vest apparently has a tendency to make such decisions at nearly the last minute.

No matter how large the graduating classes grow, no matter how long the graduation ceremony becomes as a result, MIT is careful to keep up the tradition of having each graduate's name announced as he or she mounts the podium, and handing each, not a symbolic roll of paper as in some institutions, but the actual diploma. This is a moment that recognizes each graduate's effort, and is treasured by everyone on campus. On the following Friday, graduation day, I read the names of the recipients of graduate degrees in engineering, about a thousand names, and Wrighton handed each one the diploma. Vest handed the diplomas to undergraduates. I prepared a great deal for the reading of the names of masters and doctorates in engineering, and since many were foreign I prided myself on reading them as close as possible to the way they would have been read in the original language.

Mark and I agreed to meet the next day, Saturday, so that he could give me some words of advice. We met just as he was entering the provost's office. Wrighton told me that he was glad I would become provost. He reminded me that the President reports to the Corporation, especially its Executive Committee, and that I should be especially nice to them. He pointed out that his office had several cases in which legal suits were lodged against MIT, and that I might wish to acquaint myself with their content. After he left I opened his desk, and he had left me a card with a photograph of a play he had seen in London, called "The Provost." It takes place in a jail. Remembering that a provost-marshal in the U.S. Army runs a jail, I laughed. It reminded me of the envelopes I left for Paul Penfield. The first envelope said, "Open this when you have your first crisis." Inside that envelope it said, "This too shall pass." The second envelope said, "Open this when you have your second crisis." Inside it said, "Prepare two envelopes."

Chapter 10: Provost, 1995–1998

I was generally aware of suits lodged against MIT, largely by women faculty members, during Mark Wrighton's tenure as provost. I did not know the details, but reading them made me aware how serious the position of provost was. In comparison, the position of Dean of Engineering was less serious and relatively more flexible. MIT's Policies and Procedures Manual made it clear that certain issues, such as disputed tenure cases, can be brought up as far as the provost and the president. Thus, even if a negative decision were made by a department head or dean, it could be appealed. President Truman said, "The buck stops here"; in our case "here" was the Office of the President and Provost. As most administrators soon realize, much of their time is spent dealing with personnel issues. This is certainly true in the provost's office, although I was lucky in that no one filed a suit against the Institute during my tenure that had to be dealt with by the provost's office. I actually was able to persuade a colleague who was a personal friend not to file such a suit.

Vest told me that my first job as provost was to convince the Assistant Provost, Doreen Morris, to stay on. I, of course, wanted her to stay on. What I learned as dean was how important the assistant deans and assistant provost were in the orderly running of the institution. The other department heads in the School of Engineering and I liked Donna Savicki, the Assistant Dean of Engineering, because she knew how to solve problems in the MIT context; I knew that Doreen had a broad perspective and that the assistant deans respected and admired her. I came to a similar view of her in short order.

During the process of choosing a provost, the new Sloan School dean, Glenn Urban, and I suggested to Chuck Vest that the provost job be split in two, with a chancellor appointed to handle financial affairs, such as the budget. Vest decided not to split the position. This would cause some difficulties, such as pressures on my time, and when Bob Brown later became provost, a chancellor position was created, although the actual split in duties was not what Glenn and I had envisioned some years earlier.

A Complex Agenda

The Weinberg Committee discussions highlighted the fact that in June 1995 MIT was facing significant financial pressures. As dean I had seen the financial projections that MIT used, which were presented to the Executive Committee of the trustees. I felt that these projections were unusually rosy. For example, in the election of 1994 the Republicans won a majority in both House and Senate, and let it be known that university research would be targeted for reduction. I ran a conference for engineering deans in February of 1995 at which John Deutch, then the Deputy Secretary of Defense, was the keynote speaker. John said that based on his conversations on Capitol Hill, there was no bottom to the amount of research support that Congress would be willing to fund. Yet the MIT budget for FY96 assumed annual increases of several percent in research volume, and the accompanying payments of overhead expenses. It also assumed that MIT's reengineering effort would save about $40 million per year when fully implemented; I was not so sanguine about the savings. It further assumed that Mark's RA/TA plan would work exactly as planned; I did not anticipate such a smooth implementation of the plan. Finally, our endowment of $1.7 billion was not high enough to offset significant problems in other parts of the academic budget.

One of my first actions as provost was to ask Jack Curry, the Director of the Budget Office, to help me develop my own spreadsheet version of the MIT budget looking ahead ten years, rather than the normal five. One of the reasons I wanted to look ahead ten years is that some changes, such as the RA/TA change, would become fully effective three years later, and its final impact might not be clear in five years. I also felt that we could no longer constrain a building program—including, of course, the long-deferred building for the CS faculty. Such buildings have historically required sizable capital loans, whose full financial impact does not usually show up until construction ends, several years down the road. When I entered my assumptions of research support, overhead payments, borrowing costs, and the usual MIT assumptions about a continuing five-percent growth in the payout of the endowment, the deficits did not disappear in five years as they were projected by Mark. They grew each year. I began telling Vest in our regular weekly meetings, and again when he walked into my office in the evenings, that we would need a miracle to help us balance the budget in the coming years. I guess many people with the name Moses believe in miracles.

I tried to minimize the impact of my concerns about the MIT budget going forward while I made the day-to-day decisions that a provost needs to make. MIT's academic budget planning has two major phases. One is determining the level of raises for faculty and other academic staff. The second is changes (almost always increases, rather than decreases) in the base budget of the five schools and other academic units, such as the graduate dean's office. Each academic dean gets to choose about two dozen schools against which to compare salaries of assistant, associate, and full professors. MIT rarely had an average salary that was the highest among our peers, but I did not want to lower our position, one that was often in the top five nationally relative to the universities chosen by the deans. During my tenure as provost we asked the Executive Committee of the Corporation to approve average raises that were quite competitive nationally. It was up to the deans to determine the average raises to be given within units in their schools and department heads would then allocate these increases to individual faculty members, giving more than the average to some, and less to others.. The academic council would meet annually to review the proposed raises given to all but a few faculty members (including those in the academic council). In this manner we would learn about faculty who were performing extremely well, and those who were not. We could also use our broader knowledge to reward faculty who contributed to MIT outside their home school. In addition to these annual raises, deans would ask the provost to approve special raises for new hires of senior faculty or to counter offers made by other universities. The number of such requests was actually not very large in any given year, since MIT did not make many offers to outside faculty, and not very many faculty members wished to move from the Institute. Nevertheless, each such hire and especially each departure was remembered by the relevant department head or dean for many years after the fact.

The area where we could be accused of being somewhat stingy is in the non-salary increases to the budget. Bill Dickson and I used the same increase level as Mark Wrighton and I think John Deutch had used: the academic side had an annual increase of $1.5 million other than for salary inflation, and the administrative side had about $1 million of annual increases. These figures may appear low, but they do mount up after a decade, partly because they usually add to the salary base, which is then inflated each year. We asked the various deans and vice presidents that proposals be made by November of the preceding year. We then discussed the proposals with the president, with Doreen Morris taking notes. By the end of January we had usually made the tough decisions that permitted us to stay within the budget guidelines for the next fiscal year, beginning in July.

The overall Institute budget had several other significant changing variables, notably the cost of employee benefits and the allowable amount of overhead on research contracts. Each could vary by several millions a year, partly due to changes in research volume, and partly due to changes in government policies on reimbursement. Bill Dickson, Treasurer Glen Strehle, and I would meet with budget office staff monthly to review how the variables on income and expenses changed from the prior month and from the budget. In August we would learn the final fiscal year-end totals, which would vary from our estimates by less than one percent, but still a few million more or less than we had expected just a couple of months earlier. The Institute is apparently too complex for a much better estimate of the final totals. The “deficit” would then be made up from various funds. While I was dean the “deficit” was usually around $10 million or less, and thus not overly difficult to make up. Mark Wrighton had projected that it would rise to $20 million a year, and this led to the six-percent budget cuts earlier in the decade. As I became provost, I was concerned that the $10 million figure would rise markedly in the coming years, largely due to the RA/TA issue.

Visiting China as Provost

While he was provost Mark Wrighton had promised to visit China, and it became one of my early acts as provost to keep that promise. Mark had been scheduled to lead a delegation of our faculty to Beijing a few days after he left for Washington University. I had previously declined to go to China, largely because of my food restrictions, but I was told that the Chinese hosts would take care of this, just as my Japanese hosts had done on a couple of occasions. A key goal on this trip was to attend a conference on environmental issues that we were running jointly with our Chinese colleagues. In addition we were beginning a collaboration on a number of issues with Tsinghua University, which prides itself on being the MIT of China.

We began with a visit to the U.S. embassy, where we listened to various talks by academics as well as embassy personnel. The key message, as I recall, was that U.S./China relations have always been complex, and will likely continue to be complex in the future. We had problems with their human rights policy at the time, and there was in fact no replacement at the time for the previous US ambassador to China who had resigned.

The Chinese government made a big deal of our presence. The daughter of Deng Xiao Ping met us, and she and I spoke at a plenary. I asked her off-line the $64-dollar-question of the day, which was about the health of her father, and she diplomatically answered that it was good. I was told that we appeared on that night's daily news on Chinese television. I have never been able to obtain a tape of this event. I even asked Madame Deng for a copy a couple of years later during her visit to MIT, and was promised one. I figured that my mother would be impressed if she realized that hundreds of millions of people saw that TV program.

Most impressive was a meeting with the head of the Ministry of Trade. This took place in a palace that I was told was Mao Tse-tung's last home. Our

U.S. delegation had over a dozen representatives of industrial firms, such as Ford, GM, and ABB. While the minister and I sat at the center of the room, with translators behind us, the industrialists sat in chairs arrayed in a horseshoe shape around us. The minister listened to me very politely, but his real interest was in the industrialists. He asked each one in turn to indicate their interest in being in China. I think he knew the answers ahead of time. As best as I could determine the actual message that he was delivering with his comments, he seemed to be saying that if the companies wanted to do business with China, the companies should make it clear to their governments to stop pressuring China on human rights issues. China is a huge country, he pointed out, and the government knows how to keep that population under control. If they were not able to pursue their policies at home, then there might not be much of an economy for the companies to trade with. The industry representatives listened to this exchange very intently. At the time, there was a serious competition between Ford and GM for the right to open an automobile plant in China, and every signal was viewed as interesting by at least these two representatives. GM eventually won this competition and opened a billion-dollar plant in the south of China.

We were given a tour of Tsinghua University, where they made a special effort to show us the exhibit in honor of Norbert Wiener's visit there before the Second World War. I also saw a computer networking laboratory. Most of its equipment had been donated by U.S. firms. Some of our delegation members were said to be trying to figure out where and how the Chinese government monitored the international traffic over the Internet.

The president of the university proudly told us that he created a copy of MIT's Industrial Liaison Program in the six months since he learned of it during his visit to MIT in January 1995. I was impressed with his efforts to modernize his university. Their engineering programs were clearly influenced by Russian models, and it took students nearly six years to graduate, given the large number of requirements they had to fulfill. They wanted to reduce the length of the program by one to two years, as I recall. I lauded the effort, and explained our MEng program in some detail.

We had a number of tours of the city. I loved a park at which the dowager queen had built ships. People were angry with her for spending so much money on her palaces instead of building ships, so in response she had ships built made of stone and placed in that park. We were also taken to a restaurant that served food that looked and even tasted like meat, but this food was 100% vegetarian, largely tofu I believe.

The long-term effect of the visit was good. We were able to send students to work in Chinese institutions under our MISTI program, which had been so successful in Japan, and to obtain research support to work with our Chinese colleagues on various environmental issues, such as the impact of coal burning in China.

Lincoln Laboratory

Chuck Vest and I reached an agreement that the MIT Lincoln Laboratory would report to me. This made sense, since the Lab grew out of post-war research on radar-based systems that could detect enemy planes and was later broadened into R&D on missile detection. When MIT decided to divest itself of Draper Laboratory in the early 1970s, it decided to could keep Lincoln as long as Lincoln did not build any weapons. A key step in this process is the monitoring by the top MIT administration and their reports to the Executive Committee. In order to monitor Lincoln, I would need a high security clearance. This might take some time, given my dual citizenship. I was advised to name John Deutch, then the CIA Director, as one of my references on the application for security clearance, and sure enough the clearance came through relatively quickly.

In 1995 Walter Morrow had been director of the Lab for eighteen years. I had known him best for our joint activities on the Lincoln Laboratory Committee that John Deutch had formed. Walter had an amazing, encyclopedic knowledge of the R&D that was carried out at the Lab, even more amazing when one recognizes that Lincoln's research volume was around $400 million a year, higher than the total campus volume. Of course, part of that volume paid for tuition of research assistants through the Employee Benefit mechanism that Deutch had created. Walter decided that I ought to first meet with the Lincoln Laboratory Steering Committee, largely composed of division heads and assistant directors of the Lab. I knew that some of them were not happy with the Lincoln Laboratory report of the committee I had chaired about half a dozen years earlier; they felt that the report did not give the Lab enough credit for their nationally important work. I reminded the Steering Committee that I had worked in the Lab in 1963, and as an EECS faculty member I was more aware of their work than most other MIT faculty members. The report, I said, had to be balanced in order to deal with MIT faculty who wanted MIT to divest itself of the Lab in the late 1980s. My former boss in 1963, Jack Nolan, had returned to the Lab part-time and was editing their journal. Jack had left the Lab in the late 1960s and became President of the Massachusetts College of Art, a post he held for many years. This was the most dramatic career transformation I had ever witnessed.

Walter Morrow and I would meet monthly, usually at Lincoln, to go over the Lab's budget and its issues, which often involved relations with the various congressional committees that oversaw the DoD budget. The Lab had an assistant director who specialized in relations with Washington, but Vest had to intercede on several occasions on behalf of Lincoln. Chuck tried to spend an average of a day a month in Washington D.C., and thereby developed strong connections there, and arguably became the most influential university president of his time. Thus he was usually able to deal with the problems. At most of these monthly meetings at Lincoln, Walter would escort me to a lab somewhere in the complex to attend a description of one of their projects. These descriptions were a lot of fun, although I did not fully understand the technology involved in a number of cases. Later on when I had my clearance I was given some confidential briefings. I never felt that the "confidential" information was particularly surprising, and I'm glad that I no longer need to be burdened with it.

A couple of years later it was announced that Walter would step down as director of Lincoln. Chuck and I discussed the process for choosing his replacement, and decided to follow the approach taken by Paul Gray nearly twenty years earlier. I announced that I would spend a few days at the Lab, and staff at all levels there could schedule an appointment to see me. In addition, I met with all the Lab assistant directors and division heads. Previously Vest had created the MIT Washington Office, to help coordinate MIT's interactions with the legislative and executive branches of the government. Jack Crowley, its director, took me to meet with some staffers on the Hill to discuss issues that would be faced by the next Lincoln director. I also met with Vince Vitto, President of Draper Lab and a former assistant director of Lincoln, in order to get his perspective on the Lab and potential inside and outside candidates. After meeting with about sixty people I recommended to Vest that David Briggs, the leading internal candidate, be appointed as Walt Morrow's replacement. At a party for Walter a few months later we announced that he would become Director Emeritus. Walter had obtained a courtesy faculty appointment in EECS, although he rarely had time to spend on campus. I felt that being director of the Lab was a sufficiently powerful position and Dave did not need an additional courtesy faculty appointment, in Aeronautics and Astronautics in his case. The Lab has done very well, especially since September 11, 2001 as the nation has geared itself to fight terrorism.

Family Life

Even with the pressure of the office of provost, I was actively involved in my own family life, and made sure we had at least one family vacation each year. Peggy had had a couple of cases of sunstroke, beginning with a trip to Bermuda in the 1970s, so we had taken the boys north on our vacations, often to places in Vermont, New Hampshire, and Maine. I do not recall what gave us the confidence to try sunnier climes again, but we spent several vacations in Aruba, both alone and with our growing sons, now well into their teens. Aruba is a few miles north of Venezuela, part of the Dutch West Indies. It is south of the hurricane belt, and the weather is pleasant year-around. The trade winds that come in from the northeast make the beaches in the north part of the island very difficult for swimming, but nearly cancel the waves on the south side of the island. The hotels are, of course, on the south side. The atmosphere is quite relaxed, and the hotel staff is friendly, so we came back several times. One year the hotel ran a trivia contest. The questions must have been on strange and trivial topics indeed, because the winning score was mine: just thirty out of fifty questions. Peggy was asleep in our room while I collected the prize—a free three-night stay for two. With this head start on the next trip's expenses, we were able to bring both of our sons as well.

In 1995, David and Jesse started taking the SATs and writing their college applications. We had moved to Weston in 1993, since Peggy thought that Lexington High School might have presented similar difficulties for our identical twin sons as had the Lexington preschools, where their close resemblance became such a distraction. This was solved by sending them to different elementary and middle schools, but there was only one high school in Lexington, and we didn't want to chance putting them into a similar situation. Peggy visited several high schools in the area, and liked the principal in Weston best. He was quite open and outspoken; a year or so after we moved to Weston, when he said something like, "Weston High School has improved as a result of having just graduated the senior class," the local school board fired him.

I liked the house we bought in Weston because it permitted us to give parties for the Moses Seminar members, and for members of Academic Council. We used a kosher caterer for these affairs, and it was fun to watch the deans play pool in our family room. I tried to get a volleyball game going at some of these occasions, but never managed to field enough players.

David learned that he had difficulty reading, since his eye muscles were not well coordinated. The eye specialist gave him exercises, but he also wrote a letter saying that David ought to get extra time to take the SATs. He eventually received a score in the 1300s. Jesse, who did not have the eye difficulty and did not get extra time, scored nearly 1500. I cannot account for the difference. David was advised to apply early to Brandeis, meaning that he would have to go there if he were admitted, which he was. Jesse applied to nine schools, and needed to make a choice should he be admitted by more than one. I drove him to visits at Cornell and Columbia. It rained that day at Cornell, which unfortunately turned Jesse off to that school. I liked going back to Columbia, and showed Jesse the owl inside the folds of Alma Mater's coat. Jesse went on his own to the University of Pennsylvania, where a faculty member offended him by giving him a message to relay to me. He also visited MIT on his own. In the end, and much to his father's pleasure, he was admitted to all nine schools; he chose Columbia's Engineering School. The reasons for not choosing the others included the rain at Cornell, the message at Penn, and the fact that I was the provost at MIT. Both of our sons had memorized the *US News and World Report* rankings of American colleges. I did not like that very much because colleges are very different from each other, and a single ranking does not do justice to them, even if one could create a rational approach to ranking. On the other hand, the *US News* ranking is a part the real world of college admissions these days; I believe it changed the attitudes of many high-school applicants regarding MIT. Instead of MIT being considered the top technologically oriented school in the United States, it became for many one of the top five universities in the nation. The difference, I believe, is profound, and has had an effect on faculty and administrators as well, in that while the School of Engineering is still preeminent in its field, it is in the opinion of some of the other deans not as central to MIT's mission as it was a couple of decades ago.

The Moses Seminar

The Moses Seminar, so named by Jay Keyser, has been running since 1985. Initially we met in the D.C. Jackson Room near the EECS headquarters. Later we moved to the Engineering Dean's Conference Room, where we still meet. There are a dozen regulars, and a similar number of faculty members who attend less often. A couple of our early members have died, Thomas Kuhn and GianCarlo Rota. Tom was arguably the greatest philosopher of science of our time, author of *The Structure of Scientific Revolutions*. GianCarlo was a great mathematician, especially of the foundations of the field of combinatorics, but he was also interested in philosophy, especially phenomenology. GianCarlo died in the evening after giving a talk at the seminar. Our oldest member is Jerry Lettvin, who probably should have gotten a Nobel Prize in physiology for his 1957 paper, “What the Frog’s Eye Tells the Frog’s Brain.” For a while Jerry and I were the largest faculty members at MIT. His wife, Maggie, was slender and lithe. She used to have an exercise show on PBS, and published a well-received illustrated volume, *Maggie’s Back Book*. She must have tried, but she did not succeed in getting Jerry to exercise, although he has lost significant weight in recent years. Jerry has great genes, however, since his parents lived into their nineties. Jerry is a psychiatrist who has specialized in neurophysiology. He has an amazing ability to argue any position of an issue equally well. Several of our seminar members are former AI students of Minsky, including Gerry Sussman, Pat Winston, Tom Knight and me. We are hoping for a resurgence of research in AI that will have a connection to brain and cognitive science. This approach is in contrast to Modern AI, which is closely connected to computer science but less so to cognitive science or AI’s original dreams. Jay Keyser, a former associate provost, maintains our connection to linguistics, and Arthur Steinberg keeps our association with archeology, anthropology, and religion. Jay, Arthur, and I meet regularly to plan the speakers for the seminar. These meetings, usually at breakfast at a nearby deli, are some of the most pleasant times I have had in recent years.

The Jigsaw Puzzle of Space Allocations

As provost I ran the Institute's committee on changes in space allocation and remodeling, the Committee for Review of Space Planning (CRSP). It was made up of the Senior Vice President, Bill Dickson; the Institute's Planner, Bob Simha; the Director of Facilities, Vicki Sirianni; the Space Administrator, John Dunbar; and the Assistant Provost, Doreen Morris. With Bill in charge of all non-academic functions at MIT, Vest, Dickson, and I were a triumvirate that was the upper administration in the Institute. Bill reminded me in some ways of Louis Smullin in that he sometimes came across as not knowing what was going on, but like Louis he understood what was going on better than all the rest of us, and the questions he asked got to the heart of the matter quickly. Bill was also a great problem solver, and the senior administrators that reported to him relied on this ability. Doreen Morris and her fellow assistant deans, but especially Doreen, managed the details of the academic side of the Institute. No provost or dean could function without them. They remembered all the policies and special deals regarding budgets and faculty chairs; they were the conveyers of messages from the provost and deans; and they usually prevented the provost and deans from making political or financial mistakes.

CRSP at the time had a very low budget for space changes, about $3 million per year, and only an additional $1.5 million for replacing building systems, such as roofs and elevators. If MIT's overall buildings had a replacement value of $2.5 billion, as we believed, then a good heuristic estimate for space changes and modernization would be 2% or about $50 million a year. MIT was lucky in that many of its buildings were less than 30 years old and thus their need for major system modernization of was not that great, but this was a set of potential problems that would come up as soon as the buildings aged. Yale University had underinvested in modernizing its buildings, and this had become a crisis for them a decade earlier. MIT had somehow to make sure that it did not repeat that mistake.

Building 20

A major concern of CRSP while I was provost was the need to tear down the venerable Building 20, which had housed the Radiation Laboratory in the Second World War and where radar was developed. Thrown up under great wartime pressure, it was the only significant frame structure left on campus, and strongly resembled a barracks. MIT bought it from the federal government for $1 just after the war, and it became home to a variety of programs. The 200,000 net square feet of space in Building 20 housed the Linguistics and Philosophy Department, parts of the Laboratory for Nuclear Science, biotechnology laboratories, and numerous other programs and offices. Building 20 was a provost's best friend over the years, since space for new programs could usually be found in it. Most of the people who resided in the building loved it: it was flexible, comfortable, and of human scale. Yet other people had been saying that it needed to come down for as long as I can recall. John Deutch made a decision to tear it down in the late 1980s, partly because of concerns over the asbestos used in the building's construction. Ironically, it was this asbestos that delayed its demise, since environmental laws decreed an extremely expensive method of tearing it down. The space was to be used for a new Computer Science building, which surely made me happy. Deutch felt that he could move the bulk of Building 20's occupants into spaces vacated by the biologists, who had just obtained a major new building on the campus. The renovation of the biologists' former buildings took place during Mark Wrighton's term as provost, but not surprisingly there were many programs and people in Building 20 who could not fit into that space. This then became my problem.

One issue was where to place the Department of Linguistics and Philosophy. The department thrived in Building 20, where its closeness to electrical engineers led to significant developments in phonology as well as syntax. My decision was to place them next to the AI folks in the new computer science building that we were hoping to be able to fund. What to do with them in the meantime was also easy: we simply had to rent space for them in Kendall Square near the campus.

The Laboratory for Nuclear Science had a major machine shop in Building 20. A study under the auspices of the Dean of Science, Bob Birgeneau, indicated that we could combine this shop with the existing machine shop in the Research Laboratory of Electronics. Some of the work would be farmed out to more modern shops in the Boston area. Where would we get the additional space for the combined shops? We would need to move the student shop in the basement of the RLE building. But we felt that the student shop was very valuable. We would place it across the street in a building that was being used by Nobel Laureate Sam Ting. Ting apparently could be convinced to give up some of his space, since most of the work had been completed on his experimental equipment and it had been shipped to Switzerland to form one of the high-energy physics projects at CERN.

All told, we had to find space for about a dozen different groups. Each case was different, and each involved a complex negotiation by the members of CRSP. I knew that the current and former inhabitants of the building were very nostalgic about it, so I asked Paul Penfield to arrange for a symposium in honor of the building. I gave the opening talk. I described the various groups of people that had to be moved out of the building as the Lost Tribes of Building 20. Penfield described the building as the Magic Incubator, and it truly was that. This may have been the only occasion when a building at MIT received a symposium in its honor just as it is about to be demolished. Penfield's symposium later won an award from the MIT Alumni Association.

Needed: A Campus Makeover

The Institute's last period of major construction was in the 1960s, when the Federal government was willing to underwrite some of the costs. Of course, there had been some construction since then, such as the Media Laboratory building and the Biology building in the 1980s, but there was quite a bit of pent-up demand for more space by 1995. In addition to the Computer Science building that had been actively discussed, at least by me, since 1978, Nicholas

Negroponte wanted an extension of the Media Lab, which had clearly outgrown its quarters. The Sloan School felt that it needed a new facility, since its existing facilities were shabby in comparison to the other major business schools, and their faculty were scattered over several buildings, which hampered communication in the school. “Shabby” was the word that could be used to describe some of the rest of the campus, and the time had come to give the campus a make-over. Increasingly it became clear that we needed hundreds of millions of dollars to replace major systems, such as roofs and elevators, in existing laboratory buildings. Such systems last about thirty years, and as noted above, many of the newer buildings were becoming thirty years old. The original 1916-era buildings needed new windows, since it was becoming nearly impossible to open the existing ones, which in any case were far from energy-efficient. One estimate for the replacement of the windows alone was over $25 million. In addition, the students needed various new facilities beginning with a new athletics center with a modern pool, a new dorm for graduate students, and a new dorm for undergraduate students. There was also a request for a new science teaching center with a number of large lecture rooms. My estimate for the total cost of the new facilities plus the major renovations was more than $800 million over the next ten years. We did not anticipate receiving all that money from gifts, especially the renovations money, and thus our borrowings would have to grow, and so would our annual interest and principal payments.

The Ray and Maria Stata Center

Ray Stata, who was chair of the EECS Visiting Committee, had heard from me of the need for a new CS building for over a decade. His son Raymie was a doctoral student in CS, and he may have heard more from him. One day Ray told Chuck Vest that he and his wife, Maria, would donate $10 million toward the new building on the site of Building 20. We were ecstatic, since this was one of the largest gifts ever given to MIT. There was one problem, however. I heard through the grapevine and in the CS faculty lunches that the CS faculty members were not eager to move. A key reason, I believe, was that by moving onto the campus they would lose some of the independence they had in Technology Square. This was clearly one reason I wanted them to be on-campus. I felt that the CS culture was becoming increasingly independent of the campus, and I wanted greater collaboration with the rest of the department and the Institute. As I was getting increasingly frustrated with this view of the CS faculty, Vest decided to give the job of negotiation to Bob Brown, whom we appointed Dean of Engineering late in 1995. Bob came to me early in 1996 and said that the EECS department wanted to move another laboratory to the building in addition to those units already scheduled for it: the Laboratory for Computer Science, the Artificial Intelligence Laboratory, and the Department of Linguistics and Philosophy. I agreed to include the Laboratory for Information and Decision Sciences in the mix, recognizing that the size and cost of the building would have to increase as a result. Including LIDS in the mix of occupants made the EECS administration happy, but did not appear to make the CS faculty happy enough to want to move. Dean Brown needed to negotiate further with them. I came up with an idea that led to a significant increase in the gifts toward construction of the complex. This was to consider the building as being composed of two connected buildings, one housing LCS and one housing all the other labs and departments. Thus we could name the overall complex for Ray and Maria Stata, and each building for a separate donor. Ray Stata liked the concept, and eventually raised his and Maria's gift to $25 million. At the time, this was the largest gift in MIT's history. Eventually, the building containing LCS was named after Bill Gates, who wrote the figure of $20 million on a napkin, which he handed to Mike Dertouzos. The second building was named after Alex Dreyfoos, whose gift is valued at $15 million. Since the CS faculty, once they agreed to move, would relinquish rented space that cost the Institute several million dollars per year, the actual value of the major gifts plus the rental value was over a hundred million dollars. Bob Brown and I raised additional gifts for naming a lobby, a floor, and a lecture hall among other named places, but it was still not enough, since the cost of the overall project was growing far above that figure.

One issue that was discovered late in the process was that the average office in Technology Square was smaller than the average on-campus office, but correcting that oversight would significantly increase the square footage in the complex. Similarly, demand for undergraduate education in computer science had grown by leaps and bounds since the early nineties, largely due to the popularity of the Internet, and we had to allow for even greater expansion of space for additional faculty and staff than the 10 percent I had previously assumed. The growth in the number of majors in the boom years in the late 1990's helped convince the CS faculty that the single building in Tech Square simply could not accommodate all the faculty, students, and staff in CS, and that the expansion potential of the Stata Center was needed. Later on the Stata Center grew even further to meet a number of other institutional needs and desires - an athletic facility, a cafeteria, the faculty lunchroom, and a child care facility as well as two underground garage floors. The overall complex is now the largest construction project at MIT since the 1916-era buildings, with an estimated total of over 700,000 square feet. Chuck Vest chose the world-famous architect Frank Gehry as the architect of the Stata Center, and the buildings were completed in 2004, about 40 years after we moved into Technology Square and 25 years since I first pointed out the need for such a center. Luckily, by waiting as long as we did the growth in CS in that time period could be accommodated in the Stata Center. Gehry was not everyone's first choice, and his interior design is surely not as flexible as I would have wanted, but his design has made MIT a favorite tourist stop in the Boston area for many years.

My concern about the inflexibility of the Stata Center comes from my experience with space changes as provost as well as my interest in complex systems. A building such as the Stata Center needs to be usable for a century. During this period the uses and occupants of the building will change a great deal. Groups will increase in size, decrease in size and be completely changed in that time frame. Office groupings that are linear or nearly linear in their topology will permit many such changes in occupancy patterns, as we saw in MIT's main buildings. Gehry apparently does not like linear groupings of offices. Mike Dertouzos also emphasized the importance of research groups. Thus was born the concept of ‘neighborhoods’ in Stata. My feeling was that such neighborhoods might work for the initial occupants, but will make it difficult to satisfy research groups that grow in size, for example. Some of their new members may have to be placed far out of sight of the original members. Dertouzos also wanted the two towers in Stata be as far apart as possible, since he was director of LCS and wanted the AI lab far away. Later the labs merged but their physical location remained unchanged.

Other issues also arose regarding the Stata Center. The great increase in the project’s cost made it more difficult for other building projects that I proposed as part of the campaign that began in 1996-7. In particular, the Sloan School extension and the Media Lab extensions were delayed by nearly a decade as pressure was placed to obtain a larger fraction of these buildings’ cost from gifts.

Creating Councils

The Council on the Environment

Mark Wrighton had created a Council on the Environment in order to coordinate the various activities related to the environment that grew during his tenure as Provost. I continued to chair this council, but Dave Marks, as co-chair, was the real force behind it. My advantage was that I was on Academic Council and could describe the major activities to the other deans and vice presidents. I felt that the environmental council worked reasonably well in a very complex situation involving all the schools at MIT, and suggested to Vest that we create three other councils to deal with important interdisciplinary issues. Vest liked the idea and we created three councils, each chaired or co-chaired by a member of Academic Council: the Council on International Relationships, chaired by Associate Provost Philip Clay; the Council on Educational Technology, co-chaired by Dean William

Mitchell and Professor Michael Dertouzos; and the Council on Industrial Relationships, co-chaired by Dean Glen Urban and Professor Daniel Roos.

The Council on International Relationships

The Council on International Relationships was needed to vet the increasing number of the Institute's international activities. Various governments around the world wanted to increase the technological sophistication and entrepreneurial activities of their populace, and came to MIT to see if we would be willing to help. Often they asked us to work with a local university in a number of high-technology educational programs. With Professor Fred Moavenzadeh, Director of the Center for Technology, Policy and Industrial Development, I negotiated arrangements by which MIT would help universities in Thailand and Malaysia. These arrangements were not successful initially because of the financial crisis that hit Southeast Asia in 1997. Bob Brown, however, arranged a program with the government of Singapore that was very successful from the start. In it our faculty teach live courses with a twelve-hour time delay to Singaporean university students, who are thus attending night classes while the MIT attendees attend morning classes; our faculty in engineering and management also obtain support for joint research with Singaporean students and faculty. All told the initial Singapore program was on the order of $25 million per year, a very significant amount. Since the Council on International Relationships was composed of faculty from all schools at MIT and with many different points of view, it could give the administration advice on whether it was wise to go forward with a proposed activity. The government of Great Britain began to negotiate with MIT in mid-1998, just as I was leaving the Provost's office, a significant new activity centered on collaborations with Cambridge University. A subcommittee of the Council was formed to discuss this particular proposal, which the British government wanted kept confidential until it was ready to be announced, in the fall of 1999. A key feature of this program is an exchange of students between

MIT and Cambridge University. The Cambridge students were expected to gain an insight into MIT's entrepreneurial culture that they can bring back to Britain. This was a key goal of the program to Lord Alex Trotman, former chairman of Ford and a consultant to Britain's Prime Minister Tony Blair. Trotman played a key role in establishing the Ford/MIT partnership noted below, and this experience presumably led to his recommendation of MIT to the British Prime Minister. It appears, however, that MIT students were having some difficulty with the much less structured environment at Cambridge University. We then gave them additional advice and support regarding how they can handle these two great institutions' rather different approaches to education.

The Council on Educational Technology

A second new Council was the Council on Educational Technology, whose members included true believers in the value of educational technology, especially distance education, such as Dick Larson, Director of the Center for Advanced Educational Services, as well as people who were somewhat skeptical of the role of educational technology, such as Rosalind Williams, Dean of Undergraduate Education and Student Affairs. One problem we ran into immediately was that Mike Dertouzos, the co-chair, was spending much of his time finishing his book, *What Will Be?* He warned me of this up front, but I felt that we needed Mike on the Council since he had been so involved in prior studies of technology and education at MIT. The Council eventually produced a report that emphasized the need for experiments with technology to find out what works for MIT students. In the meantime we were already committed to using distance learning with the SDM program and with Singapore. I spent a couple of million dollars of the Provost's limited funds to create three classrooms for remote education in the CAES building. Since the report was published there has been significant fundraising for additional experiments, largely for the use of computing and communications technology for our on-campus students. Alex D'Arbeloff, the Chairman of the MIT Corporation in the late 90's gave $10 million for educational experiments; Microsoft donated another $25 million for experiments similar to those for Athena 10–15 years earlier. We have learned a great deal since 1982 about what works for computer/communication-based learning, and we will learn even more with these experiments.

MIT's approach to distance education emphasizes significant arrangements with companies and governments. Our students are largely taking Master's-level programs; in contrast, many if not most distance-learning programs in the United States at this time involve students taking one subject at a time. In such situations the price per subject becomes a critical issue, and many of these programs do not return a profit to their institution that covers much of the overhead costs. This area is of course quite fluid. The Singapore program has been working quite well, and has been renewed. The distance learning component of SDM has been having financial difficulties, I believe largely because companies have not been as willing to invest in their people in the post-bubble years as they had in the mid 90's. This financial problem was resolved in the following decade.

The Council on Industrial Relationships

The third new council, the Council on Industrial Relationships, studied the viability of the concept of industry/MIT partnerships. By 1996 we had two such partnerships, one with Amgen and one with Merck. A partnership differs from a grant or contract in that it is longer-term, usually five years. Partnerships are funded at the level of $3 million per year or more, and have built-in flexibility regarding the research program. Partnerships are usually arrived at through an early involvement of MIT's president and the CEO of the company. The Amgen partnership and the Merck one following it were relatively easy to negotiate, since biotechnology and pharmaceutical companies rely heavily on basic research and university research is often appropriate to their needs. MIT negotiated these partnerships with no change in our normal contracts regarding intellectual property and publication rights.

I wanted to involve some major companies that were not so reliant on basic research, and we asked the Council to advise us regarding the feasibility of this concept. A major reason for creating new partnerships was that in 1995 there was serious concern over the level of federal support for research; in fact, I believe that MIT's level of federal support had declined by about 25 percent in real terms since 1985. Thus I felt that there should be significant faculty interest in additional support. Of course, if the faculty had evinced little interest in proposed projects, we simply would not have gone forward with them. The Council wanted to interview CEOs regarding their views of such partnerships, their views of universities in general and MIT in particular, and their views of the needs for research in their firms. We were pleasantly surprised when about twenty CEOs of major U.S. firms agreed to be interviewed in person; disappointingly, the interviewees did not think that the partnership concept would be a roaring success. By the time we received the report of the Council, however, we had formed several new partnerships, so that by the end of the decade about 10 percent of all MIT's on-campus research was funded by such partnerships. In fact, much of the growth in research support on the campus from 1995 to 2000 came as a result of such partnerships. The Council also suggested setting up a new Office on Industrial Relationships, possibly headed by a faculty member with the title of vice president. The success that we had in the next few years made this suggestion moot, at least in my opinion. On the other hand, when the position of Chancellor was created in 1998, a variation of what Dean Urban and I had suggested to Vest three years earlier, the Chancellor could become closely involved in a number of these partnerships.

Our first effort at a new partnership was with Disney. Our main contact was an alumnus, Brad Ferren, who played a major role in Disney's research activities. Vest and Ferren had a preliminary meeting with Disney's Chief Operating Officer in Los Angeles. We then spent many months negotiating with Disney about possible joint activities, both in developing new games and in distance learning. It became clear that Disney is very concerned about its intellectual property, largely due to Walt's experience in the 1920s when he lost control of one of his earliest cartoon characters. Disney's lack of experience in dealing with universities finally led us to discontinue our negotiations. A number of us did have fun visiting "backstage" at Disneyland, and seeing some of their newer products and rides.

Our next effort was with Ford Motor Company. Vest and Ford's CEO at the time, Alex Trotman, were both members of IBM's Board of Directors. They decided to see if MIT and Ford could be of greater help to each other. Trotman visited MIT and gave a talk at our lecture series for CEOs. He assigned John McTague, a former science advisor to the President of the United States and a senior vice president of Ford, the task of seeing if there could be a partnership between MIT and Ford. McTague asked John Powers, VP for Research at Ford, to do a feasibility study. I was Powers's counterpart at MIT. We held a meeting at the MIT Faculty Club and discussed issues of common interest. We knew that Trotman was especially interested in environmental issues and felt that MIT could help determine which of these issues were the most important to bring to the attention of the world's governments. Another topic was the cooperation between MIT, Ford, and Daimler Benz on a new automotive electrical power standard of 42 volts—needed by future cars, which will rely much more on power electronics. We agreed to continue and enhance such a consortium activity. Finally, there was joint interest in R&D related to automotive design. MIT was embarking on a new Center on Intelligent Product Design, and we were also interested in having Ford send students to the new Systems Design and Management program. In all, there appeared to be enough joint activity to justify a partnership.

Over the coming months, we developed a proposal to Ford for activities to be undertaken in a partnership. We asked for $8 million per year for five years, and Ford was surprised, partly since the previous two partnerships with Amgen and Merck were for about $3 million per year. McTague also sent us a clarification of what they were interested in, in the environmental area, listing a number of issues and suggesting that MIT and other institutions should be able to arrive at a ranked order of such environmental issues as global warming, to which world bodies such as the UN and companies such as Ford ought to pay attention. This list caused our economists great difficulty, and it appeared they were going to avoid working on this critical element of the partnership. At one point I asked Larry Bacow, who has an economics degree, what was the problem with the list. He pointed out that Ken Arrow, a Nobel Prize-winning economist, has a famous theorem that says that any non-trivial rank ordering of people's interests, such as that wanted by Ford, would be logically inconsistent unless the group had a 'dictator' who forced a particular ranking. This was very helpful to me. At a subsequent meeting of the environmental group I noted that it ought to be possible to do important research by modifying McTague's request rather than violating the Arrow impossibility theorem. Bacow said later that I had held the group's feet to the fire, and eventually they agreed to accept $1 million a year in research related to the environment. I do not think that I forced anyone to do research in areas that they did not wish to pursue. On the other hand, I can be persistent on occasion.

John McTague sent a letter to Vest outlining what Ford was willing to sponsor. The level was $3 million per year in addition to what Ford was already sponsoring at MIT, which was well over $1 million a year at the time. The letter left about $600K under the control of the Provost, to be negotiated with Ford on a regular basis. We had not specifically asked for this element, but it was Ford's great insight to create such flexibility in the eventual contract. In fact, Ford's key people, Powers and McTague, understood our position very well, and created a program that made it quite easy for MIT to negotiate a contract with them, without any changes in our normal intellectual property clauses.

The Ford partnership seems to have broken the logjam of industrial partnerships. There were about ten of these, and as noted earlier, the total research volume has accounted for most of the growth in research on campus during the period 1995–2000. There was some concern expressed by faculty members about the change in the process of obtaining research support, with the administration taking a more active role and with support from industry growing. I don't believe MIT changed its usual contracting agreements to create these partnerships, which accounted for only about 10 percent of our overall on-campus research volume.

The Education Committee

The Academic Council is a relatively formal committee, with about two dozen members, so Wrighton had started a committee, called the Education Committee, made up of the academic members of the Council and operating in a less formal manner. Doreen Morris, the Assistant Provost, was secretary to the Education Committee, and I continued the practice of having the Provost chair it. We dealt with issues such as the academic and overall MIT budget and the progress of the various other Councils. I started most of the meetings of the Committee with a joke, so as to get everybody to relax. Here are a couple of them:

A grandmother and grandson go to the beach. The boy goes swimming and the grandmother notices that he is drowning. She screams for help, and a lifeguard swims out, gets the boy, and brings him back to shore. The boy miraculously is fine. The grandmother now says to the lifeguard, "But he had a hat."

Two men go on a cruise, but their boat sinks. They swim to the nearest island. They find that the island has no one living on it. One of the men tries to attract attention from other ships by making fires and swinging his white shirt from the top of a tree, but it is to no avail. The second man lies on the beach and suns himself. The first man gets angry with the second and says, "Don't you realize that we will die soon if no one rescues us?" The second man responds, "Don't worry. Just before we left on the trip I pledged a million dollars to the United Jewish Appeal. They'll find us."

The Retirement Plan

Cost reductions were on the mind of Bill Dickson for a number of years. In the mid 90's he created an early retirement plan for the MIT staff. I decided to use this opportunity to create a retirement plan for the faculty as well. For many years US faculty members were required to retire at age 65, then this limit was increased to age 70, and finally the mandatory retirement age in private universities was eliminated altogether. My hope was that with proper incentives many of the faculty above age 65 would consider retirement.

I studied approaches that were taken by other universities. UC Berkeley had a different retirement system than MIT's which is largely based on a 401K account. The UC retirement incentive in the early 90's was so good that too many of the faculty there took the retirement option. On the other hand, certain private universities, such as Yale, had an incentive plan which attracted relatively few faculty members. I decided to offer a plan a bit better than Yale's. Our plan had several choices, but basically it offered faculty an increase in their retirement account of 2.5 times their academic year salary if they retired immediately, and 2 times their academic year salary if they chose to stay on less than half time for up to five years and then retire. I told the deans in the Education Committee to tell the department heads that they need to meet with each interested faculty member and work out a plan for their participation, if any, in the departmental education and research programs upon retirement. I was specifically concerned over space promised to each potential retiree, since new replacement hires would, of course, require space as well. I also told the deans that I hoped to replace nearly all the retired faculty with new junior faculty hires on a one-for-one basis. After setting the plan in motion, and giving the eligible faculty several months to consider it, we simply had to wait and see who accepted the offer.

At first, it seemed that relatively few faculty members would accept the offer, but it appears in retrospect that the faculty waited until the last few days to make their decisions. Eventually, 79 faculty members accepted the offer, approximately 8% of the entire MIT faculty. Although there was skepticism by some that we would use the plan to reduce the size of the faculty, this was not the case. A few of the retirees were athletic coaches who were considered faculty members, but under Dickson's plan for staff members were not necessarily replaced on a one-for-one basis. A few other faculty members had part-time appointments at the time of the retirements, and they were replaced on a basis which was negotiated with their deans in each case.

On the whole, the plan was very successful. Average research volume of the retired faculty members was about $40K in the year prior to their retirement, about one tenth, on the average, of the other faculty members' research volume. As their replacements took over, MIT's on-campus research volume began growing significantly, after some years of stagnation. The budget we needed to allocate to faculty salaries decreased at the same time, since starting faculty salaries are lower than those of senior faculty members. The difference would initially be needed to pay for the one-time costs of the program.

Bob Brown, the Dean of Engineering, used the opportunity of having three dozen faculty members in the school retiring all at once to change the process by which faculty slots are allocated to the eight departments in the school. Traditionally departments at MIT expected to create a slot following a retirement which they could use to hire a new faculty member, usually an assistant professor. Brown said that all retirements would revert to the school and that departments would bid for new slots at a meeting each fall. This greatly increased the flexibility of the Dean of Engineering in managing the school. I had hoped that Bob Birgeneau, then the Dean of Science, would implement a similar process, but Birgeneau did not do so. While a dean has to use this process carefully, it is an important change in MIT practices that I hope future deans will rely on. I believe that MIT deans had less power than they should have, and this placed more issues before the provost. Brown's change increased the power of the Dean of Engineering, which if used wisely will be good for MIT in the long run.

Birgeneau's key issue while I was provost was the role of women faculty in the School of Science. I supported Bob's efforts in this matter, since I had heard of anecdotes regarding the treatment of women faculty in the School of Science. Bob's leadership in this issue led to the issuance of a famous report in 1999 and to his later appointments as president of the University of Toronto and Chancellor of the University of California at Berkeley.

The Capital Campaign

This brings me to a discussion of the MIT budget and fundraising. As I mentioned earlier, I felt that with the RA/TA changes and the need for new construction there could be very large deficits for many years. Parents would likely resist tuition increases well beyond the rate of inflation, and faculty and staff would begin to leave if MIT did not continue to give them competitive raises. I told the Executive Committee that we needed a capital campaign to help with this issue; only later, when the budget forecasting model was in better shape, did I realize that in the short term a campaign that assumed sizable construction activity would not help the bottom line very much. Initially I thought the campaign needed to be for at least $1 billion, or twice the initial goal of the previous campaign, which ended in 1992. On the other hand, that campaign had raised $710 million; twice that figure would be close to $1.5 billion. That figure eventually became the goal of the campaign. The goal was raised later to $2 billion and the campaign ended successfully in 2004.

University campaigns begin with a two-year quiet period, when one tries to obtain a sizable set of pledges. Then the campaign is announced, and usually lasts for another five years. Vest asked me to work with the academic side to create a list of needs, and I used the Education Committee to help with the task. The needs of each member were to be presented to the group, and I would compile the overall needs, which I hoped would not exceed $1.5 billion. I spent

some time analyzing John Deutch's work on a similar list a decade earlier. I noted that he tended to underestimate the amount of money that donors, whether individuals or corporations, would give toward specific research projects. We tended to overestimate our ability to get donors to give toward our own goals and perceived needs, but donors want to give toward things that they felt would be of interest to them, especially if the donors were corporations. MIT has generally received a larger percentage of its gifts from corporations and foundations than most other peer institutions. I estimated that we would obtain $600 million for specific research programs during the campaign. This was later changed to $550 million. My guess was that even the higher figure would be greatly exceeded. This turned out to be true, but then the $1.5 billion overall campaign goal was itself exceeded in 2003 when overall goal was then raised to $2 billion.
The RA/TA analysis done under Wrighton showed that MIT had relatively high research-assistantship costs. Stanford had announced a special campaign to raise $200 million for graduate student fellowships and I felt that MIT ought to aim for a similar figure, while recognizing that alumni tend to give money toward undergraduate scholarships, rather than graduate fellowships. Of course, we put $100 million toward scholarships, and another $100 million for support of undergraduate programs and student life. Faculty chairs were budgeted for $150 million, and unrestricted funds were estimated at $100 million.

Although it was not the largest element of the campaign in dollar terms, the most work went into estimating the priorities and cost of the construction anticipated in the coming decade. I asked Dean Mitchell of the School of Architecture and Planning to chair a committee that gathered the information about building projects and made estimates of their costs. I made the estimates regarding the likelihood of obtaining private gifts for these projects. The best estimate for all the projects was $650–700 million, with $300 million in gifts during the campaign, and around $300 million in borrowings. It was felt in the Treasurer's Office that we could borrow no more than that figure and still maintain MIT's excellent AAA bond rating. Of course, such a high rating lowers borrowing costs for the Institute.

I made a presentation of the overall wish-list to the Education Committee and Academic Council. Since most people's major requests managed to get on the approved list, there was general acceptance of the priorities. A major source of unhappiness, as I knew it would be, was the fact that a new building devoted to science education with large lecture halls was not on the approved list. My feeling was that we had to limit construction, and that this building would in effect break the camel's back. I certainly hope that it will be built soon. A dorm for graduate students was approved, but the undergraduate dorm took on urgency with a change in student housing policy, described below, and has since been completed and named Simmons Hall. A generous donation from Albert and Barrie Zesiger made possible construction of a new athletic facility.

There were two remaining construction-related issues. One was that the campus needed a sprucing up. For example, the main corridor—the Infinite Corridor, in MIT parlance—was felt by many to be quite dingy. Chuck Vest asked Dean Mitchell to oversee this facelift. The other issue was more serious and far more expensive. As noted earlier, many of MIT's buildings had been built in the 1960s and their systems, such as air conditioning or elevators, were coming to the end of their useful lives. It was estimated that we needed to spend well upwards of $20 million per year in major maintenance activities. Hardly anyone would donate significant funds for replacing roofs or windows. While we might be able to obtain some help by placing these costs on the overhead, the overhead rate was already quite high, and we did not wish to increase it if we could help it. This issue had to be resolved somehow, but it could not be resolved via the campaign.

Student Life

Two issues dominated my term as provost. One was the budget issue. I knew about this issue up front, and wanted to make significant progress on it. The second was unexpected, and had to do with student life on campus. Just as

I became provost in 1995, Vest and I agreed to ask Rosalind Williams, of the Science, Technology and Society Program, to become Dean of Undergraduate Education. We knew that Roz had relatively little administrative experience, but she was nominated by the search committee, and her interest in undergraduate education, especially in teaching writing, was manifest. The office that Roz was to undertake was dominated by staff, rather than faculty, and some of the staff had a reputation for being difficult to handle. Nevertheless, we felt that with some help Roz could manage.

Some time later one of our vice presidents, Jim Culliton, died. Jim, who was a very good manager, had oversight over many of the administrative functions on campus, such as accounting, medical, athletics, and student housing. He had inherited student-oriented activities from another vice president, Constantine Simonides, who died unexpectedly a couple of years earlier. In reallocating Culliton's functions, I was cognizant of the feeling among some faculty members that the number of members of Academic Council had grown too much over the past two decades. The search committee that recommended Roz Williams as dean of undergraduate education also suggested that most student-related activities, such as dining, housing, admissions, athletics, and financial aid be consolidated in one office. Chuck Vest and I agreed to let Roz take over these functions as well. In hindsight, it is not clear that any faculty member had the administrative acumen to run such a complex operation, and our decision in retrospect appears unwise. I told Roz, who appeared eager to take on the additional duties, that Bill Dickson and I were ready and willing to help her manage the overall complex of activities. I had many meetings with Roz regarding her proposals to reorganize the joint office, but I did not get involved in the details. I had used this advisory approach to management in the dean's office, and it seemed to work well, as it did in the provost's office with this one notable exception. Roz apparently relied on the additional advice and support of Bob Birgeneau, the Dean of Science, and Larry Bacow, then the chair of the faculty. Both would later become university presidents, Bob at the University of Toronto and then at UC Berkeley, and Larry at Tufts University.

Roz put her heart and soul into running the newly named Office of Dean of Student Affairs and Undergraduate Education. In 1996, as I recall, she came to Chuck and me with a proposal that we create what came to be called the Task Force on Student Life and Learning. Her grandfather, Warren K. Lewis, had headed a commission on MIT's education policies in the 1940s that had a great influence on the Institute, partly by recommending the creation of the School of Humanities and Social Science. Soon it would be fifty years since the Lewis report, and we thought this anniversary would be an excellent time to reexamine current policies. A great deal of effort was put into the Task Force. The Task Force Report particularly emphasized the concept of MIT as a community comprising students, faculty, and staff, which arguably made easier Chuck Vest's 1998 decision to house all freshmen on campus, as well as the inclusion of seminar rooms in the new undergraduate dorm. The report has led to a great improvement in the quality of student life; the Task Force was Roz Williams's shining moment as dean, a post she would hold until 2000, when she returned to teaching.

The work of the Task Force became urgent and the Office of Dean of Student Affairs and Undergraduate Education's need to make changes became pressing following an event none of us could have predicted: the death of a freshman, Scott Krueger, in October of 1997. MIT freshmen were expected to choose their "living groups" from among fraternities and dormitories during "Rush Week," the first few days after their arrival, just before the start of classes—an unusual arrangement in American universities, but the faculty and students had been unwilling to change it in 1989, when Molly Potter's committee recommended such a change.

Krueger chose a fraternity and was undergoing one of its initiation rites—consumption of as much alcohol as possible in an hour or two, otherwise known as binge drinking. While MIT's orientation for new students emphasizes the laws related to drinking, and while the legal age for drinking in Massachusetts is 21, in practice the younger students sometimes flouted both policies and the law when they were unsupervised. The fraternity houses are self-governing, and even the dorms with a faculty presence saw alcohol abuse. Although studies by the Harvard School of Public Health indicate that binge drinking at MIT is lower than the average among universities, the notoriety of Krueger's tragic death had a profound effect, on the MIT campus and at colleges across the nation.

Krueger died during a weekend. Vest called an emergency meeting of a number of people for Monday morning to deal with the issue just before he was to leave to attend an IBM board meeting. When Vest returned he scheduled a press conference for Wednesday. I have never seen him as shaken as he was that day. The national press had made a great deal of the fact that the death occurred at MIT, one of the nation's top educational institutions. There was also a great deal of discussion of the fact that students can choose to enter a fraternity right away at MIT. On-campus binge drinking received so much comment that Vest named a committee to study it and develop recommendations for controlling it.

Roz Williams and her office spent a great deal of time on the aftermath of Krueger's death. We learned through the press that the parents wanted to sue MIT because it did not properly supervise the student living groups; some former MIT undergraduates claimed that they wrote a letter to Vest several years earlier saying that they thought deaths would occur in fraternities largely as a result of drinking, and that nothing was done in response; a faculty committee came forward to propose that all freshmen be housed on campus, and not be permitted to join a fraternity until the sophomore year. There were open discussions of this and related proposals, some of which I attended. The fraternities were concerned that if no freshmen were allowed to join them their income would be significantly reduced, since no one assumed that the percentage of upper-class students joining fraternities would rise. In fact, it was widely assumed that the percentage would drop once students were housed on campus. It was, of course, noted that we did not have enough beds to house all freshmen, and the *Boston Globe* kept reiterating this point.

One positive idea was to have a party along the Infinite Corridor one Saturday, with lots of food. The relative narrowness of the corridor forced people to mingle, and I thought it was a great event. I tried to do my part in moving MIT student life forward using the Institute's budget. For years student leaders had complained that MIT did not spend enough on student activities. There was a line item of about $100K to be disbursed by student activity boards, and many groups asked various administrative offices for funds for their special activities. The administration's reply over the years was that MIT did not have a student activity fee as many universities did, and the students refused to have one imposed on them, so there was an impasse. But in the academic year 1997–98 several things needed to be done to make the students feel better about the institution. The party was one. Then I announced that the following year the student activity budget would triple to $300K, with much of the increase to be spent on major events, such as outside musical groups. The students appeared quite happy with this announcement, if mentions of it in the student paper, *The Tech*, are any guide.

The 1997 campaign's academic wish list had included an undergraduate dorm, but construction was not expected to be completed for several years. Vest announced in 1998 that MIT would build such a dorm as soon as possible, namely by the fall term of 2001, because the decision had been made to house all freshmen on campus. Committees were created to figure out the new methods by which students would make housing decisions. Plans for the new dorm, Simmons Hall, were drawn up, and construction, somewhat delayed by an abutter's lawsuit, is now complete. The decision to house all freshmen was a significant step toward achieving the primary goal set by the Task Force on Student Life and Learning—the creation of a community on campus.

The Freshmen Alumni Summer Internship Program

Another initiative involving students that I began as provost, was based on my experience with coop students in EECS. My recollection was that, on the whole, these students were happier than ones who did not have such summer positions. I believe I understand a key reason: it had little to do with the money they earned in such positions, although the money did not hurt. MIT admits students who were at the top of their high-school class, a significant fraction of whom were class valedictorians. Of course, only half the MIT student body can be average or above-average, and this can be very difficult for many students to accept. Our freshman year, in particular, tends to make some students feel that they have suddenly stopped being as smart as they used to be. This is a false view, and good summer jobs, such as the coop program, can readily dispel it, because they bring students in contact with a more normal population than they find at MIT. The problem is that such coop programs begin at MIT after the sophomore year, and the self-esteem issue needs to be addressed during and just after the freshman year. I thought that alumni of MIT would understand the needs of these freshmen, and would also realize that these students were so good that even with a single year of college they can contribute a great deal in a technical coop position. I mentioned this to a faculty committee and at an annual gathering of alumni, and Arthur Steinberg decided to take me up on the idea. Arthur did an outstanding job, and the program is now being handled in the Office of the Dean of Undergraduate Education. Arthur advertised for coop positions among the alumni, and one who took him up on the offer was Teradyne, founded by Alex D'Arbeloff, the MIT Chairman. Arthur then advertised the program among the undergraduates, and even in its first year, several dozen students applied. Arthur devised a set of lectures in which the students learned about how they should present themselves in an interview and what they can expect in a job. He asked them to write a paper about their summer experience, to be presented in the following fall. The initial set of students contained a disproportionate number of women, especially Asian-Americans, probably because they were shy about applying for a summer position and wanted the program's help. The program has been a success, based on the students' reports, and lately 15 percent of all freshmen are enrolled in it, a sizable percentage considering all the opportunities available to MIT students.

Accrediting Harvard

In the fall of 1997 I spent a week on a committee that was charged with accrediting Harvard University. This was a distinguished committee as befits Harvard. Its chair was the president of Princeton, Harold Shapiro. It turns out that every university needs to be accredited once each decade. We started the week agreeing that Harvard clearly would be accredited. Our feelings by the end of the week were much more mixed. I became very impressed with Shapiro, whose twin brother was then president of McGill University. It is said that their first management experience was running their parent's Chinese restaurant in Montreal while their parents were on vacation.

Since Harvard is a very large and complex institution we were asked to concentrate on its undergraduate program in the School of Arts and Sciences. We met with the dean of the college, the dean of the School of Arts and Sciences, a number of the faculty, and a couple of dozen students. In addition the committee split up and interviewed certain individuals with specialized responsibilities. Some interviewed the president, others the treasurer. I met with the director of the Harvard Libraries, the largest university library in the country. I also met with staff associated with the computing and telecommunications functions at Harvard.

Early on we got the feeling that something was missing in the undergraduate experience at Harvard. The top private universities, such as Princeton, Yale and Harvard each claim to have a student faculty ratio of nine or ten to one. Yet Harvard seemed to have counted in the base some of its graduate and professional school faculties, faculties that rarely taught undergraduates. Thus we were not surprised to hear that in some popular majors, such as economics, undergraduates rarely saw a professor in a small classroom setting, until possibly the senior year. Many Harvard faculty members, we learned, liked to teach in front of a thousand students. The recitations in such subjects were taught by teaching fellows, usually graduate students. This system, of course, also helped fund such teaching fellows, which is especially important in fields where there is relatively little research support. This system was not used at MIT, Princeton or Yale, where faculty members often taught undergraduates in small classroom settings, nor was it true at Columbia. This difference became a key issue for the committee, and our interviews yielded a consistent picture on this point.

The committee members who interviewed the treasurer came back with the following view: Harvard is willing to spend a great deal of money on maintaining buildings, but is reluctant to hire additional faculty and staff given the long term commitment to salaries that arises out of such hires.. We interviewed the students and asked them about undergraduate teaching. I think they understood very well the Harvard system, and how it differed from other major universities. They explained to us how the Harvard housing system encourages them to learn from each other, given that there is relatively little contact with faculty. The dean of Arts and Sciences indicated that he was going to hire more faculty members. When asked how many more, he said about ten. Ten may have been a large number in the Harvard context, but would not have made much of a difference in the system as we saw it. The Harvard faculty seemed to feel that everything was fine, that the students were great, which they were, and that they had, with a few exceptions here and there, an adequate number of faculty members. We kept our views to ourselves until it came time to draft the report. Our report emphasized the system as we saw it, and indicated that we felt that a significant expansion of the faculty who taught undergraduates was needed. This report was sent to the accreditation board, and a version of it was presumably sent to Harvard for their review and comment. I am not sure whether the final report emphasized the issue regarding undergraduate teaching. In any case, I was pleased to hear that the incoming president of Harvard, Lawrence Summers, indicated a few years later that one of his goals was improving undergraduate education. Unfortunately, the dean of Harvard College, Harry Lewis, who would have been highly supportive of such an improvement was replaced as dean some time thereafter. Clearly Harvard undergraduates obtain an outstanding education. On the other hand, one could argue that Harvard could afford an even better education for its students. Summer's plans to build a new campus for Harvard in Allston, across the Charles River, are likely to be great for Harvard. There is just one problem. Harvard will increasingly compete with MIT in science and engineering, for faculty, students and funding. Likely this competitive situation will be a key issue for MIT's new president, Susan Hockfield. While I felt that Larry Summers was working on the right issues for Harvard, especially given my experience in this accreditation review, it is clear in retrospect that his personality and desire for significant changes made it difficult for him to govern, and as is well known, he resigned his presidency in 2006. I believe that the next Harvard president would be well advised to pursue goals similar to Larry's, but be sure that the faculty buys in on these goals and strategies for execution..

Balancing the Budget

As noted earlier, MIT's budget deficits had been on the order of about $10 million a year for a number of years—a cause for concern, but not a crisis. When it appeared that this figure would rise to $20 million or so, Mark Wrighton announced the six-percent budget cuts, and Bill Dickson began the reengineering effort previously described. Of course, at the end of the fiscal year we had to balance the books. There were various reserves that could be tapped to balance the budget at the end of the year. An interesting reserve existed because the MIT Treasurer was a wise investor. At MIT one can have three types of funds. Endowment funds are invested largely in stocks, bonds, and increasingly in vehicles such as venture capital and hedge funds. In addition there are funds that pay interest and funds that pay none. The Treasurer's Office invested some of the money in the interest-paying funds in the endowment fund as it rose in the 1990s. This portion of the funds thus earned well over the interest rate that we had promised the owners of these funds, largely faculty members and academic departments, and we could spend some of the increase in the value to reduce the deficit. This pot of money would dry up soon because we were spending it down so much, but it was useful in helping balance the budget while it was available.

My analysis indicated that deficits would grow to as much as $60 million a year over the next decade, partly as a result of changes in the RA/TA system and partly as a result of the construction and renovation program that we needed to undertake. I told Vest on several occasions that we needed a miracle to deal with the deficit. The miracle was underway, although it took me a while to recognize it: it was the growth in the value of our endowment. At the end of the academic year 1994–95, when I became provost, the MIT endowment was about $1.7 billion. At the end of academic year 1997–98, when I stepped down as provost, the endowment was close to $4 billion, largely due to market appreciation. If we could tap into that growth, I thought, the deficit would greatly diminish. There were a few problems before one could declare victory.

The growth of the endowment was a potentially transforming event. The transformation was in the role that endowment income could play in the Institute's budget. At the $1.7 billion dollar figure, and assuming normal growth of about 10 percent per year in the portfolio, and an increase in payout of 5% per year, the endowment income played an important role in the budget. At more than double the amount, and even more so at triple the amount, the endowment could play a much more significant role. The key players in determining what the endowment would actually pay out are the trustees, whose role is to ensure that the institution is in good financial health and that its endowment maintains its value looking far into the future. The Investment Committee of the trustees worries about the long-term value of the endowment, and decides to pay out an amount each year that will not put that value at risk. They are not, however, in the best position to know what it is reasonable for the institution to spend in order to maintain and even enhance its reputation as an educational and research institution. There is thus a built-in conflict. The endowment was in a position to help solve MIT's budget difficulties, but it was now necessary to convince the trustees that we should actually spend the money that the miracle of the growth in the endowment value had created. It is normal to spend about five percent of the value of the endowment. As a result of the tremendous growth in the market, and the annual 5% increase in payout which had been the MIT policy since the 70's we were able to spend only a little over 3%. There clearly was a need and a capability for spending more, but it is natural for the trustees to play a bit hard-to-get when their role becomes more crucial to the Institute, especially when prior analyses by the Budget Office and prior provosts had previously claimed a balanced budget five years out. The chairman of the MIT Investment Committee, Sam Bodman, was also chairman of the Cabot Corporation and a former faculty member in the department of Chemical Engineering. Mark Wrighton was on his Board of Directors, and they likely discussed my new projections for the MIT budget.

For my first two years as provost, we balanced the budget with temporary measures, as described earlier. But this would no longer work in FY99 because of the RA/TA effect that year. Alex D'Arbeloff, the new chairman of the trustees, worked on creating a new budget book that detailed income and expenditures by school. The Executive Committee seemed to like this expanded format, although I noted that it was not helpful from a management point of view since departments in each school vary so much. We needed a departmentally oriented budget book, and the Budget Office promised to generate such a book in the future. Even with this new budget book there did not appear to be a resolution regarding the need for a return to a higher payout rate. I finally told Vest in February, 1998 that this issue must be resolved. Soon thereafter, Vest formed a subcommittee of the Executive Committee of the Corporation to discuss major changes in the budget and the payout rate. Dickson and I were asked to prepare a case for the need for a higher payout.

Bill and I addressed three key budget issues. First, we needed to balance the budget as it stood without creating any significant new programs. The projection for FY99 was for a deficit of nearly $40 million. There were no tricks that would allow us to balance deficits of such magnitude each year. Second, we needed to deal with deferred maintenance to the tune of at least $20 million a year. Third, we needed a major infusion of graduate support, in the form of fellowships. MIT's cost for a research assistantship was high compared to our major competitors, even with the old RA/TA scheme. Some of our departments, such as Economics, had little research support and were competing with departments around the country that had much more fellowship support from internal resources. I had earlier created a $7.5 million fund for 150 fellowships, fifty each year for three years, named for former provost Walter Rosenblith, but this was a stop-gap approach. If these three wishes, a balanced budget, deferred maintenance, and fellowships, were added up, the annual total would be about $70 million, or nearly the net income from undergraduate tuition. The transformation we needed was sizable indeed.

If we increased the payout rate to about six percent, the gross change in income would take care of the three issues above, but the net flow to the bottom line would be far less than is implied by the six-percent figure. The reason is that a sizable fraction of the endowment sits in schools and departments. Some of it is in departmental chair accounts, for example. If the amount paid annually to a chaired professorship account is increased by 50 percent one year, but the salary of the professor increases only about four percent that year, much of the money will be idle. We needed to spend that idle money on the three issues. This required an agreement with the deans and trustees that a change in the payout would go to the bottom line for the purposes outlined. Most of the deans immediately understood the need for such an agreement. The key issue that remained was convincing the trustees, and Vest's process looked as if it would converge soon.

At that point I announced my resignation as provost as of August 1, 1998. Many people were surprised, but I still think it was a good time to leave the post since the critical issue for me, the budget shortfall, seemed to be resolved as a result of the endowment appreciation and the eventual agreement by the trustees to a change in payout. The campaign was about to take off, and Vest could use more support in fundraising than I was able to give, although the stock-market growth of the late 1990s made the original $1.5 billion goal easier to achieve than before.

Another set of reasons for stepping down as provost, which Vest recognized, was that I was getting itchy to do research again in a couple of related areas. Discussions in the Moses Seminar made it clear that it was a good time to revisit the fundamental issues in AI. There were tantalizing clues about the architecture of the brain in MRI brain studies. Since AI had not succeeded to developing a true learning machine, we might succeed in building one by learning from the work in related fields, such as linguistics, brain science, and cognitive science. A second related area of interest to me was the complexity and architecture of large-scale systems. This was a fundamental topic in SDM, but it was still an area where much not much of a fundamental nature from an engineering perspective was known.

My resignation as provost was announced in May of 1998. I had already arranged with Dean Zvi Galil of Columbia's Fu School of Engineering and Applied Science to spend the fall term of my sabbatical in New York City. This would allow us to be closer to our son Jesse who was studying in that school. In addition I could visit an aunt and cousin who lived in the City and whom I had not seen in years.

Chapter 11- Sabbatical, return to regular professorship duties, 1998-2007

I began my sabbatical in New York in the fall of 1998. My host, Zvi Galil, Dean of the Fu School of Engineering and Applied Science at Columbia, was very helpful in getting me started. He obtained an apartment for Peggy and me near the campus, and kindly let me use his Computer Science office, where I wrote the original version of these memoirs. The office next to mine belonged to Joe Traub who founded the CS department at Columbia. Joe was formerly chair of the Carnegie Mellon University CS department, and moved to New York to accommodate his wife, Pamela McCorduck, who was a writer. In fact, she has previously interviewed me for her well-known book, *Machines Who Think*.

One day Peggy told me that she found out that there were auditions for singing courses at Juilliard. Peggy knew that I was interested in learning how to chant Jewish prayers in the synagogue, and she felt that Juilliard would be the best place to learn to sing. I was reluctant to go to the audition, but Peggy was insistent, so I went. There were dozens of people auditioning for the introductory singing course, and when my turn came the teacher asked where my music sheets were. I said that I did not know that I needed to bring music with me, so she said "Go ahead and sing a song." I sang Kol Nidre, the solemn and majestic prayer that is sung on Yom Kippur Eve. The teacher was Scottish and evidently had never heard that song, but appeared to have been impressed, and so she said "I think we can help you." Little did she realize that not only did I not read music, but I also did not possess a good memory for music, and thus made several mistakes. In any event, I was called a few days later and officially admitted to the course.

The class of eight of us was composed of seven women and me. The others all appeared to have experience with choruses, mostly in their churches. I had no such experience. I needed to prepare a song for each week's session, and the teacher would offer criticism and advice. The class had a full-time Juilliard student who played the piano for each of us. He was also my paid accompanist at a session during the week, when I tried to prepare the song. I had difficulty keeping the tempo, which made me unique in that class. I also realized that I had an easier time singing songs in German or Hebrew, my original languages, than in English. Peggy also felt that I was better in songs in a minor key than a major one, since such songs were common in a synagogue. For my final presentation I practiced singing *Mack the Knife* in the original German. I bought a record of the song in German and practiced it again and again until I was able to get the tempo right. It was my best performance of the term. The teacher felt good about this performance, and wanted me to sing *If I were a Rich Man*, which I had not practiced that week, and sure enough all my problems surfaced again. In the closing meeting with the teacher she said something along the following lines: "A singer you are not, but an actor you could be." The line had an influence on me, as will be seen. In any case, she gave me a grade of B+, better than I deserved. Back at MIT, Chuck Vest heard that I was taking a singing course at Juilliard, and announced it at a faculty meeting, and got the expected laugh.

Peggy also took courses while we were in New York. She took courses on screenplay writing, both at a place near NYU and at Columbia. She also took a course on Jewish culture and traditions. Peggy had previously not been a fan of New York City, but this sabbatical's experience changed her mind. Previously we stayed in midtown hotels, but this time we lived in the Upper West Side, which is closer to the neighborhoods with which Peggy was familiar. Peggy shopped in the local supermarkets and went to kosher butcher shops about ten blocks from our apartment. We brought our dog, Curly, with us and took turns walking her twice a day. Peggy was concerned, however, about purse-snatchers near Morningside Park where we lived.

In October Peggy and I went by subway to lower Broadway. We got out at the World Trade Center stop and walked to the parade honoring the Yankees' recent world championship. Even with my height it was difficult to see much of the parade. Our son David has never forgiven us for this breach of etiquette, since Boston and New York are such rivals in baseball. This was the only time that either Peggy or I were in the World Trade Center.

We used the opportunity of the sabbatical to sample some of New York's nightlife. We went to the opera to hear Aida. I liked the way the Met displayed translations of the text on the back of the seats. I was disappointed that the Met used horses rather than elephants in the famous march scene. We also went to fancy restaurants. We celebrated the 30th anniversary of our first meeting, November 13, 1968, by going to the Le Bernardine restaurant. A few days before our scheduled dinner, the restaurant was declared by the latest Zagat guide to be New York finest restaurant. Its prices certainly were consistent with such a standing.

Jesse would come to our apartment every once in a while, and we would go with him to restaurants and shopping. He was a Computer Engineering major at the time at the Engineering School. David stayed at our Weston home, while attending Brandeis as a Computer Science major.

We met with some of my New York relatives. My mother's sister, Frieda Kauders, lived in Queens and had an aide to assist her. She was in her mid eighties when we visited her, but her mind was quite sharp. A highlight of the visit was the photographs of my mother that Frieda had that were taken while they were still in Germany. Frieda left Germany prior to the start of World War II, and thus retained more of her pre-war possessions than my parents did. She lived in South America, and came to the US with her children and mother-in-law after the death of her husband by a car accident. I also met with my father's nephew, Fred Brieger. Fred was a retired mechanical engineer who also lived in Queens. His wife became very ill and this took an immense toll on Fred. Fred had memories from his childhood of visiting my father's mother's family in the city of Posen in eastern Germany. Unfortunately, few members of that family survived the Holocaust.

New York City is a special place during the holidays. We went to Rockefeller Center and saw the people ice-skating. We also went shopping for gifts for our sons and Peggy's family. Although Vest arranged it that former members of Academic Council had a full year's sabbatical, I was eager to get back to MIT and renew my research career. So we returned to Massachusetts after the holidays. My Columbia host, Zvi Galil, remained as dean for nearly another decade and made me a member of his advisory council for a number of years. Recently he became President of Tel Aviv University, an institution that his father had co-founded decades earlier.

When I returned to MIT I did not have a teaching assignment that Spring, but it was known that I was around. Thus I was not surprised that the Dean of Engineering, Thomas Magnanti, called me in for a meeting. Tom was the founding co-director of the Leaders for Manufacturing Program, and helped launch the Systems Design and Management Program as founding co-director from the Sloan School while I was dean of engineering. Tom wanted to know whether I would agree to join the newly formed Engineering Systems Division (ESD). I agreed immediately, partly since I had long been in favor of the creation of such a division. Recall that one of my last acts as dean was to author a report that urged the creation of divisions that crossed departmental boundaries within the School of Engineering. For some months in 1998 I was questioned by members of the Corporation Executive Committee regarding when ESD would be created, and I said "Soon." Actually, Bob Brown as dean was pressing ahead in 1997-1998 to create a division on bioengineering. That division was formed first while I was provost. ESD was officially formed late in 1998 while I was in New York.

The founding of ESD was the culmination of many studies. The Large Scale Systems committee that I co-chaired with Dan Roos in 1989-90 was one such study. I do not recall that it led to a particular proposal, but the members of the committee enjoyed the discussions and it did create a community of senior faculty that had relatively common views of issues of importance in engineering. One notion that I recall discussing with the committee was the difference between open and closed systems. Closed systems, such as many large software systems, had relatively well-defined specifications, but might be complex due in part to their sheer size and intricacy. Many on the committee were familiar with systems that involved people in the core or the outside context, such as the construction of roads and bridges. Such systems are open systems. When I became dean early in 1991 I began discussions that eventually led to the creation of the System Design and Management masters program. A key document of the Long Range Plan for the School of Engineering written by me in 1993 was called "Engineering with a Big E." It argued for a broad view of engineering including policy and management issues. The title is a takeoff on "Manufacturing with a Big M" that was used by the LFM program. SDM was launched in 1995. Finally, a second long range planning activity in the school included a committee I appointed that was chaired by Tom Eagar. Its 1996 report argued for the creation of a division in the school that eventually became ESD. Some would say that ESD ought to have been a school in itself, akin to the Kennedy School at Harvard, but such a change would have been too difficult for MIT unless we had a donor with a naming gift of $100 million or more. We did not have such a donor.

The birth of ESD was not an easy matter. The engineering science movement had been very successful, especially in academia. The engineering systems approach made the most sense to people with significant experience in the practice of engineering, which most faculty members did not have. The reason I was so much in favor of this approach was that the development of MACSYMA gave me experience in practice of software design. Thus I understood the importance, for example, of designing systems with built-in flexibility so that the cost of the initial design plus the cost of the changes throughout the lifetime of the system is relatively low. Another issue with the engineering science faculty was that the engineering systems field relies on concepts from management as well as social science (e.g., economics, political science and sociology). Engineering faculty members were used to working with science faculty, but not so much with faculty members in these other schools. A key to the formation of ESD was that the Institute's leadership at the time, Vest, Brown and I were all sympathetic to the direction pointed to by engineering systems. ESD was founded with the understanding that there would be a review of it in 5-7 years.

Dan Roos was the founding director of ESD. He was the obvious choice for this role given his many efforts to create a structure in which education and research on engineering systems could be undertaken. By the time I joined ESD a few months after its founding there were already a couple dozen faculty members associated with ESD, each of whom was also associated with another department or even another school at MIT. Of course, I kept my association with the EECS Department where my chair, the D.C. Jackson professorship, was located.

Bob Brown had a creative idea in developing the concept of a dual faculty member. Faculty members at MIT usually had a home department, and some were asked to join another department with a joint appointment. Joint appointments did not have a clear minimum requirement for service. As a joint faculty member you probably ought to teach a jointly offered subject every year or two, and probably attend some additional faculty meetings, but your home department determined your promotion and raises, and you had better pay close attention to its needs. The advantage of a joint appointment is that you had the opportunity to have graduate students and faculty colleagues from another department and some access to joint research support that you might not have had otherwise, although MIT is quite good at fostering interdisciplinary research, teaching and student mentoring even without joint faculty involvement. I believe that a key reason for MIT's success in interdisciplinary activities is due to the fact that almost all its departments are top rated ones, in contrast to most other institutions, where some departments are top-rated and others are not. Hence the likelihood of success in joint research is high, and this fosters greater reliance on joint activities.

Dual appointments as created by Bob Brown were quite different from joint appointments. There was a written understanding that one half of one's time was to be devoted to one department and one half to the other department or division. This split applied to teaching, research and service. Dual appointment had one half of the faculty salary in one department and one half in the other. The change in status to a dual appointment thus cost the Institute nothing except for a bookkeeping change, although the initial home department often felt that it was losing half a faculty member. Many of the initial dual faculty in ESD came from the Civil and Environmental Engineering Department, and that department's administration at the time and a number of its faculty members were not happy with the arrangement. Another sizable contingent of dual faculty came from the Aero and Astro Department, and there was some, albeit far less tension there as well. A third large faculty contingent came from the Sloan School of Management, but all these were joint appointments, and this did not cause great difficulty for Sloan. In fact, the Sloan faculty greatly appreciated working with doctoral students in ESD and participating in ESD's research programs. The frustration in ESD was that we could not rely on Sloan faculty for core teaching needs in SDM, for example, since Sloan did not give them teaching credit for teaching ESD core subjects. With the addition of other faculty members from all but one of the engineering departments, there were in a period of a few years nearly fifty faculty members in ESD, about half of them were dual faculty and about half joint faculty.

ESD was founded with four pre-existing masters programs. The Technology and Policy Program existed since 1975. The Leaders for Manufacturing program existed since 1988. The System Design and Management program existed since 1995, and the Masters of Engineering in Logistics existed since 1997. There was also a doctoral program, called Technology, Management and Policy program, which I helped create in 1991. Several research centers were closely associated with ESD. The Center for Technology, Policy and Industrial Development traced its roots back to 1982. The Center for Transportation and Logistics could trace its roots even further back. Both were founded by Dan Roos. The Laboratory for Energy and Environment was formed in 1999 by combining the Energy Laboratory and the Program in Environmental Engineering Education and Research that I announced in 1992. ESD had about 8% of all graduate students at MIT and a comparable percentage of MIT's on-campus research volume. ESD thus started with a sizable complement of faculty, students and research support without requiring significant new support from the central administration.

Our vision for ESD is that it will lead a transformation in engineering education potentially comparable to the one led by Gordon Brown and others, which resulted in the Engineering Science paradigm. I particularly like the use of the word "system" in the title of this division. Engineering systems are getting increasingly large-scale and complex. We need to develop ways of designing, manufacturing, and operating such systems, whether they are hardware or software-oriented. Of course, one can view the brain as a very complex system, and thus both of my major interests converged. The transformations caused by the brain and cognitive sciences and aspects of AI research, and the engineering systems view of engineering, make for very exciting times indeed in the coming years.

In addition to increasing my involvement in engineering systems, I also wanted to renew my interests in Artificial Intelligence as I returned to the regular faculty. I felt that AI needed now to be based in part on advances in cognitive science and brain science. While I was at Columbia I visited Professor Eric Kandel at Columbia's College of Physicians and Surgeons. I had heard of his work on memory, and I wanted to try out my emerging ideas about the architecture of the brain. He was very kind to me, but said that there really have not been many theorists in biology. He gave me a copy of one of his books. Given my limited knowledge of the brain he decided to give me his undergraduate text, rather than the graduate one he had written. Eric won the Nobel Prize in physiology a few years later. I next met him at the inauguration of the new Columbia President, where I was MIT's representative and he introduced the incoming president on behalf of Columbia's faculty.

I called my view of the emerging approach to AI "neoclassical AI." The original approach of Minsky, McCarthy, Newell and Simon in the 1950's was classical AI. It relied heavily on reasoning from axioms and on search. My thesis introduced what came to be known as the Knowledge Based Systems approach. My critique in the thesis of classical AI was based in part on the belief that complex problems could not be handled well simply using reasoning from basic axioms. Simon and others bought into the need for knowledge in my approach, but as I noted earlier they did not understand, nor was I that clear about the difference in organizational structure that KBS represented to me. Minsky did not like the KBS approach because he felt that the expertise offered by KBS systems was in relatively narrow areas, such as symbolic integration. Children had the ability to learn about broad domains and this led to the so-called common sense reasoning of the child and adult in his and McCarthy's point of view. He was right about the lack of success of the KBS approach except in specialized areas. In fact, the rule-based expert systems approach of Ed Feigenbaum, popularized in the 1970's, failed for moderately complex problems as I had predicted it would. The reason I was unhappy with the rule based expert system approach was that I felt it would run into complexity problems as the number of rules grew and the rules began to interfere with each other. The weaknesses of rule based systems led to a significant decline in interest in AI in the mid to late 1980's, which has been reversed in recent years.

In the 1970's it became increasingly clear that neither the classical nor the KBS approach would result in intelligent systems rivaling humans in their overall ability. There emerged an approach that is called "Modern AI." Modern AI is an engineering science approach to particular capabilities, such as computer vision, robotics and machine learning. Modern AI tends not to be much concerned with how humans obtain such capabilities, but emphasizes getting computers to have high performance in each of the areas above. Another difference between the earlier approaches and the modern AI approach is that the former emphasized symbolic reasoning, and the latter emphasized statistical and numerical analyses.

Pat Winston and I in our respective administrative roles in the 70's and 80's hired AI faculty in the modern AI vein. The reason is that we felt that there was a limit to how well the classical or KBS approaches worked. What we did not realize was that eventually the number of modernists would dominate that of the former students of Minsky on the MIT faculty.

One of the issues that we discussed in the early years of the Moses Seminar was the neural net approach. This approach relied on a model that uses three layers of neuron-like devices in order to recognize objects. Minsky and Papert had shown in their 1969 book, *Perceptrons*, that single layer neural nets or perceptrons could not recognize connected objects. They felt that multi-layered nets would not do much better, but in the early 80's it was shown that three-layered nets could recognize many types of visual objects. These neural nets relied on statistics rather than symbolic reasoning, and were in the modern AI vein. I was interested in three layered systems for other reasons, largely related to ESD issues. Thus I was disappointed that the power of three layered systems was discovered before I had a chance to write up my views. I felt, however, that symbolic reasoning was needed in the mind at some level of abstraction, and in fact neural nets had difficulty with language issues, which of course involve symbols.

Noam Chomsky asked a fundamental question regarding language several decades ago. Why is it that children around the world learn roughly the same approaches to natural language syntax? Noam's answer is that the human brain is hard wired to use the same approaches to syntax, with surprisingly minor modifications for each language's syntax. I agree with Noam's thesis. My concern is not over grammar but over semantics. Noam specifically did not get much involved with meaning or semantics. This lack of involvement in semantics unfortunately affected generations of linguists.

I was unhappy with modern AI because it largely gave up on the goal of understanding the full panoply of human intelligence in order to build a capability, such as vision in a computer. I felt that progress in sufficiently understanding the meaning of sentences in a natural language was a key to achieving AI. Such an understanding likely required knowing how the brain represents and understands visual scenes and learns how to move the body in three dimensions. Vision and locomotion were built into the brain at a far earlier stage of development of the species than language, and it is likely that language understanding by a child relies heavily on such capabilities. For example, a two-year-old child's semantic understanding of a sentence, such as "Throw me the ball" is likely based on a visual mental model of the act of throwing a ball between two people. Since this approach returned to AI's original goals while using new knowledge gained in part from the cognitive and brain sciences, especially the architecture of the brain, I thought the term "neoclassical AI" was appropriate for it. I wrote a proposal to the dean and provost to create a research program in this neoclassical approach to AI, and I was very pleased that they were able to internally fund this work. My closest colleagues in this approach were former students of Minsky, namely Pat Winston, Gerry Sussman and Tom Knight. I had known each of them for over three decades at that point.

As I returned to full-time teaching and research in the fall of 1999, the EECS department assigned me to teach two sections of our introductory Computer Science course, 6.001. I had not taught a full subject at that point for nearly twenty years, but 6.001 was right up my alley. It was based on the language Scheme, a variant of LISP. Scheme solves a technical problem in LISP, which I had written about in a paper entitled "The Function of FUNCTION in LISP." This paper was not accepted for publication in the CS journal to which I submitted it because the reviewer felt that it was too narrowly based on LISP, not a widely used computer language, except in AI. The unpublished version of it is one of my most cited papers, however. The course 6.001 was developed by Gerry Sussman and Hal Abelson, and became one of the most popular courses at MIT. The text won awards, and was a key component of the series of texts that I helped establish in the 80's. I enjoyed teaching the course, especially the material in Chapter 2, which deals with polynomials among other examples. I had discussed such material with Hal Abelson in 1978, when I was slated to develop the new introductory CS course. Thank heavens that I became an administrator and thus Hal worked with Gerry to create their wonderful course, 6.001.

I actually learned some new material in the course. Someone had created a package in Scheme for Object Oriented Programming (OOP). This style of programming, where one sends messages to objects rather than calls procedures in the usual programming style, is fairly natural in LISP-like languages. The OOP approach allowed our students to design relatively complex projects in a short period of time. I recall one student developed a model of Middle Earth with its huge number of characters. The lecturer in 6.001, Eric

Grimson, was especially proud of that project, partly since the student placed him in Middle Earth. Eric taught 6.001 over two dozen times, including several times while he was head of EECS.

Early in the fall term of 1999, Chuck Vest asked me to meet him in the President's office. I assumed that he wanted to touch base on some administrative issue. As I walked into the room, I saw the provost, Bob Brown, the new Chancellor, Larry Bacow, and the chair of the MIT faculty, Steve Lerman. I recall saying that I knew what this had to be about when I saw all four of them. Chuck handed me a letter signed by him and Lerman that stated that I was made an Institute Professor, a position that only a dozen or so MIT faculty members have at one time or another. I told Bob Brown to watch out that he would be made Institute Professor too when his term as provost was up. Little did I realize then that Bob aspired to become MIT's president. He eventually became president of Boston University and Larry Bacow became president of Tufts University. Lerman later became dean of MIT's graduate school. Bacow said that they followed the usual process for appointment to Institute Professor, and that I fully deserved it. I was almost speechless at the meeting. Later I read the letter and noticed that the appointment began on September 1, a few weeks before the meeting. I then sent an e-mail to all four thanking them for the honor and briefly describing all the things that happened to me on September 1 – coming to America, "passing" my doctoral exam, being appointed Associate Department Head, then Department Head and now Institute Professor. Some of these appointments are due to the fact that September 1 is essentially the beginning of the academic year, but I was still impressed by the confluence of all these events on that date.

The EECS Department gave me a party to celebrate the appointment. I was roasted a bit by Dertouzos as LCS director and Guttag, the department head at the time. Tomas Lozano-Perez, the Associate head for CS, gave me a gift of a book on the Talmud. I made the mistake of pointing out that I had that book and started reciting the opening lines. Gerry Sussman had suggested the book to the department heads, and he got them later to buy me half a dozen more books in the same series. He made sure that I did not have those books already. I made a short speech that said in part that I knew the complex and confidential process of appointing someone as Institute Professor, and that I wanted to thank all the people involved in the process, even though I did not know who they were. Sanjoy Mitter told me later that he was chair of one of the committees, Dean Magnanti was another member, and Sanjoy said that Elwyn Berlekamp of Berkeley played a key role in explaining to them some of my early work in Macsyma.

Institute Professors have a prestigious title, but few other perks. They do get two of their summer months paid by the Institute as well as a scholarly supplement that all chair holders get. The president invites them to a lunch each term to seek their advice on broad issues, such as improved relations with the federal government. There is also an annual dinner with spouses and former Institute Professors. Officially there is no reduction in duties, such as teaching, research and service. Some, such as Tom Magnanti, even held a major administrative position, dean of engineering in his case, while an Institute Professor.

Although I was now an Institute Professor, I kept both of my departmental affiliations, in EECS as well as ESD. Roos asked me to chair a committee that was to deal with fundamental issues in engineering systems. The committee had exciting discussions for several months. ESD had meetings at the end of each term that lasted one or two days, called ESD offsites. I recall giving updates on our discussions at one such meeting. It was decided that the ESD faculty ought to have a conference regarding the foundations of the field we were creating.

Since we did not know whether we had any significant agreements on such issues, we decided to have an internal symposium to which we would invite very few graduate students and no outside faculty. The Internal Symposium finally took place in May of 2002. The symposium was organized by the ESD Symposium Committee, which I co-chaired with Tom Allen. The Symposium Committee wrote an overview paper with an appendix that defined 70 terms relevant to engineering systems. Overall we had 28 papers and nearly 600 pages in the printed proceedings.

A key set of issues that the overview paper said characterized engineering systems is that they are large scale, complex, have high or varying degrees of change and possess “ilities.” The latter term comes from the aerospace systems engineering community. Many important system properties tend to end in “ility.” These terms include flexibility, maintainability, scalability, sustainability, and reliability. Some important properties, such as robustness, do not have such an ending, but are also critical properties of engineering systems. I pointed out that we tend to emphasize in our engineering courses how to design systems that achieve certain functions with given performance and cost. Yet large scale systems tend to have changing requirements, and must have additional properties, such as the ilities. As a result of this co-authored paper I have been called “Mr. Ility” by some ESD faculty and students.

We were surprised by the outcome of the Internal Symposium. There was a great deal of alignment in our views. For example, flexibility was mentioned by several speakers as a key issue that they were interested in doing research on. There were repeated comments regarding the interest by ESD faculty in issues that involve the life-time of systems, such as sustainability. Frankly, the mood during and immediately after the Internal Symposium was more than a bit euphoric. I was influenced by the mood of the ESD faculty. Thus I was willing to work toward an External Symposium, one in which we would also invite our graduate students as well as faculty from other universities who were interested in similar issues. It would take two years of planning for that symposium to take place.

As I have noted, I have been overweight ever since I had an operation on polyps in my nose at the age of five. I had tried many approaches to losing weight over the years. The most successful approach was to stop eating one day a week. This occurred when I was 23 years old, and I lost 60 lbs. over the summer of 1965. It took several years for me to gain it all back and then some. To make a long story short, I weighed over 300 lbs. when I was sixty years old. I recall being in Gerry Sussman's office when a young AI faculty member, Gill Pratt, was talking about his wife, Janey Pratt. He said that she was a surgeon at MGH and that she did gastric bypass surgery on patients like me. That woke me up to the idea that I should try such a surgery. I found out that the waiting time to get into the MGH Weight Center was six or more months, but I was able to get an appointment within a month due to Janey's influence. I was interviewed by a nutritionist, a psychologist and Dr. Lee Kaplan, the director of the program. They decided to permit me to have the operation, although I was older than their average patient. I found out that there is a risk of death in this operation (less than 1%, but still nontrivial). As a result of this information, Peggy was opposed to my having the operation. My PCP at the time, Ralph Freidin (also a Columbia grad), said that the operation would change my life. I do not know why he did not previously recommend the operation to me.

I first had to enter an educational program, led by a nutritionist. The program lasted several weeks of evening meetings. I also went to meet the surgeon, Dr. Randall. I learned later that he had more experience with the surgery than anyone else in Massachusetts, which is a very good sign. I had the operation on Friday, June 14, 2002. The anesthesiologists had some difficulty giving me an epidural, but were able to give me a general anesthesia. I was told that the operation went well and that I would likely leave on Tueday. That weekend I felt quite weak and started spewing blood. On Sunday the nurse wanted me to get up and sit on a chair. I tried it, but fainted. The spewing of blood continued, I complained and asked that they do something about it. Finally they inserted a catheter in my throat down to the stomach in order to get the blood out. Dr. Randall came to visit me, and said that I would be going home on Tuesday, as scheduled. I did not believe him. He felt that the spewing of blood was due to my nervousness prior to the operation, which showed up when they had difficulty with the epidural. Peggy and our sons came to visit, and I made them feel bad with my condition. I was later given two pints of blood to restore the blood in my system. Amazingly, I was indeed able to go home on Tuesday after all.

The following weeks were not easy to endure, since I could eat relatively few things without "dumping" them. But slowly I began to lose weight and the types of food that I could eat began to approximate normal food. I continued to go to the Weight Center and within a year I lost 100 lbs. In retrospect the operation did change my life. I suffered from sleep apnia before the operation, and it was gone a few months after it. My cholesterol went down, my blood pressure went down, and my blood sugar level became normal. I also did not have significant side-effects after the hospital stay. I do have to take extra vitamins and minerals, such as iron, the rest of my life since the remaining intestines do not absorb some things. I also have to be careful about eating foods with significant sugars and white flour. I gained some of the weight back, more than I had hoped, but overall the operation was a great success.

I only learned afterward how serious the complications of a gastric bypass operation could be. Charlie Weiss, the offensive coordinator of the New England Patriots, had the operation a week before me at MGH, using a different surgeon. Apparently some of the staples failed to work while he was in the hospital and he started bleeding internally. They gave him 7 pints of blood, a huge amount, and a priest gave him the last rites of the Catholic Church. He nearly died before they redid the surgery. Some of his complications persist to this day. He became head coach of Notre Dame, and was quite successful in his first years as coach. He sued his doctors, and the case went to trial in 2006-7, and he lost the suit, largely on the grounds, I believe, that he knew the risks of such an operation. Notre Dame did very poorly in its 2007 football season. I do not know the relationship of this result to the outcome of the trial.

Jesse graduated from Columbia with an engineering degree in 2002. Late that year he fell down the staircase in our house in Weston and hurt his left knee. The doctor who examined the x-ray suggested that Jesse see a specialist. That doctor suggested that Jesse have a biopsy on his knee, and when the results came back he said that Jesse had bone cancer (osteo-sarcoma). The next several months were hectic and put a lot of strain on all of us. Jesse began chemo-therapy in the Dana Farber Cancer Center. Jesse was visited at our Weston home by several members of our temple in Lexington. One, Hans Herda, a mathematics professor in U. Mass, Boston, even went so far as to accompany Jesse to Dana Farber for one of his chemo sessions. After several treatments Jesse had a seven-hour operation to remove the cancer from his left knee, and place a bone graft in it. This was followed by more chemo-therapy. Jesse, Peggy and I then went to meet with his oncologist, Dr Demetri. He told us that when the cells taken out during the operation were examined, only 30% of the cancer cells were killed. Hence he increased the amount of the chemicals used in the following chemo sessions. However, Jesse fainted after one such session. Therefore Demetri was recommending that the chemo be stopped, and that we enter a phase of “watchful waiting.” He pointed out that the M D Anderson center in Houston likely would have recommended increasing the dosage further even with the side-effects, but that he did not recommend this course of action. We agreed to follow his advice.

Jesse was given a scan every few months. They were especially interested in his lungs, since this is the most frequent site for the bone cancer cells to proliferate. Unfortunately, about a year later there was a spot found in his lungs. The oncologist said to wait to see if the spot changed, but Jesse asked for a second opinion from a surgeon, and she said to remove the nodule from his lungs. Jesse decided to undergo the operation, and the nodule was removed and was indeed cancerous. Jesse has been examined for the past three years and thankfully no further spots have been found.

The situation has had a big impact on Jesse. He began to develop a massive web site of information for his cancer (www.osteosarcomasupport.org). This has taken much of his time. Lately he has begun to develop web sites for other diseases, such as bipolar disorder, partly since osteo-sarcoma is a relatively rare cancer, and thus not many people visit his cancer site.

In preparation for the External Symposium, I helped create six committees whose goal was to write a position paper on their assigned topic. The topics were: system architecture, uncertainty, enterprise perspective, system safety, sustainability and context. I was on the architecture committee, but I paid attention to all the others. We spent time at some of the subsequent ESD offsites letting the committees meet, discuss their topics and present updates on their progress. Frankly, progress was uneven in the six committees. Eventually, I had to disband the context committee, largely on the ground that they were not making sufficient progress according to their ESD colleagues. We wanted to prepare a monograph for the Symposium, and the closer we got to the March, 2004 date of the Symposium, the more writing took place in the remaining five committees. I wrote an overview paper for the committee reports, which once again emphasized the ilities, but also dealt with system complexity among other issues. We asked Dan Roos to write a paper on the history leading to the formation of ESD, and the new co-director of ESD, Dan Hastings, wrote a paper on the education of engineering leaders. The monograph papers were presented at the Symposium and were placed on ESD's web site.

The ESD Symposium was a major event. Three hundred and fifty people attended. The first two days were devoted to plenary discussions of the emerging field of engineering systems. The president of MIT, Chuck Vest, spoke, as did the president of the National Academy of Engineering, Bill Wulf, and the Deputy Director of the National Science Foundation, Joe Bordogna. There were many other outstanding speakers from industry, academia and government, both from the US as well as Europe and Asia. All were supportive of the importance of the issues we in ESD had an interest in. The monograph reports were presented on the second day. The third day was devoted to presentation of submitted papers. Fifty presentations were made. The final session had summary comments, largely from the audience. What I recall best were comments by Heinz Stoewer, a leader in systems engineering. I recall that he said that he had thought that systems engineering was very broad, but that MIT's view of engineering systems was even broader, largely because of the social science aspects. He congratulated MIT for taking such a bold step, which he felt was much needed. Comments such as these during that session brought tears into my eyes. I felt that the long efforts that we had made to create the field of engineering systems were beginning to pay off. My ESD colleagues felt on the whole that we made a good case for MIT's leading position in this emerging field.

My mother had moved from New York to the Boston area in 1989. Peggy was a wonderful daughter-in-law to her. She helped get her into Jewish housing for the elderly, and after my mother began to fall in her apartment on a frequent basis, we moved her to Hebrew Rehabilitation Housing for the Elderly in Roslindale, a section of Boston. In her late 80's she began to increasingly lose her memory. In the fall of 2004 she had a series of mini strokes. The doctors told us that the end was near, and my mother died in November 2004 at the age of 90, almost 25 years to the day after my father died.

I felt that given all the incidents in my mother's life, her weight problems, and the colon cancer she had in the late 1980's that she was lucky to reach the age of 90. I invited many of my MIT colleagues, especially members of the Moses Seminar, and Peggy invited her family to the ceremony that was held in Brookline. Our new rabbi in Lexington, Rabbi David Lerner, officiated, and he gave a eulogy based on interviewing Peggy, me, and our two sons. I prepared a talk about my mother's life based on Chapters 1 and 2 of these memoirs. The talk went well. In fact, I was asked to repeat it to the entire Temple Emunah congregation on a Saturday a few months later.

The burial was at Mount Lebanon in Queens, New York. My mother was buried next to my father, as planned. We were able to obtain a local rabbi to officiate. I went and saw the graves of my grandmother, uncle and aunt, all of whom are buried in that cemetery. Afterward we had lunch with some of the family members who came to the burial ceremony. My aunt's son and daughter, Robert and Ruth, came, as well as my uncle's former wife and son. Fred Brieger was the only member of my father's family who came. We passed photographs around, and promised to keep in touch.

As I did with my father, I kept the Jewish traditions following a death of a parent. We had special evening services at our home for a week. I then went to the synagogue each day for nearly a year. In contrast with the time 25 years earlier, when my father had died, the synagogue was better organized for the daily services, and I rarely had to pitch in and lead them. We also rarely missed getting a minyan, the ten men (and now women as well in our synagogue) needed in order to be able to say the Kaddish prayer for the dead. At the end of a year of mourning I flew to New York for the unveiling ceremony.

Dan Hastings became the Director of ESD in 2004. One of his goals was to develop an ESD doctoral program. Recall that we had a doctoral program entitled Technology, Management and Policy that was created while I was dean of engineering. The ESD doctoral program was to be broader than TMP, and was to include topics, such as system architecture and enterprise structure. I was involved in the process of getting the new program approved. That is when I realized how complex such an approval process is, a process I somehow avoided in the TMP program. We had to explain the program and its requirements to committees, such as the Committee on Graduate School Policy. Finally, we had to present the program at a meeting of the MIT faculty. The ESD PhD program was approved. In it first few years the percentage of accepted students who decided to come was over 80%, a very high figure. I believe that this means that the program is viewed by the admitted students as a relatively unique one. Our long-term goal, however, is to transform the standard engineering departments around the world by introducing systems concepts into their teaching and research.

One of the issues we had to face in the doctoral program was the lack of core subjects in some of its areas. While we had a systems architecture course in SDM, such a course was geared to professional masters students, not doctoral students in need of finding research topics through reading of the literature. That course also emphasized the systems engineering approach to architecture (that is, architecture is an early phase of the overall system engineering process). I, on the other hand, viewed architecture as a separate field of engineering systems. I asked my colleagues, Chris Magee and Dan Whitney, to help me develop one such course in system architecture. We had co-authored the monograph report on system architecture. We spent part of the fall of 2004 discussing the topics to be presented in the course in spring 2005. I found out over the following years that increasingly Magee and Whitney had become fans of modern network theory, a theory developed in large part by physicists.

Early in the fall of 2005, Dan Hastings talked to me about the advisability of his becoming MIT's Dean of Undergraduate Education. Dan was associate director of ESD, then co-director and finally director of ESD in 2004. I pointed out that much of the dean's function was taking care of day-to-day issues, such as undergraduate admissions and oversight of the freshman programs. The office had a large number of staff, and relatively few faculty members. Dan later admitted that he used my arguments against his taking the position as a way of reorganizing the office by creating an associate dean who was to be in charge of many of the day-to-day activities. Dan's goal was to spend much of his time as dean implementing the forthcoming recommendations of the Task Force on the Educational Commons.

Dan's acceptance of the dean's position gave the administration about one month to find an acting director of ESD. I agreed to take the position with the understanding that there would be a search for a new director, and implicitly that I would serve as acting director of ESD for only a few months. Actually Susan Hockfield, the new MIT President, did not like to create "acting" positions, she preferred that people be called "interim" directors, but I insisted to Tom Magnanti that I be called the acting director. The reason goes back to my class in Juilliard where the teacher said that I was not a singer, but I might be an actor. I wanted to see how good an actor I could be.

I became acting director of ESD on January 1, 2006. Dan had little time to straighten out his ESD activities or projects. I found out in relatively short order that there were over 30 different activities associated with ESD. Initially I felt that this was too large a number of activities, and I stopped or slowed down some of them, such as a new Engineering Systems Symposium. Eventually, I became comfortable with almost all the ventures, and even added a few myself.

The ESD Director Search Committee under Sheila Widnall was late in getting formed. The Committee eventually produced a short list of candidates to the dean in July 2006. A key reason for the delay was the Committee's desire to use the report to the dean and provost as a response to the recent oral report of the ESD Review Committee. The MIT Faculty resolution that formed ESD said that there ought to be a review of the Division 5-7 years after it was formed in late 1998. Provost Robert Brown formed the Review Committee in March 2005. The ESD Review Committee presented Dan Hastings with its preliminary recommendations in November 2005, and Dan Hastings informed the ESD Council on which I served of these recommendations. The recommendations sounded reasonable to us, and we had no concern that ESD could implement most of them relatively soon.

Dean Magnanti held a meeting with Hastings, Roos and me in March 2006 to hear the final version of the report. This version had an introductory section with a fundamentally different tone that Hastings had not heard earlier. Frankly Hastings, Roos and I were shocked by the introduction of the report. We asked Magnanti and the Committee chair, Ahmed Ghoniem, modify the introduction, but eventually the Review Committee chose not to do so. As a result the written report was never released. The Review Committee met with the ESD faculty in May and presented their oral report. The faculty was quite somber in hearing it. This led me, and somewhat independently the ESD Search Committee, to write lengthy responses to the Review Committee's Report.

The Review Committee Report had a significant effect on my term as acting director. Candidates for the position felt that MIT needed to provide ESD with significant new resources in order to deal with the challenges presented by the Report and by ESD's situation within the School of Engineering. This delayed my replacement as acting director even further. In retrospect one could not be too surprised by the negative tone of the Report since many of the people interviewed by the Committee were engineering scientists, and ESD can be viewed in part as a critique of engineering science and an argument for broadening engineering education to include greater connection to the practice of engineering that ESD provides.

The ESD Review Committee made its oral report to the entire MIT faculty in September 2006. Ghoniem's report was quite long, and introduced some new issues. I made a relatively short response, based on a recommendation of the ESD faculty that we not give a detailed response. I emphasized, for example, the quality of the faculty in ESD. Ghoniem introduced the notion that MIT ought to not have a department that is not viewed as one of the top ones in its field, and he gave national rankings in fields, such as Systems Engineering. In the discussion that followed both presentations, it was pointed out that ESD is broader than the areas noted by Ghoniem and never would wish to be ranked in those fields. Although much of the discussion was relatively favorable to ESD, the impact of the Review Committee report still cast a significant shadow on ESD.

The Visiting Committee for ESD was to meet in the fall of 2006. Since I was still acting director, there was a discussion as to whether to delay the meeting. I suggested to Art Gelb, chair of the Committee, that we go ahead at the scheduled time in November. I planned to have a significant portion of the discussion with the Committee be related to the ESD Review.

The ESD Visiting Committee membership is quite prestigious, even relative to other MIT Visiting Committees. For example, it has as a member Norm Augustine, former chairman of Lockheed Martin, as well as John Reed, former Vice Chairman of Citibank. Among other reports, we sent them copies of the ESD response to the Review Committee, although we could not send them a copy of the Review Committee report itself. The VC's immediate response at the meeting was that MIT should recognize the importance of ESD to MIT and basically declare ESD a success and stop evaluating it, except in the usual way all departments are evaluated, such as via a Visiting Committee meeting. I was very pleased with this view, which was communicated to the MIT upper administration at the end of the meeting. I reported this view to the ESD faculty after the VC meeting, and they were comforted by it.

Epilogue

In answering the questions regarding why I had rebelled against classic AI, why I was unhappy with Simon's hierarchical view of organizations and why I was unhappy with the limited view of engineering science, I come back to the beginning of this story. I think cultural backgrounds have a deep impact on the way one thinks about certain issues. My parents came from Germany, and my father especially was deeply influenced by German culture. Herb Simon was born in the Midwest and had an affinity to British philosophical views, as did many others in the social sciences in the US in the 50's. In a sense, Simon was close to a modern version of Aristotle – logical, factual and scientifically oriented, with a tendency to using tree structures as an organizational form. I am closer to a modern version of Plato - relatively abstract, idealistic, holistic, with a tendency to using pyramids as an organizational form. The distinction shows up very clearly in the Herrmann Brain Model, where the logical thinkers tend to be in one quadrant and the holistic ones in another quadrant. Although Plato was not an engineer, his views are consistent with those of architects of large scale systems that I know.

Some people are name droppers. I dropped out of fields in my career, largely because I did not like the dominant approach to problem solving and design in these fields. I dropped out of AI for that reason. I moved to computer science, but became unhappy with software engineering, which is at the core of CS, when I realized that it was using an approach similar to that used in classical AI – a top-down, tree structured design methodology. A few years later I became an academic administrator, and soon thereafter I realized that the dominant theoretical approach to organizational structures was also developed by Herb Simon. As I noted earlier, it was only when manufacturing became a key issue in the US in the 80's that I began reading about Japanese culture and approaches to organization. In particular, I was influenced by Ouchi's book, Theory Z. I finally found an approach that I was comfortable with, and only later did I connect that with my father's German background. I am not surprised that Germany and Japan were on the same side in World War II. Their cultures have significant similarities. This does not mean that their approach to issues is always ideal. In fact, in my view there is no approach to complex issues that works best under all circumstances. It is best to know multiple approaches and understand when each is best, or when a combination is best. Cultures that are based on multiple religions, such as Germany's and Japan's cultures or for that matter China's and India's, tend to have such multiple approaches.

Academic engineering in the US was radically changed as a result of World War II and became Engineering Science. Simon's title for his wonderful little volume, The Sciences of the Artificial, indicates fields that want to be called sciences, such as design science, management science, cognitive science, and even AI. Engineering Systems recognizes the need for holistic approaches to engineering design, approaches that include management and social sciences as well as engineering. The engineering science approach to most design problems emphasizes optimization of a solution based on fixed specifications. The engineering systems approach recognizes that specifications for complex, large scale systems cannot be fixed, that one thing that is fixed is that such systems need to be able to change. I have therefore emphasized system properties that are related to change, such as flexibility and robustness.

Rather than optimize a systems for a fixed set of specifications, one ought to design a system to be able to cope with the types of changes it is likely to encounter in its lifetime. Such an approach will lose some efficiency and performance, but not necessarily lose a great deal of either. The architecture of highly flexible systems will rarely be a pure tree structured systems. Abstractions will likely play a key role in the architectures of flexible systems. That is probably why I was attracted to mathematics and to my approach to Knowledge Based Systems.

One thing that I learned in the Engineering Systems Division is the different assumptions made in different fields of engineering. Although many engineering fields discuss the concept of flexibility, they tend to mean different things by it. For example, in civil engineering one does not want to make many changes during the lifetime of a system, such as a bridge or a parking garage. The design of a parking garage with four stories that has the option of being increased to six stories can be considered a flexible design in civil engineering. In computer science having just two options is usually not very interesting. Having a billion or more options is often possible in computer systems, and results in their complexity. Abstractions significantly reduce such complexity in many cases.