NOW IT'S MY TURN

VANTAGE

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Engineering My Way

Eric A. Walker

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by Eric A. Walker

Although crossing the Canadian border under the not-too-watchful eyes of a sympathetic guard may not be the first ingredient in most people's idea of a menu for success, it seems to have worked okay for Eric A. Walker. From such humble beginnings, Dr. Walker went on to study engineering at Harvard, moved on to important defense work during the Second World War, served on numerous academic committees and financial boards, and finally became president of Pennsylvania State University, a position he held from 1956 until 1970.

Of course, it would surprise no one to learn that *Now It's My Turn: Engineering My Way* is an autobiography, but Dr. Walker seems to be much more at home talking about other people than about himself, and this is what makes the book so readable. While he came into contact with numerous important figures in the worlds of academe, government, and industry—many of whom are given informative profiles—Dr. Walker doesn't forget to honor the people from his boyhood and developmental years, the people you don't get to read about in other history books: like the boss at the iron foundry who bombed his employees' makeshift latrine on the banks of the Susquehanna; his mysterious roommate at Harvard, who dressed like a dandy and drove a Packard, but who returned to the obscure mining town from which he'd come after deciding that higher education was not all it's cracked up to be; or the restaurant worker whose jelly-bean ice cream was such a success he thought he'd try radish and onion varieties.

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the Commonwealth Campuses, Cline says. "We have LIAS (Library Information Access System), an online public access catalog of the Libraries' holdings, and have also implemented a network of telefax services to answer requests for information from other campuses and organizations."

The trouble is, the price of even basic services is skyrocketing.

Cline estimates the cost of nearly 30,000 subscriptions across the libraries system will approach $3.3 million this year. By next year, the same subscriptions will cost from $396,000 to $450,000 more.

"And the faculty tell us even this number of journals is not enough," Cline sighs. "And they're right."

Will the work end when the Campaign for Penn State ends in June 1990?

"We expect to continue high-profile fund raising after the Campaign," Foster says. "We have to. Library costs, like the journals, are rising rapidly and Penn State has to keep pace. We want to promote a spirit of continued giving to the Libraries."

"We're a strong board, and we still have a lot to do," he adds.

For more information, contact: Office of the Dean, University Libraries, The Pennsylvania State University, E505 Pattee Library, University Park, PA 16802, phone (814) 865-0401.

Penn State Update

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Editor **Jo Rider Chesworth '60**

To join the Association, return the membership application from page 1. Or contact the Alumni Office, 105 Old Main, University Park, PA 16802, phone 814-865-6516.

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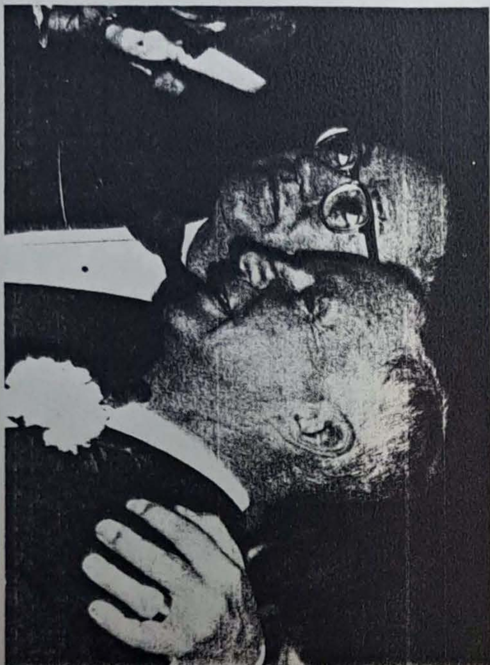
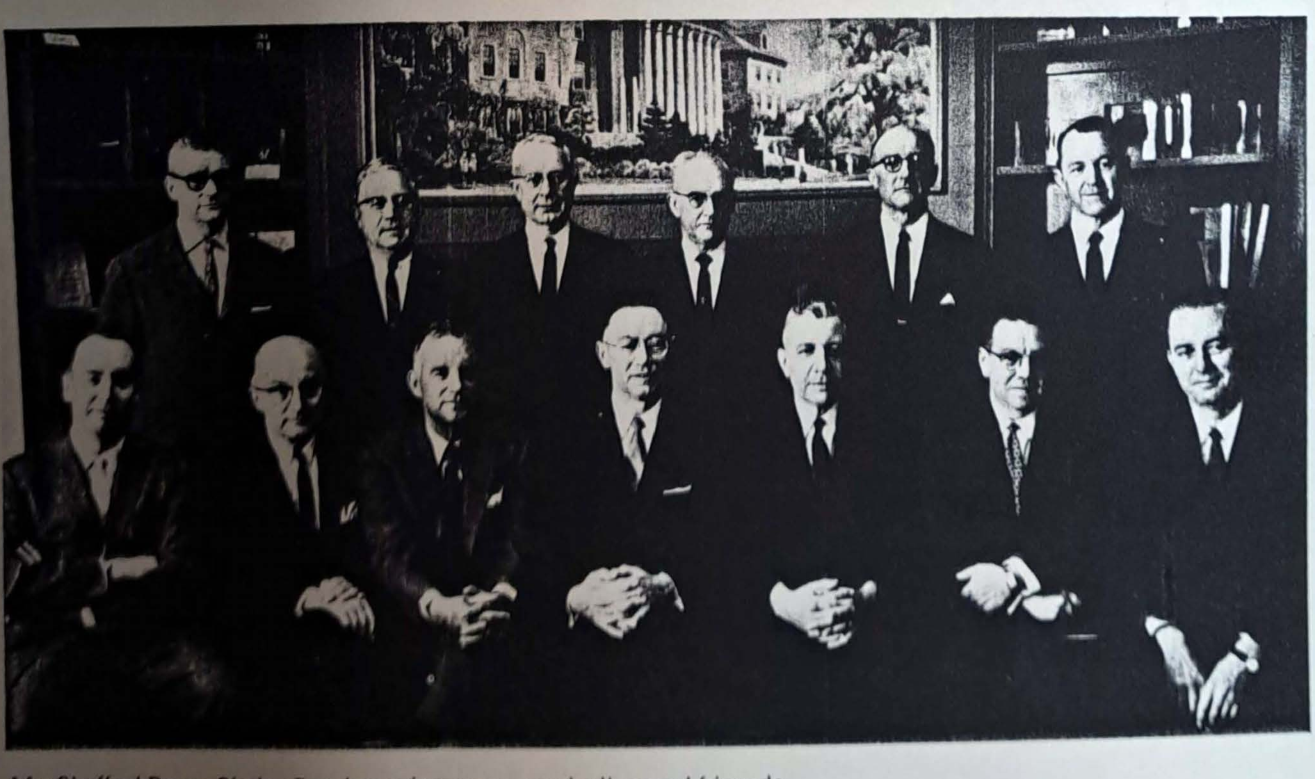


With my good friend McKay Donkin (photo by Bill Coleman).



My Staff at Penn State: Twelve wise men, and all good friends.
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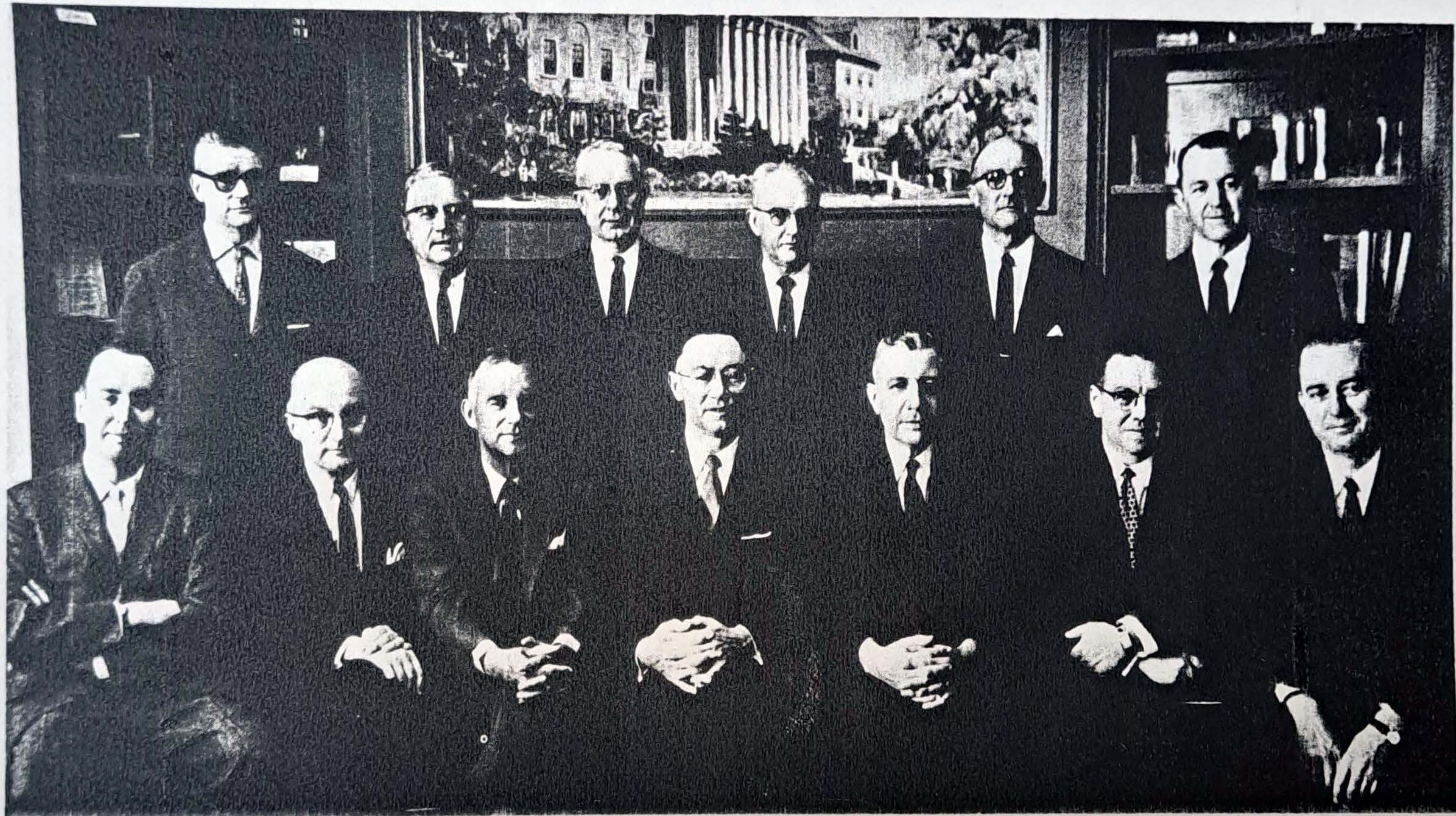

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PROLOGUE
What Engineering Means to Me

Science is wanting to know about things simply because, as climbers say about mountains, they are there. Engineering is wanting to make things because they seem likely to make life easier or pleasanter. Cost is always a challenge for engineers, since excessive cost may outweigh the gain in ease or pleasure resulting from technology. Technology is the fancy name for the fruits of engineering. Cost is usually a consideration for scientists, as it is for engineers, although excessive cost in science is hard to judge. When is space exploration too costly? Cost includes social cost—that is, damage to the environment or the quality of life. Social cost is increasingly important.

Cost must be differentiated from value. Cost is primarily an economic concept, whereas value has moral and aesthetic meanings. Both concepts imply judgments, though value is more subjective than cost. The value of an engineering project—a labor-saving device, for instance, or an improvement in transportation—is usually established by consensus. But there is no accepted way to put a price tag on the scientific discovery of knowledge. In science, cost is what it takes to find out, while value is the potential return to posterity. Unlike engineers, for whom cost is a central concern, scientists are only peripherally concerned with cost. Engineers typically work within budgetary limits, whereas scientists usually expect some outside agency to pay for their work. Both science and engineering involve politics and management—or administration, if you think management implies manipulation—because engineering and science are human activities and they therefore need consent and direction. Politics has been called "the engineering of consent," and management has been called "the art of steersmanship," both of which are definitions you may quarrel with but that make my point.

Engineers and scientists must know how to focus their attentions without developing tunnel vision. The need for focus is the same whether one is learning how to understand atoms or blood cells or how to make an engine or an artificial heart. Focusing on an objective, however, should not blind one to serendipitous findings but only to irrelevant distractions. Distinguishing between serendipity and irrelevancy obviously takes talent or training—or some combination of the two.

How much "formal" training is needed by a scientist or engineer? 'here is obviously no clear answer, any more than there is in the arts. ·hakespeare, Jane Austen, Mozart, Franklin, and Edison did most of ıeir learning by doing. Milton, Handel, Newton, Marie Curie, and Stein-ıetz attended universities. Talent is the key, and there are many ways ı nurture talent. Clearly, formal education has never stifled talent, al-ıough it may stifle initiative. A major challenge to educators is the evelopment of scholar-explorers—academic Columbuses.

Do great scientists and engineers tend to be "loners"? Again, there s no clear answer, any more than there is in other fields. A glance at the ames in the preceding paragraph shows a wide range of gregariousness. :ven in philosohpy, whereas Kant had solitary habits, Plato was a social eing. Franklin was a great "joiner." Darwin did much of his work at ome because of ill health after his voyage, though he had frequent isitors. Edison founded his own laboratory but had associates there. 'ommittees and societies can be distractions or substitutes for action; yet will testify that the National Advisory Committee on Aeronautics con-ributed to the allied victory in World War II and that the American ·ociety for Engineering Education has contributed to the training of en-ineers.

I was a scientist before I was an engineer. My scientific career began ı 1924 in the Wrightsville (Pennsylvania) Electric Club and the Boy couts, when I made a primary battery and an electric motor simply to ee how these things worked. Actually, I had another motive: becoming n Eagle Scout. Thus curiosity was not my sole inspiration, and I shall eturn throughout my story to the question of motivational purity even mong "pure scientists." At the same time, the battery and motor were f no practical use to me, and I would not have chosen the electricity ıerit badge in the Scout manual if curiosity about the subject had not ed me into the Wrightsville Electric Club, membership of three. My ehavior at this point in my fourteenth year was that of a scientist.

My engineering career was also sparked by the Boy Scout handbook, /hen I read its directions for making a crystal radio set. Here was some-ıing I could use for entertainment and prestige—two needed, or at least esired, objectives for a relatively poor and socially insecure immigrant oy. Uncle Arthur, my guardian at the time, gave his consent to my ıaking the radio—as he had approved the battery and motor projects—after ome politicking by me. My radio politics required convincing a fire-ısurance agent, and thereby my uncle, that my lightning arrester on the aerial would keep our house from catching fire. Although there was no management involved in building my first radio, I had previously had to manage Uncle Arthur's purchase of the sulfuric acid needed in my first scientific experiment: making a wet battery.

The crystal set led me to opt for engineering over science. To begin with, I wanted a better radio for myself, one powered by batteries and using vacuum tubes instead of a crystal. Making the new radio required a lot of my time and energy to get the thing to work—not just to understand it, the goal of the scientist. My reputation concerning this radio led to my first consulting fee. A local lady had received a superheterodyne radio as a Christmas gift from her boyfriend, and neither she (an artist) nor he (an effete New Yorker) knew how to assemble it. On someone's recommendation, I was called to hook up the components: tubes, batteries, horn speaker, and aerial. When I finished my hook-up and the thing worked, the boyfriend gave me two dollars—more than twelve times the hourly pay I was used to—and I thereupon resolved to become an engineer and get rich. I also enjoyed being treated like a wizard by the lady artist and her New York boyfriend, as well as the general citizenry of Wrightsville. Both wealth and glory seemed to be the wages of engineering.

In becoming an engineer I might have chosen the path of learning by emulating Edison or the Wright brothers, since I enjoy tinkering, but I chose the path of formal education. My choice was strongly influenced by the social environment of Wrightsville, York County, Pennsylvania, where seeking and accepting a scholarship to Harvard seemed only right and natural. I mean, it seemed right and natural for the upper crust of Wrightsville's layered society—for the well-to-do people who lived at the top of the hill rather than the laboring people who lived at the bottom. My family lived halfway up. Going to Harvard also gave a boost to my ego, since I was unwealthy, unhandsome, foreign born, and without social standing. Perhaps, if I had felt more psychologically secure, I would have skipped college and plunged right into making things. On the other hand, I probably would not have done so because I am not gifted with the dexterity needed by a great mechanic like Henry Ford or Orville Wright.

A few engineers are great mechanics, but the two vocations are different. I have spoken of the engineer's desire to "make things," but I should revise that to read "be involved in making things." Does that mean "design?" Well, some engineers are designers and some are not. William Caxton, the first English publisher, directed that the following legend be printed in his books: *Caxton me fieri fecit* ("Caxton made me to be made"). That describes the engineer's role: An engineer arranges for things to be made, coordinating the roles of various craftsmen and technicians, sometimes, albeit not necessarily, playing some of these roles. Engineering therefore resembles publishing or architecture or producing in the performing arts.

Which brings me to some other resemblances. Scientific research, it seems to me, resembles research in the humanities. Seeking a fact or relationship in history or in our heritage of artistic expression is not intrinsically different from seeking a scientific fact or regularity. The humanities also have their engineering or technology. Stagecraft and movie or television production are obvious examples, but I contend that journalism, public speaking, and expository or persuasive writing are more technological than exploratory. Art is a special case: it may use the techniques of engineering, but its aim is to produce wonder and delight rather than to be useful.

Do engineers need to do research? Well, if they do not know the "state of the art," they are open to the charge of trying to "reinvent the wheel." (For "wheel" you can substitute "transistor," "laser," "digital computer," or any of the myriad of marvelous inventions we enjoy.) Some amount of research is necessary to keep abreast of the state of the art. Engineers occasionally need information in areas that scientists have not yet probed and therefore feel impelled to do research that might better have been done by others. This could be called "gap-plugging." The danger for engineers, as for practitioners in any "applied" field, is that they can become too enamored of research for its own sake. A good engineer needs the fruits of scientific research, but he is not a scientific researcher. Like the physician or the clinical psychologist, he must strike a balance between knowing and doing—or, as we say, "practicing." The same point can be made about practitioners in fields in which the body of relevant knowledge is not exclusively scientific: law, politics, administration, journalism, teaching, or diplomacy, for example. Seeking a workable balance between knowing and doing—first in engineering and later, during my years as a university president, in other applied fields—has been a continuing quest for me.

Is engineering a profession? The answer is certainly affirmative if professionalism refers to accepted standards of responsibility and competence. If you would not want a dilettantish amateur—or a self-anointed expert—to operate on your brain, defend you in court, plan your local medical center, or produce your play, why would you want such a person to create your highways, bridges, power plants, factories, and farm machinery? Prescribing and enforcing standards is, of course, a more complex issue than recognizing the desirability of such standards. The one essential ingredient of professionalism, as I have argued all my life, is putting the good of humanity above one's own selfish needs. A teacher or physician under my code would never go on strike, because to do so would be to place his or her welfare above that of his students or patients. The amount of formal education that should be required for entry into any profession—and specifically into engineering—as well as the validity of formal examinations as measures of professionalism are questions I have pondered throughout my career.

I went straight through to my doctorate at Harvard between 1928 and 1935. (The university chose to give me the doctor of science degree even though the topic of my thesis was in electrical engineering.) My motivation, along with interest in my subject, was the scarcity of engineering jobs in those Depression years. While a student at Harvard, I worked on engineering projects for professors with contacts in industry, in addition to holding various subsistence jobs. After graduation I became assistant professor of electrical engineering at Tufts, spending my spare time and summers working for an electrical engineering company concerned with high-voltage power lines. Then I was appointed head of the electrical engineering department at the University of Connecticut, where I continued part-time industrial work. By choice and by necessity, I was never a purely academic engineer, though I always had one foot in the academy.

With my background, I found my way into national defense work during World War II, since our government's policy in that global emergency was to combine scientifically based research findings with hands-on engineering. Among the projects in which I was involved were the development of sonar, the undersea counterpart of radar, and a homing torpedo. I went to Penn State in 1944 as head of the department of electrical engineering and director of the Navy-sponsored Ordnance Research Laboratory. (The ORL grew out of the wartime Underwater Sound Laboratory at Harvard, where I had served.) My way of engineering evidently suited a rapidly growing land-grant university such as Penn State, since I was named dean of engineering in 1950 (after a year in the Pentagon during the Korean War) and president in 1956. After my retirement from Penn State in 1970, I spent five years as vice-president of science and technology for the Aluminum Company of America.

I am gratified that my way of engineering has contributed in some measure to a higher standard of living for my fellow human beings. My work on national defense is also a source of gratification, since that work is partial payment on my debt to the country that adopted me at the age of thirteen, as well as to my native England. My way of teaching engineering seems to have merit, since it has had some impact on Penn State's curriculums in engineering and engineering technology. These curriculums rate highly for the quality and number of engineers and engineering technicians they graduate. Finally, I am proud that Penn State has seen fit to name the environmental sciences building after me, since my way of engineering has always included concern about the environment.

Now It's My Turn: Engineering My Way

On commencement day in 1932 I had no doubt that I was being awarded a degree by the leading university of the nation, if not the world. My confidence in Harvard's preeminence did not, however, erase two lingering doubts from my mind. One doubt was whether Harvard, despite my creditable record there, had been the ideal college for me. Another doubt was whether the arrogance paraded by many Harvard professors—but by no means all of them—was justified. In short, I retained the outlook of an immigrant boy from York County, Pennsylvania.

The keenness of the competition at Harvard and the rapid-fire method of instruction kept me constantly on my toes during my four college years. Was that a healthy intellectual posture for my late adolescence? Was I stimulated in a salubrious way by knowing that I could rarely outdo my classmates? Was my learning facilitated by my feeling that I never quite understood what was going on until months after a course had ended? These are questions that the educational psychologists have not fully answered. They are also, to a degree, questions of value for consideration by philosophers. The issue in its extreme form has been posed in the query "Is it better to be a happy pig or an unhappy Socrates?" At the age of twenty-two, I felt—as I still feel at seventy-seven—that there are various possible and desirable positions between swinishness and Socratic excellence.

Excellence may seem like arrogance at times, but the two qualities have always struck me as unrelated and usually conflicting. To be sure, when anyone is disabused of some cherished error, he is likely to perceive his donor of enlightenment as arrogant, at least at first. Fairness, however, usually leads us to forgive our real mentors, even the toughest ones, like Dean Clifford in my case. We can and should, I believe, forgive excellent teachers or supervisors for lack of sympathy with our slow wittedness and for inattention to our human needs up to a point. But I see no reason for a teacher to rush us through complex material or to gloss over intractable points. Blatant discourtesy is also unacceptable in my book—as demonstrated, for example, by the instructor who extended a lecture beyond the appointed hour when I had told him of my desire to catch a train home for Christmas. Outright rudeness is out of character with true greatness.

An arrogant pose may at times be a useful teaching strategem. On occasion a teacher may feel a need to humble a student confronting some vast field of knowledge. But this approach is the antithesis of the Socratic method, where the student *seems* to be finding answers autonomously.

The Socratic method even allows for the off chance that a student really will find a valid new answer, and it gets the student into the habit of posing questions and seeking answers. The Harvard approach, as I experienced it, was characterized by too much arrogance, actual or feigned. It was authoritarian. By anticipating my questions and pronouncing categorical answers, my Harvard teachers killed my initiative as a learner until I had recovered from the "undergraduate daze." These Cambridge mandarins also misled me about the evolution of scientific and engineering knowledge. My professors astounded me by leading me through the thicket of potential error to what they called, if not Truth, at least The Correct Solutions. Not until many years after my college days did I learn the reality about scientific laws and engineering theorems: That they are not simply *revealed* to an elect with the brainpower to comprehend them, but that they have been *developed* by an ongoing process of hypothesis-testing through experimentation. Scientific and engineering research are thus open to all conscientious and ingenious investigators, including non-Harvard men like Franklin and Edison. Notwithstanding Harvard's one-word motto, "*Veritas*," I found that my alma mater has no inside track to Truth.

FIRST TEACHING POSTS —
Tufts College and University of Connecticut 1934–41

My teaching career began unexpectedly and pleasantly after I got my master's degree in 1934. When Professor Dawes summoned me one day at Harvard, I assumed his purpose was to discuss my thesis research. Instead, he told me that he had received a call from the dean of engineering at Tufts College, four miles away, who said he needed someone to teach a course in electrical engineering. Dawes said he had recommended "The Commuter," Jessel, and me. I was not concerned about the first, who was a hack and a copycat, but Jessel was a good student and seemed sure to get his doctorate within a few months. I expected that Jessel would get the job. However, I was asked to come over and talk to the people at Tufts. I saw the dean, who took me to see the head of the math department, explaining that their anticipated increased enrollment in electrical engineering had not happened but that they might need someone to teach a course in freshman math.

In the math department offices I met a strange and wonderful character, R. B. "Billy" Ransom, a bewhiskered gentleman who looked every inch a college professor. He explained to me that he did not teach mathematics in the usual way. He had his own textbooks, his own plan of student development for which he claimed uniqueness, and his own way of teaching. He urged me to take a copy of his freshman text, study it, and stand by; he might have to call me on very short notice. Since there were a couple of months before the next semester, I did as I was told and waited. Apparently because my record showed more interest in math than did Jessel's, I was chosen as the likely candidate to be added to the staff of either the math department or the electrical engineering department of Tufts College if anyone were needed. Finally, a call came. I went again to the dean's office and was presented with a contract stating that for $300.00 I was to teach a three-hour, one-semester course in mathematics at the discretion of the head of the department. Again I trotted over to see Billy Ransome. This time he never mentioned freshman math; he merely said, "On Monday I want you to teach a course called 'Higher Mathematics for Engineers and Physicists.' The text will be

Sokolnikoff; your students are all seniors. Here is a copy of the text. I wish you luck."

Thus began my teaching career. It was a close race. On the first day, I was only two days ahead of the students, and my lead never increased until the final examination. Ransome never visited my class, never questioned how much work I was covering. My only gauge of progress was how much homework the seniors would do without complaining. My practice sessions with my Harvard peers in 207 Pierce Hall stood me in good stead, and with a friendly class of budding engineers and scientists, I learned as much or more than they did. We had a good time, and I would have liked to present seventeen A's, but I did not dare. The next term I taught three courses: one in freshman math, one in senior math, and one in electrical engineering; and the term after that I had five different courses. My new contract paid much less than I expected. Five courses was a full load, which made me a full-time instructor, though not at five times \$300.000 for each semester but at \$1800.00 for the full year. This was a disappointment, but it was a regular job at last! With my appointment at Tufts, I quit the Georgian quietly—without fanfare and with no broken dishes—but not without some regrets after a six-year association.

Harvard threw me a small curve in connection with my first listing in a Tufts catalog. In April 1935 the Tufts dean asked me about my doctorate. Since I felt confident I had met all the requirements and since Dean Clifford had addressed me as "Doctor" at the end of my dissertation defense, I said I expected my Sc.D. at the June commencement. When my Tufts dean checked with Harvard, the official designated to answer his query would not confirm my standing. Upon my learning of this rebuff, I became apprehensive; despite my rather successful career at Harvard, was this Jovian institution going to strike me down? Finally in May, I received a letter from Harvard alerting me that my doctorate would be conferred the following month. Thus I missed the deadline for getting Sc.D. printed after my name in the 1935–36 Tufts catalog, and I endured several weeks of mild but unnecessary apprehension.

After becoming assistant professor of electrical engineering at Tufts in 1935, I was busier than I can now imagine without effort. In addition to teaching five full courses, I set up a lab in the new Cousins building. With the support of Professor Dawes, my friend and Harvard advisor, I had organized a student chapter of AIEE at Tufts. Naturally, I became the chapter's faculty advisor and thus a member of the northeast section's committee on student branches, a committee that I chaired a few years later.

In 1934, soon after my arrival at Tufts, a friend asked me whether I was going to vote for Mr. Curley for governor. I replied that I could not vote at all, not being a citizen. When he asked me why, I told him the story of my informal arrival in the United States at the age of thirteen. Legally, I explained, I had never entered this country. My friend asked me if I had thought of leaving and reentering properly. Yes, I said, but I did not want to risk being kept out. My friend asked for permission to look into my status at the federal court in Boston. I gave him my okay, seeing no danger. About two years later, I was summoned to appear in court—actually, in a district judge's chambers. When the judge asked me why I had not applied for naturalization earlier, I explained that I lacked proof of having entered the country. The judge then told me that I had been under a misapprehension, inasmuch as Congress had passed a law waiving proof for persons who had entered the United States before 1924. He then asked me a few questions about our political system, requested that I raise my right hand in an oath of allegiance, and told me I was now a citizen. At times, when I look back at my experiences in becoming an American citizen—from my stroll across the border with my dad in 1923 to my interview with the judge in 1935—it all seems like a dream. This dreamlike quality is most overwhelming when I am consulted by an agency of our government, including the Congress of the United States. Here am I, a poor immigrant boy, advising senators and representatives about some phase of running the country. The wonder of it is that any citizen with any kind of know-how may be asked for help in running the country. This sense of being part of it all is why every citizen should say "I am proud to be an American."

At Tufts I inherited a high-voltage laboratory similar to the one I had inhabited at Harvard. How Tufts had acquired such a lab remains a mystery to me. No one on the staff used it, and there were no courses on high-voltage engineering. The lab was rented during mornings to Doble Engineering Company, whose offices occupied the second floor of the same building. The president and chief engineer of the company, Frank C. Doble, interviewed me soon after my arrival at Tufts and offered me a job at two dollars an hour for all the time I could spare from my teaching duties during the academic year, plus summers "if things work out all right." Things worked out right enough so that I worked for Mr.

Doble—nobody called him Frank—for three years. I learned a lot from the experience, often the hard way.

Having left Tufts without a degree, Mr. Doble was obsessed with proving his superiority to persons possessing degrees, especially doctorates. Fortunately, I could usually avoid him because he rarely came to work before two in the afternoon and I was able to dodge most of his late-evening conferences. While the rest of his staff had to pass his open office door in order to escape at five o'clock, I could duck out of my professorial office on the ground floor without being seen. Mr. Doble arrived at his Tufts office in a chauffeured Cadillac, always wearing white gloves, a three-piece suit, and gray spats, even in July. His late arrivals may have been caused by his sickly wife, who reputedly ruled the Doble roost.

Regardless of Mr. Doble's marital and managerial shortcomings, he was an astute entrepreneur. Somehow he had allied himself with a Boston Brahmin who was a practical self-trained engineer. This engineer had invented a gadget for testing high-voltage string insulators, the ceramic disks by which high-voltage wires hang from the cross arms of transmission towers. If an insulating disk fails, it should be replaced before others fail. The Doble Company's testing device was simply a two-tine fork at the end of a hollow tube. When the fork was placed across an insulator while the line was energized, a spark would pass between the tines, making a buzzing noise audible through earphones at the end of the tube. The Brahmin engineer had developed considerable agility in climbing transmission towers to validate the accuracy of his testing device. Mr. Doble had sold this device to electric power companies throughout the country, despite his abrasive and confrontational style of salesmanship. Typically arriving more than an hour late, he would keep on his hat and overcoat if he found his customer's office chilly. When he was invited for lunch, he would wipe off the silverware and return food for reheating. His sales pitch was blunt: "What excuse would you give if your whole system suddenly shut down?" or "How would you respond if I said you won't invest a few dollars in your employees' safety?" No wonder engineers tried to keep Mr. Doble away from their bosses.

Since the power companies were eager to get testing equipment to help them spot weak bushings, switches, or transformers before they failed, my main duty was product development. I helped develop a testing device for transformers, which the company patented. Among my related duties were writing instructions for Doble handbooks, speaking at customer seminars, and sometimes accompanying our president on sales trips. On one occasion we were visiting an electric company where the chief engineer really needed our help with a problem on which I had been working for a month. Mr. Doble presented "*Doctor* Walker's solution" in such an obnoxious way that the chief engineer would not listen and threatened to have us thrown out of his office.

The Doble Company's policy was to lease, rather than sell, its equipment. The utilities did not object to this policy with the insulator tester, since Doble supplied skilled manpower with every equipment lease and the customers had no desire to train their own operators. But the transformer tester was different, since it had several potential uses by employees of the utilities, which therefore wanted to buy the equipment outright. When one of our competitors began selling a transformer tester resembling ours, Doble filed suit for patent infringement. Our competitor soon realized that its case was hopeless and settled out of court. The settlement consisted of a cease-and-desist pledge plus a million dollars. For awhile Mr. Doble carried the competitor's check in his pocket so that he could wave it in front of resistant customers.

The Doble Company still prospers. When it reached the height of success, it made a handsome gift to Tufts College, now University. Tufts, in turn, bestowed an honorary doctor's degree on Frank C. Doble. With his lean, ascetic figure, his sharp features, and his abrasive manner, Mr. Doble gave the impression that he trusted nobody. Yet, after he died, I learned that he had bequeathed his company—lock, stock, and barrel—to his employees.

A great and joyous change took place in my life in 1937, with two of my Tufts colleagues as catalysts. Mabel and John Barnes both taught in the math department, having taken their doctorates at Ohio State and Princeton and having met at a Princeton seminar at which Mabel was the only female participant. One evening in that memorable spring the Barnses gave a dinner party for Mabel's sister, who was visiting from Chicago. As an "eligible" young man, I was invited to round out the foursome. As a Harvard graduate, I was assumed to have a sophisticated knowledge of cocktails and was put in charge of mixing. Actually, being a product of a Methodist upbringing and Prohibition, I was as innocent about cocktails as my host and hostess. While we were awaiting the ladies, John furnished me with a bottle of rum accompanied by a recipe for pink daiquiris, as well as the lemon juice and grenadine syrup to be added. Before the ladies appeared and while my host was occupied with some household chore, I consumed two generous samples of my experimental mixture. By this time the world, like the pink daiquiris, was beginning to look rosy, warm, and beautiful.

Suddenly, at the top of the stairs, appeared a vision in black and red. Before Josephine had reached the bottom step, this sophisticated young Harvard man had made a mental resolution: "This is the girl I'm going to marry." A family joke holds that the pink daiquiris were a trap, but I know they only hastened an inevitable process. Josephine and I were married the following December and have lived happily ever afterward. We have a daughter, Gail, and a son, Brian. Both are sources of pride and joy, as are their respective daughters, Josephine and Laura.

Before the end of our first evening together I learned that Josephine Schmeiser was a dress designer for Carson, Pirie, and Scott in Chicago, after having studied liberal arts at Cornell College and textiles at Iowa State. She had grown up with three sisters and two brothers on a farm near the confluence of the Iowa and Mississippi rivers. Her ancestors had immigrated from Germany to the Saint Louis area in the mid-nineteenth century, mostly as Lutheran missionaries. One ancestor, Jacob Schmeiser, founded Eden Seminary. Attending college was a family tradition. When I eventually visited the Schmeiser homestead, I found it to be the ideal American family farm: a spacious white clapboard house surrounded by outbuildings, an orchard, vegetable and flower gardens, and fields and pastures.

Josephine and I discovered that our backgrounds were almost identical in essential ways: loving but strict parents, adequate but lean economic circumstances, and years of hard work to gain our educations. We laughed when we compared notes about eking out our rations before paydays—she on Fig Newtons and I on three-day-old cakes. We took only a brief honeymoon during the Christmas break, resolving to invest all of our worldly wealth in a Grand Tour of Europe the following summer. We spent fourteen hundred dollars covering fourteen hundred miles, mostly on bicycles, from Plymouth all the way around England, visiting my birthplace, then east to Prague and north to Oslo. Our only disagreeable experience was an encounter with Hitler's troops—this was 1938, when the Nazis annexed Austria and western Czechoslovakia—an experience that impelled us to cut short our visit to Germany and to make a dash to Denmark. We returned to New York in steerage class with ten dollars between us. Since a fellow passenger coveted our well-worn English bikes, we sold him the pair of them for $25.00 and travelled to oston in a Pullman chair car instead of by the three-dollar overnight oat. At our modest flat in Medford we were well supplied with crystal om Vienna, china from Prague, beer steins from everywhere, and a fetime's store of memories.

While teaching at Tufts, I became acquainted with one of that colege's most distinguished alumni, Vannevar Bush. Van had taught elecical engineering and mathematics at Tufts from 1914 to 1917 before oving to M.I.T., and his sister was dean of women at Tufts during my ears there. Van had been a founder of the American Radio Corporation, hose former plant adjacent to the Tufts campus had been taken over by e college. Part of that plant was being converted into an electrical ngineering laboratory, and I was involved in the conversion. My first ance meetings with Van Bush gave me no hint of my future deep volvement with him in connection with national defense and research olicy.

In 1938, after I had become head of the electrical engineering deartment at Tufts, I began my acquaintance with another engineer who as to be important in my life: Harry Hammond. As dean of engineering Penn State, Hammond was on an exploratory trip to other campuses. e later told me that he had been favorably impressed by my department. wo years later, in 1940, I attended an ASEE meeting at Penn State. I as at that time making my second visit to Pennsylvania's land-grant niversity, my first visit having been made a dozen years earlier when was a high-school senior. In 1928, after the Reverend J.T. Willoughby ad convinced me I should go to college, Penn State had offered me a 100.00 scholarship. Although I had opted for the more generous scholship awarded by the Harvard Club of Philadelphia, I had friendly feelgs toward the Pennsylvania State College, as it was then called. There as some talk of my joining the Penn State faculty in the 1938–40 period, it I was committed to become head of the electrical engineering deartment at the University of Connecticut in the fall of 1939.

That summer, before moving to Storrs, I served as a temporary ember of the transformer engineering design department of General lectric in Pittsfield, Massachusetts. My assignment was to design disbution transformers below 23 KVA and under 12 KV—a design clasfication in GE's transformer repertoire. During my first week I was ven some books to study on transformer design in general and on GE sign in particular. During my second week I was instructed on "how do it here," to wit: When an order arrived, it was examined to see whether GE had ever made a transformer to the same or similar specifications. If it had, the situation called for no new design. If it hadn't, we were to make modifications as possible—retaining, for example, the same laminations or the same tank size. Only if characteristics would not be modified would a new design be started from scratch.

Every Monday morning each engineer was given a stack of problems, each one marked in the upper right-hand corner with the number of points it carried. When an engineer completed his quota of points before noon on Saturday, he could use this "bonus time" for experimentation and research, study and writing, or merely reading the *Saturday Evening Post*. The superintendent's assistant kindly gave me problems that carried high points without consuming excessive time. Thus I had spare hours to delve into questions concerning the shielding of transformers from high-voltage surges, an area of research suggested by the helpful assistant.

The engineering office occupied about an acre above the manufacturing plant and was inhabited by a hundred engineers, designers, and draftsmen in an open "bullpen." Along the outside wall, with windows to the outer world, were offices for the department heads. The three best offices were those of the superintendent and his assistant and one simply labeled "Special Problems." The superintendent rarely conferred with any other member of the staff, and he was mildly disliked, disdained, and completely ignored. I gathered that he was judged out of touch with modern transformer design. His assistant, who ran the department, was knowledgeable, helpful, pleasant, and considerate. As for the occupant of the Special Problems office, lo and behold it was Charlie Wells, whom I had not seen since he'd left our 207 Pierce Hall study group four years earlier. This was the job for which Charlie had dropped his doctoral work.

My exclassmate certainly acted as if he owned the place. Charlie spent much of this time walking around, swapping jokes not only with engineers and designers but also with department chiefs and secretaries. He seemed to barge into the superintendent's officer whenever he wanted, without warning. A typical comment about Charlie was "If you get into a tight squeeze or run into a roadblock, go see Charlie. He knows all the answers." Obviously, no one ever thought of going to the boss, about whom Charlie said in his booming voice, "Oh, the super—he's perfectly harmless. If you let him alone, he'll let you alone."

Clearly, my happy-go-lucky friend had no respect for his boss. And just as clearly, though he was forced to respect Charlie, the superintendent disliked him heartily. Realizing his own indispensability, Charlie got the maximum amount of enjoyment out of a rather grim environment. For one thing, he kept his own hours, always giving GE its due but often coming late and staying late. One Saturday when the super was having a particularly stressful morning, Charlie bounced in about an hour late, took off his straw hat, and spun it across the room toward a clothes tree. Charlie was always somewhat of a dandy, dressed in the college uniform of blue blazer, gray flannels, and button-down shirt. "Hi, you guys," Charlie greeted us as he watched his hat land successfully on the clothes tree. As this was happening, the super came out of his office and, seeing my friend's display, felt constrained to do something. "Wells, you're late!" he roared across the room.

"Yes, I am," replied Charlie, showing no remorse.

"Where have you been?" shrieked the supper.

"I have been to get my hair cut," replied Charlie.

"You cannot get your hair cut on company time," said the super.

"But it grew on company time," said Charlie.

"Not all of it," said the super.

"I did not get all of it cut off," said Charlie, as the super speechlessly slammed the door.

When I started teaching at the University of Connecticut in the fall of 1939, I found that it had no student branch of the AIEE. Organizing one put me in solidly with colleagues at nearby campuses such as Conrad of Yale and Phelps of Rhode Island. As early as the fall of 1939, President Roosevelt was stepping up defense production and research. After the fall of France in June 1940, the defense buildup was accelerated. Then, in January 1941, Congress gave FDR support for his claim that the United States was the "arsenal of democracy." Vannevar Bush and other acquaintances of mine were leaders in the mobilization of scientific and engineering talent. When an Underwater Research Laboratory was organized at Harvard, I accepted an invitation to join up. World War II got me into two new fields to supplement electrical engineering and educational administration: acoustics and research management.

I learned a great deal about pedagogy, electrical engineering, and educational administration during my first seven years as a college teacher. My students taught me a lot, my colleagues taught me quite a bit, and my superiors taught me a little—the natural order of things. Above all, I learned that a teacher can make heavy demands on students, provided these demands seem reasonable and provide the teacher seems to be making equivalent demands on himself (or herself). I also continued to learn about politics—academic, organizational, and corporate—and to see how all politics, from the family or neighborhood level to the level of a nation or a global alliance, is of a piece. As Aristotle observed, "It is politics that determines what other sciences should be studied in states; which of them should be learned by each group of citizens; and to what extent they should be learned."

How much basic (or "pure") science and mathematics should be learned by engineers is clearly, according to Aristotle's dictum, a political question. My two previous chapters alluded to Harvard's unsatisfactory resolution of this question by ducking it. At Tufts College and the University of Connecticut, I began a lifelong quest for a justifiable balance of theory and practice in engineering.

In my work for little Doble Engineering Company while I was at Tufts, I was permitted to do some "basic" research in seeking to develop new testing devices. The entrepreneurial Mr. Doble—in spite of his Napoleonic arrogance, or maybe because of it—gave me and my colleagues some scope of exploration. Mr. Doble was shrewd enough to realize that the success of his firm depended on new approaches to newly felt needs. The mission of the Doble enterprise was to spot potential "accidents" before they happened. Accomplishing this mission required distinguishing between controllable and uncontrollable factors. Theoretical analysis facilitated such distinctions, as did "hunches" based on experience.

By contrast, the General Electric transformer division, when I worked there in the summer of 1939, discouraged basic or exploratory research. I found that the typical way for a GE engineer to "design" a transformer was to look through a small shelf full of books for an existing transformer with specifications approximating those on a given requisition. If the one in the book had a secondary voltage of 2500 and the engineer wanted 2450, he would simply calculate the appropriate number of wire turns to be removed. Thirty years later, when I worked for IBM, one of the world's most advanced companies in many ways, I found that much of their engineering was done the same way. I refer to the IBM division that produced new models, but IBM also encouraged their engineers to take off after wild ideas, which sometimes produced great strides forward.

Now, I recognize that no enterprise can be constantly "reinventing the wheel." A certain amount of adaptation of existing tried-and-true models is mandated by economic considerations. On the other hand, sticking to the same models can be risky, as the automobile industry has learned. Of course, product development is not all done through mathematical and scientific analysis. Experience and intuition, applied in trial-and-error methods, often produce engineering advances and, for that matter, scientific discoveries. The amount of an enterprise's resources that should be allocated to research and development, as well as the division between pure and applied research, are political questions. I mean they are questions of policy, reflecting the enterprise's conception of its purposes, limits, and goals.

NATIONAL DEFENSE RESEARCH
World War II to Cold War

Shortly after the Japanese attacked Pearl Harbor, I took a leave of absence from the University of Connecticut and returned to Harvard for duty with the Underwater Sound Laboratory. This lab later had two divisions. The one working on sonar was headed by Paul Boner and reporting through the OSRD to the Navy Department's Bureau of Ships. The other, headed by me, worked on torpedoes and reported to the Bureau of Ordnance. Outfits like the Underwater Sound Laboratory resulted from two steps President Roosevelt had taken on converting a somewhat isolationist nation into the "arsenal of democracy." The first step was to create a National Defense Research Committee (NDRC), a "brainstorming" operation with subcommittees on all facets of war-making from aeronautics to submarine detection, from electronics to petroleum. There was even a committee on radioactivity, inspired by a letter that Albert Einstein had sent to the president. The second step was to give this network of committees more clout by making it an *office*—the Office of Scientific Research and Development (OSRD). A fuller account of national research mobilization is given in my profile of Vannevar Bush, one of its architects.

Converting the military to acceptance of scientific and engineering research took more than the creation of OSRD by the commander-in-chief. Successful ventures were needed to convert the generals and admirals. Such successes started quickly and kept coming at a dizzying pace: radar, sonar, the proximity fuse, the homing torpedo, and finally the atomic bomb. Today many scientists and engineers are concerned about having overly close ties with the military, and even President Eisenhower warned against the potential dangers in a "military-industrial complex." The nation has undergone a sea-change since the early days of World War II.

One of the crucial hurry-up projects of World War II was the development of an effective antisubmarine weapon. During the time of the Battle of Britain, the Axis—especially Nazi U-boats—began sinking Allied ships faster than we could build them. The Battle of the Atlantic was being lost. With it, the war was being lost, since the Allies could not hope to win if we could not move our men and supplies to places where they were needed. The project began as a committee activity even before OSRD was created. For the sake of secrecy, it had a code name—a less glamorous one than the Manhattan Project, namely FIDO.

Project FIDO was greeted with skepticism by the Navy. The Navy was having so many difficulties with its standard torpedoes that the idea of an acoustic homing torpedo seemed crazy. Standard torpedoes frequently ran too deep and passed under their targets. Actually, the fault was not with the torpedoes but with their calibration. Problems were caused by the ineptness of the people who tested the weapons, took them apart, and then reassembled them. Being unable to overcome these problems, naval officers could not believe that a group of long-haired scientists and crack-brained engineers could devise a torpedo that would listen for a submarine, pursue it, and destroy it.

The standard method for destroying an enemy submarine was first to detect it and then to attack it with depth charges. This method sounds very simple, but a small percentage of submarines was never detected because of several factors: first, the sonar gear that was supposed to find them under the water had a very limited range; it was very usually difficult to see the submarine's periscope from ship or plane, since very little of it had to be exposed in order for the sub to aim its torpedoes; and the depth charges had a very limited range. At first, a 300-pound charge of TNT was standard, so an antisubmarine vessel would cross over the path of a submarine and drop charges, hoping they would come close enough to do some damage. "Close enough" usually meant within a hundred feet of the submarine. But since very few submarines were ever detected, since few of those detected were ever attacked, and since still fewer of those attacked were driven to the surface, antisubmarine warfare was a losing battle. In desperation, the Allies used more antisubmarine vessels, and depth charges were made considerably larger, with little or no effect.

It had often been suggested that the way to catch a submarine was to combine the detection and destruction functions in a single weapon, which should obviously be a torpedo that would seek the target and then blow up on contact. This idea is very old, and indeed there are a number of patents describing homing torpedoes. But none of these schemes had ever been reduced to practice. Indeed, as far as is known, no model had ever been built to test such a torpedo.

Late in 1941, after America had officially entered the war, a number of scientists and engineers took up the problem seriously, and NDRC's Project 61 was formulated. This project called for the development of an antisubmarine weapon to seek out a target under its own power. Four different teams representing four different organizations were put on the job. The organizations were General Electric Company, Bell Telephone Laboratories, Western Electric Company, and Harvard University. The teams were given almost complete freedom of action and were told to solve the problem as best they could. The Navy offered its services in obtaining information and providing material assistance. The job had to be hurried, for almost daily the team members were told that so many more ships had been sunk with cargoes valued at so many million dollars, not to mention the lives lost. There was no attempt to carry out the research in the way usually favored by scientists—namely, to change one thing at a time and measure its effect. The method was to build a torpedo containing a combination of devices designed in accordance with the best knowledge available and then hope and pray that it would work. First came the problem of making the torpedo steer properly; then the problem of making it stay under water; then the problem of finding enough space to put in explosive and an exploder. Next came the problem of trying to make the torpedo seek out a target and then make it steer toward the target and not miss it because of interfering noises in the sea. Last came the tremendous task of trying to build electrical circuits many times more complicated than those of the average radio and have them still operate after being thrown out of an airplane traveling at the speed of over 200 knots. (It had been decided that an air-to-sea torpedo would be more effective than one launched from a destroyer, because a destroyer is a noisy vessel, which a submarine can detect from miles away and thus avoid contact with. While it was thus more feasible to drop a torpedo from an aircraft, the difficulty was that the aircraft must in some way find the submarine.)

For a whole year our group worked, the group expanding from the original ten in December 1941 to over one hundred a year later. On December 7, 1942, exactly one year after Pearl Harbor, the first weapon was dropped from an airplane and made a successful pursuit of a target. Even this attempt almost met disaster. For several months our group had tried to develop a device that would withstand an air launching. Carefully we dropped each component from an airplane to test its shock-resisting ability, but when we put all the components together, something always failed. Their factors of safety had been shaved too thin. The weather had been continually getting worse at the test stations where our tests were being conducted. Fewer and fewer days were suitable for going out and from ship to ship, lie out in the hot sun or in the cold and ice, and be dumped into the ocean at airplane speed. That these difficulties were ever surmounted using old-fashioned vacuum tubes and mechanical relays is a tribute to the intelligence and care of Navy people in planning and using the weapon.

A combat test of FIDO was not long in coming. Instead of using some 700 pounds of explosive, like a depth charge, this weapon used a much smaller quantity, but it was supposed to be lethal because the explosion would take place very close to the hull of the enemy submarine. The British Navy thought FIDO would not work. Many members of our own Navy were skeptical, although they thought it was worth giving a try. Planes carrying FIDOs soon reported successes. One aviator, after using the device, reported that the enemy submarine had upended before sinking, so that he had a chance to observe the condition of its after end. His report was that the whole after compartment was peeled back as the muzzle of a spiked gun might be. There was such a big hole, he reported, that he could look into the after compartment and see the sub's torpedoes still lying unused on the racks as she went down. Since the test of destruction of any submarine is the penetration of the pressure hull, there was no doubt that this submarine had sunk and that the weapon, if properly used, was a successful one. The reporting pilot did not get credit for a "kill" because he had no proof. His comment: "They wanted me to bring back the skipper's cap."

Politics played an ever-expanding part in my life during World War II; I do not mean party politics but organization politics. Of course, when the organization is a governmental one, party politics may cast a shadow on things. My conviction was confirmed that political activity is not intrinsically dirtier than any other human activity, though it is often a great deal sweatier.

Committee work for the American Institute of Electrical Engineers was one of my involvements, starting in my undergraduate years. As a junior, I was president of the Harvard chapter of AIEE. While I taught at Tufts, I became chairman of the Institute's committee on student branches for the northeast. Since I am a strong believer in the sharing of information among persons pursuing the same vocation—the guild tradition, one might call it—I remained active in the AIEE after it had merged with the Institute of Radio Engineers (IRE) to become the Institute of Electrical and Electronic Engineers (IEEE). My interest in AIEE and later IEEE waned, however, when I saw members' political orientation becoming nontechnical and preoccupied with numbers of student chapters like a college fraternity. The first group with a technical mission that I was asked to join was the National Research Council's subcommittee on insulation, the subject of my doctoral dissertation. I do not recall ever meeting with that subcommittee, from which I resigned thirty years later after making the surprising discovery that I was still listed as a member. I joined the Acoustical Society of America during my wartime work involving sonar, and I served for a year on its membership committee but drifted away from this estimable outfit because I am not a true acoustician.

After I became chairman of my department at Tufts and made the acquaintance of Dean Harry Hammond of Penn State, I joined the American Society for Engineering Education (ASEE), which had recently emerged from the Society for the Promotion of Engineering Education (*not* "Prevention," as some irreverent wags would have it). Just before World War II, I was persuaded by Dean Hammond to serve on an ASEE committee on the future of engineering education. This committee's report was expected to produce the same tidal wave of change as the 1910 Flexner report on medical education. Its impact was smothered by the war. When the project was revived after V-J Day, I got involved again as a member of the Penn State faculty.

Long before the war ended, Harvard and a number of other universities had decided to stop doing national defense work after the cessation of hostilities. The chief stated ground for this decision was that the results of such work are "classified"—that is, not universally shared—and therefore violate the proper tradition of science and technology. A second and related ground was the feeling that military activities are incompatible with the proper humanistic concerns of a university. These issues "hit me where I live," in the parlance of the sixties, and will be a continuing theme from here to the end of my book.

The issue of incompatibility strikes me as easier to resolve, in principle if not in practice. A university, especially a publicly supported one, should in my view serve all legitimate interests of its larger society. The governance of the university should assure a balance of these interests, without encroachment of any one upon the others. Religious studies, for example, may properly focus on particular creeds, provided no orthodoxy is sponsored by the university. Business studies can and should coexist with investigations of socialism and other alternatives to capitalism. I see no reason why weapons research and officers' training preclude considfrom luxurious to spartan. Gradually, other groups trespassed on the committee's territory, while big-name members defected. Finally an assistant secretary of the Navy for research and development let the committee members' terms expire without informing the chairman–me—of his intentions. Since the committee was mandated by Congress, new members were appointed, but everyone knew these appointments were meaningless.

When President Truman created a combined Department of Defense in 1947, under James V. Forrestal, former Navy secretary, prospects improved for the utilization of outside technical advice by the military. Secretary Forrestal created a Research and Development Board chaired by Vannevar Bush. In 1950, during the Korean War, I would become this board's executive secretary. The story of the board is told in my profile of Van Bush and in the chapter on my first Penn State years. Here I simply want to make the point that the R&D board of the Department of Defense suffered from the same inadequate political spadework as did the Naval Research Advisory Committee. That is why the board never achieved the coordination of Army, Navy, and Air Force R&D that the tragic Secretary Forrestal dreamed of. Political effectiveness requires either influence (the way of diplomacy) or power (the way of government). But neither influence nor power can flourish without proper tillage—assuming, of course, hospitable soil.

Administration takes place within a given political environment. The administrator as such has no control over the political constraints within which he must work, although obviously he may also play the role of politician. Administrative problems—planning, organizing, establishing priorities, staffing, scheduling, expediting—are different from political problems but just as challenging.

As assistant director of the Harvard Underwater Sound laboratory in charge of the torpedo division, I learned much about the administration of a research laboratory. Since in 1942 I found no books on the subject, I, like others, learned by trial and error. Decisions had to be made in a hurry. Goals were often changed, so projects were suddenly canceled and people moved. Often egos were bruised, though most staff members recognized wartime pressures and overlooked or forgave administrative errors. It was a great time to learn.

Although we normally worked ten to twelve hours a day, seven days a week, while the war was being fought, occasionally a few of the administrators—Hunt, Boner, Hickman, Houston, and I—would have dinner together to compare notes and commiserate. We agreed that collectively we had probably made every possible administrative mistake and that we should find some way to share our experience with others. I found a way in 1947, after I became director of the Ordnance Research Laboratory at Penn State. My way was to organize a conference on the administration of research under the laboratory's sponsorship. I put together an invitation list equally divided among research directors in the armed forces, industry, and academic institutions. The conference was such a success that it has continued to be held annually at various centers for forty years. One of the side effects of this conference was the stimulation of sharing, not only of administrative know-how but also of substantive information. Since conference participants came from diverse industries—Kodak and Union Carbide, GE and Westinghouse, United Aircraft and General Dynamics—they saw that researchers in acoustics can learn from those in electronics, those in hydrodynamics can learn from electric field theory, and so on indefinitely.

I went to Penn State from Harvard along with the torpedo division of the Underwater Sound Laboratory, renamed the Ordnance Research Laboratory. This move resulted from a concatenation of my friendships with Penn State's engineering dean and some Navy R&D officers, especially Captains Bennett and Livdahl. I had made a commitment to Dean Harry Hammond to head the electrical engineering department in his school when I completed my war work. Meanwhile, my Navy friends had been notified of Harvard's intention to cease doing defense research at the war's end. The Navy was considering several new locations for its torpedo research, including Penn State. Late in 1944, my Navy friends approached me about putting together a "small group" to continue some of this work. The captains and I were thinking of perhaps eight persons had a budget of $120,000.00 a year. This idea matched the plans that Dean Hammond and I were making to expand Penn State's engineering research. Also, it seemed like such small potatoes that the dean and I saw no reason to seek approval from college president Ralph D. Hetzel. The sequel is well described by historian Michael Bezilla in *Penn State: An Illustrated History* (1985):

> No sooner had Hammond given his consent than key officials in the Navy and the OSRD decided that torpedo research deserved greater support and asked Walker to set up a laboratory at Penn State containing at least 100,000 square feet and employing over one hundred persons.

A proposal of that magnitude had to be put before Hetzel and the trustees. On a snowy day in January 1945, Walker and several high-ranking naval officers called on the president at his private residence. They outlined a program whereby the Navy would pay for the construction of a large laboratory building on the campus, where defense research would be done under contract to the Bureau of Ordnance. The Bureau would pay all operating and maintenance costs plus annual fees to he College. Hetzel expressed his willingness to have his institution cooperate with the Navy in what seemed a financially worthwhile as well as patriotic endeavor. Yet the whole matter had arisen so suddenly that he could not help asking his visitors, why Penn State? "Well, Walker's here," replied one of the officers, "and it's a nice place." That simple remark apparently satisfied the president. He promised to recommend approval of the Navy's proposition when he took it before the board of trustees at its semiannual meeting a few days hence.

The trustees approved, and so began my quarter century at Penn State. Here in Happy Valley—as Penn Staters call it with a mixture of irony and affection—I was able to use all my talents and pursue all my interests. At Penn State, I was given the opportunity to apply my engineering skill and experience to important problems ranging from health care to environmental protection to national defense. I also had ample opportunity to show my mettle as an administrator and a politician. The pressure of war was sometimes grueling, but it was valuable preparation for the rest of my career.

VANNEVAR BUSH:
Statesman of Engineering

The full range of Vannevar Bush's service to our country has not yet been recorded. Van guided much of the United States' weapons research during World War II—a key factor in the Allied victory. He was the first coordinator of such research, responding to President Roosevelt's call to make our nation "the arsenal of democracy." Van's pioneering principles of research administration set the course for weapons research from 1940 to the present. If his principles had been followed more closely, we would have achieved a stronger national defense at lower cost during the Cold War era. Van made his patriotic contributions while continuing to work as an electrical engineer (with fifty patents) and an engineering educator.

Van Bush would have been one of my models even if I had viewed his actions from afar. As it happened, the long arm of coincidence pulled us together at a number of points. Born in 1890, a generation before my birth, Van first taught mathematics and electrical engineering in 1914–17 at Tufts, where I taught the same subjects in 1934–39. While at Tufts, I did practical research in my specialty, electrical power transmission, and came up with some patentable devices. During Van's Tufts years he did research in the new field of radio transmission and patented some improvements in vacuum tubes. Later, while teaching at MIT, he invented the differential analyzer, a calculating machine that works by electronics. Van served as engineering dean of MIT in 1932–38 while the world was sliding toward World War II. My deanship at Penn State, in which I learned many lessons from Van, came during the Korean War. My closest relations with Van Bush were from 1939 to 1955, when he was president of the Carnegie Institution of Washington and also, from 1940 onward, the key figure in national defense research. Van set forth his credo in 1949 in the book *Modern Arms and Free Men*.

As the war clouds darkened in 1940, President Roosevelt asked his friend Vannevar Bush to coordinate a nationwide defense research effort. Van set up the National Defense Research Committee (NDRC) with Congressional approval and help from three friends: Karl Compton, president of MIT; Jim Conant, president of Harvard; and Frank Jewett, president of Bell Telephone Laboratories as well as the National Academy

f Sciences. This distinguished quartet organized and funded subcom-
nittees and laboratories on every phase of modern weaponry, including
ne Johns Hopkins Applied Physics Laboratory, the Harvard Underwater
ound Laboratory, and the MIT Radiation Laboratory. The last was a
pecial concern of Van's. And in 1942, the Army's Manhattan Project
vas added, which harnessed the nuclear chain reactions started by Enrico
ermi and his associates at the University of Chicago.

The strength of the NDRC was its blending of basic scientific re-
earch with hands-on engineering know-how. Its weakness was its lack
of clout with the military, which had had its own labs since World War
and was suspicious of long-haired professors and their newfangled ideas.
When Van reported this weakness to FDR, the president and Congress
stablished the Office of Scientific Research and Development (OSRD)
vith Bush as director. Roosevelt refused one of Van's requests: namely,
hat OSRD be given control of *all* military research and development.
The commander-in-chief was unwilling to strip the generals and admirals
of turf they had held for a quarter century. But the OSRD did have a
ransition section to move inventions from laboratories into factories. And
he brass hats were won over by OSRD's early and continuing successes,
ncluding radar, sonar, proximity fuses, homing torpedoes, and the atomic
bomb.

I often wonder how history would have changed if Vannevar Bush
had been as friendly with President Truman as he was with President
Roosevelt. Toward the end of World War II, Van conceived a grand
scheme for national research, nonmilitary as well as military. He sets this
forth in his pamphlet *Science, the Endless Frontier*. At the heart of Van's
scheme was a National Science and Engineering Foundation, a free-stand-
ing organization whose board of directors would select its director. This
foundation, as Van saw it, was to be supported by grants from industry,
other foundations, and the government. The foundation in his mind would
set policy and encourage research in *all* fields—civilian and military,
pure and applied, physical, social and medical.

Van was thwarted in his first attempts to get enabling legislation for
his brainchild. Military and medical leaders opposed an omnibus foun-
dation because they had vested interests in such agencies as the National
Institutes of Health, the Office of Naval Research, and National Advisory
Committee on Aeronautics, and the Army Materiel Command. Truman
successfully vetoed one bill because he opposed the degree of autonomy
it gave to the science and engineering foundation. The idea might well

have died except for Van's dogged tenacity. Unfortunately, most scientists and engineers had lost interest in national policy, having returned to their old posts on campuses or in corporations. Finally, in 1950, a compromise act was passed, establishing a National Science Foundation. While the bill was before Congress, I remonstrated with Van about the dropping of "and Engineering" from the foundation's title. "Let's not rock the boat," he replied. "We must get something passed. And anyway, to most people, science includes engineering." On that observation, Van for once was wrong.

The NSF act authorized the expenditure of $15 million, but this money still had to be appropriated. Van Bush was not appointed to the NSF board. Indeed, he discouraged such an appointment for himself on the grounds that the president, many members of Congress, and many scientists and engineers had had enough of him. I became one of a trio who testified before the House Appropriations Committee on behalf of funding the NSF. The others were Alan Waterman, chief scientist of the Office of Naval Research, and Lloyd Berkner, who was establishing the Southwest Research Institute at Southern Methodist University. I appeared as executive secretary of the research and development board of the Department of Defense.

Hearings on the NSF appropriations took place in an ill-lighted, cavernous room, with five of us at a table that would accommodate thirty. Present besides the witnesses were the committee chairman, "Uncle Joe" Cannon, and an aide. Since Cannon read a newspaper while Lloyd and Alan delivered carefully prepared statements, I risked being considered impudent by saying, "Mr. Cannon, I am not going to read a statement because I want you to listen." While I was speaking, the chairman left the room. Upon the Congressman's return, he asked if anyone in the small audience wished to speak. "Yes, I do," came a voice from the shadows. Upon being recognized, the speaker stated, "I am a horticulturalist, a rose grower from Ohio. I know something about research and the need for more of it. I think NSF is a good idea and Congress should give it the money to get going. Why don't you do it?" Chairman Cannon was used to asking questions, not answering them. After a few seconds he turned to his aide and said, "Give them three hundred and fifty thousand. Any other remarks? Meeting adjourned."

Van Bush's vision and leadership were lost by the National Science Foundation because of his conviction that his brainchild had been robbed by the politicians of both independence and breadth. I shared Van's disappointment, especially regarding the short shrift given to engineering. The dominating scientists in NSF felt that engineers had adequate money sources in the armed forces and in industry. Only one engineer was appointed to the NSF board at the start, and I was then too far down in the establishment to be the one. Since I was acquainted with Alan Waterman, who moved from ONR to become the first NSF director, I attended a few NSF conferences and judged a few proposals. But my contacts with NSF during its first dozen years were minimal. Then, in 1962, I was appointed to the NSF board. That appointment occurred while I was president of Penn State, and my resulting adventures are described in my final chapter. Here I simply want to report that I outdid myself in eloquence when I pleaded the case for support of engineering research at meetings of the NSF board, and that Van Bush would have been proud of me.

Disillusioned with NSF, Van found an ally in James Forrestal, first secretary of the newly created Department of Defense. In 1947, when the Air Force became a separate branch, Congress created a three-pronged Defense Department, originally called the National Military Establishment. Forrestal, who had been appointed secretary of the Navy by Roosevelt in 1944, became the first secretary of Defense. Bush and Forrestal saw eye to eye on the twin objectives of opening all promising avenues of military research while avoiding wasteful duplication. To meet these objectives, Van and Secretary Forrestal proposed a departmental research and development board, with a prominent research administrator as chairman and with a membership consisting of the assistant secretary for R&D plus the chief research officer for each of the services. President Truman approved the plan and, on Forrestal's recommendation, appointed Vannevar Bush as chairman.

Van's master plan for the research and development board had two parts. On the input side, he set up a score of committees from industry and academia to prepare position papers in all pertinent fields from aeronautics to P.O.L. (petroleum, oil, lubricants). The committees succeeded in supplying a wealth of suggestions for action. On the output side, Van devised a clever scheme to avoid "logrolling"— political tradeoffs—to wit, that the chairman would cast the deciding vote whenever the other six members failed to reach a consensus. Thus a majority could not overwhelm a minority, and a minority could not "sell" its vote to a majority. Secretary Forrestal accepted Van's voting scheme. After Forrestal's breakdown, however, his three short-term successors in

1949–52—Louis Johnson, George Marshall, and Robert Lovett— established majority rule. I suspect that the three services took these secretaries into camp. The result was that the research and development board became a paper tiger, in two ways. On one hand, the board could not stop wasteful duplication or even triplication. On the other hand, the board could not start research that no one of the services wanted to tackle.

When I served as executive secretary of the DOD research and development board in 1950–52, I learned the wisdom of Van's voting scheme. Elsewhere I have described interservice duplication and my failure to thwart it even in such an obvious field as parachute testing. I also learned the futility of trying to fund stepchild projects—those that none of the services wanted to adopt. On one occasion I sought $50 million for such projects. I convinced William Webster, who had succeeded Van Bush as board chairman, that the projects were vital. Chairman Webster convinced Secretary Lovett. Joyfully I reported our success to my staff. McKay Donkin, always the realist, warned, "Don't believe it until you see it. The services will do their best to block independent action by the board." McKay's prediction was accurate. The RDB never got an independent budget and slowly slipped away. It was partially reincarnated late in the Korean War as DARPA (Defense Advance Research Projects Agency), a unit with its own funds in the office of the undersecretary of defense.

I left Washington in 1952, mostly because I wanted to go home to Penn State but partly because I was frustrated by the failure off RDB to quell interservice rivalries. Vannevar Bush left around the same time in considerably greater frustration. He told me, "I'm putting on my hat, leaving, and never coming back." He never did. Thus both of Van Bush's ideas were aborted, and most of his grand scheme for the national research program went down the drain. Our nation is still paying for its failure to heed this statesman of engineering.

ADMIRAL RICKOVER:
The Explosive Engineer

My contacts with Hyman George Rickover were few, brief, abrasive, and, in retrospect, highly amusing. I trust he was aware of my respect for his achievements, despite the sharpness of our encounters. Actually we had a common bond besides our passion for creative engineering: we had both started as immigrant boys, the admiral having been born in Poland in 1900 whereas I had been born in England in 1910.

Rickover was proud to be known as "the father of the nuclear navy," a title he richly deserved. He put the nuclear navy together by sheer drive while often being cantankerous, unreasonably demanding, and even abusive. Unquestionably Rickover got results from the Navy with backing from the Congress. Whether he could have gotten the same results with gentler means—toward fellow officers, industry executives, and university professors—is a question I leave to the historians.

I first heard of Rickover during my World War II service with the Underwater Sound Laboratory. My Navy friends disliked his manner but respected his work. In 1947, Rickover became head of the Navy's Nuclear Power Division as well as the Naval Reactors Branch of the U.S. Atomic Energy Commission. Rickover directed the project leading to the world's first nuclear-powered warship, the submarine *Nautilus*, launched in 1954. In the developmental work for *Nautilus*, Rickover worked closely with engineers from Westinghouse who were constructing the first commercial nuclear electric-power plant in the United States; that plant began operating in 1957 at Shippingport, Pennsylvania.

My first encounter with Admiral Rickover erupted when I became chairman of the Naval Research Advisory Committee in the late fifties. By now the Soviets had nuclear submarines, so we were concerned about staying competitive. One of our committee members raised the question of whether our Navy should be developing a new type of reactor in line with the state of the art—a "fireball" reactor that would be more compact than the water-cooled reactor in use. To be sure, the water-cooled reactor worked efficiently, had a perfect safety record, and presented no health hazard to crew members. But this type of reactor had a basic design developed a decade earlier and was relatively large. Adopting a little of

Rickover's feistyness, we concluded our report by saying "the admiral is guaranteeing an obsolete nuclear navy."

When the assistant secretary of the Navy for research and development transmitted our report to Admiral Rickover, he predictably exploded. Rickover then phoned me. Although I did not hear all his words because I had to hold the receiver two feet from my ear for fear of injury, I gathered he considered me a nincompoop who should be fired for incompetence. In fact, I was on weak ground since I was not prepared to cope with his expertise. The admiral's parting shot was "You take care of your business, and I'll take care of mine."

My second encounter with Rickover came when I became president of the National Academy of Engineering. I was among the members who supported Rickover's election to membership. He certainly deserved it, despite his unpopularity in some quarters. By now he was a vice admiral, and in 1965 he was to win the Enrico Fermi Medal, the highest atomic-science award. But Rickover was not always generous to other prospective members of the academy. The admiral had a special prejudice against industry executives, for whatever reasons lying deep within his psyche. On one occasion, the academy was trying to improve the balance in its membership among representatives of the academic community, government, and industry. When the admiral received the slate of proposed new members, he gave me another smoking phone call. "Some of these guys aren't engineers," he charged. "They're accountants—mere bean counters." "Slow down," I replied, "All candidates' credentials as engineers have been established by peer review. At present some of them are directing programs in industry, just as you are directing a program for the Navy." After some sputtering, he dropped the matter.

My third encounter with Rickover came in 1959 when I was president of Penn State and the university was hiring a head for our nuclear engineering department. The front-running candidate, Nunzio J. Palladino, was an engineer with Westinghouse who had worked on the Shippingport reactor. Since the admiral had been involved with this reactor, he regarded it as one of his private preserves. Palladino cautioned me that the admiral would try to block his departure from Westinghouse, which still had Navy submarine contracts. Sure enough, Rickover gave me another hot phone call. "You are wrecking our country's submarine program," he explained. "So I will see to it that Penn State gets no more Navy contracts." That was an empty threat, as Rickover probably realized. Joe Palladino became head of nuclear engineering at Penn State in 1959, dean of engineering in 1966, and chairman of the Nuclear Regulatory Commission in 1981.

My fourth and last encounter with Rickover came in the sixties shortly after I had made a speech about the future of engineering education. The admiral read the speech and phoned to tell me that I was all wrong, that I had misunderstood engineering education, and that, if the world listened to me, the engineering profession would go down the drain. Here was the reverse of our roles during our first encounter, when I was in effect challenging the leading expert. Admiral Rickover in the fifties was clearly the chief authority on nuclear propulsion of submarines. By the sixties I could claim, without excessive immodesty, to be a leading expert on engineering education. It is true that the admiral's staff was highly competent, its members often becoming stars of the Navy, and that their competence stemmed partly from Rickover's training program. It is also true that he started with picked men—officers with the finest educations. Despite Rickover's criticisms of the educational establishment in speeches and in books such as *Education and Freedom* (1959), our universities must have been doing something right. Improvement of engineering education—as of all things—was in order but seemed to me most likely to come from those who had devoted a large part of their careers to learning the ropes. Responding to Rickover's telephonic blast, therefore, I said, "Admiral, you once called me about my advice regarding nuclear reactors. You told me to mind my own business because I obviously was no expert on the subject. On that occasion you were right. On this occasion you are wrong because I am the expert, so I must ask you to mind your own business."

Admiral Rickover and I met occasionally after that conversation. Our behavior was always guarded, as though neither of us wanted another encounter in which one of us would come out on top. I hope Rickover realized that I never regarded our sparring matches as more than a sideshow and that the "main event" was our shared ambition to serve our adopted country to the utmost degree of our ability as engineers.

BALANCING EDUCATION AND RESEARCH
Penn State Engineering School 1945–56

My goal as a department head and a dean at Penn State was to balance the needs of my university with those of my country. The fact that the objectives of my two loyalties were adopted simply sharpened my sense of obligation. The mission of Penn State, as the Commonwealth's land-grant college (it was not formally designated a university until 1953), was to provide higher education for young Pennsylvanians, together with research findings for all Pennsylvanians. By claiming to be a university rather than a mere training school, Penn State took on a further obligation: to serve the world of scholarship by sharing its pedagogical and investigatory know-how. But Penn State had a third obligation in my view: to serve all of the United States of America and the armed forces that protect our federal republic.

Life is, among many things, a balancing act. Obviously there are conflicts, or seeming conflicts, among the demands of a university's various constituencies. Cannot the same be said about every institution—or, for that matter, every individual? Balancing one's obligations to self, family, friends, community, vocation, and larger society has been a classic challenge since the Athenian and Roman republics. Balance, as I see it, should not be gained by omitting one element altogether, such as national defense research in the case of a university. Balance, moreover, is not a static phenomenon achieved once and for all but is, rather, a dynamic process.

I came to Penn State in 1945, balancing my obligations as head of the department of electrical engineering with my duties as director of the Navy's Ordnance Research Laboratory. On the side of national defense, I served on the Committee on Undersea Warfare and the Naval Research Advisory Committee, and I did a hitch in the Pentagon during the Korean War. On the side of engineering education, I was active in the American Society for Engineering Education. Bridging my military and academic concerns was the annual Conference on Administration of Research, which I launched. That conference brought in a third element: business and industry. This element was prominent in the work of two committees on which President Eisenhower asked me to serve: one on scientists and engineers and one on research for small industry. Although some of the reports of these committees are gathering dust, some had a stimulating effect in their day, and a few continue to be influential.

Most of Penn State's administrators welcomed the Ordnance Research Laboratory, but some resented the degree of autonomy and secrecy with which the Navy surrounded its outpost. President Hetzel was favorable from the start, perhaps because of his satisfaction with the college's wartime programs such as Engineering Science and Management Defense Training (ESMDT). Hetzel sold the trustees and most of his staff on the need for ORL's special status. Some resistance came from the purchasing people, who saw the lab's procurement officer, a Navy commander, as an interloper. Five years later Milton Eisenhower, as Penn State president, faced a similar internal political problem. For twenty years the college's agricultural extension service had resisted the field staff of the U.S. Department of Agriculture's Soil Conservation Service as rivals. Milton used his experience in the USDA to eliminate this friction. Similarly, I tried to use my Navy experience to gain acceptance of ORL on the Penn State campus. One Penn Stater who was hard to sell was Lou Bell, director of public information. At first Lou got all steamed up about the new lab and wanted to start issuing news releases. Then I told him that he could not see the ORL work because he lacked security clearance. Perhaps I was not tactful enough, especially in view of Lou's initial enthusiasm. His wounded feelings cost us his support. I partly patched things up with Bell in 1956, when he launched a university press with my enthusiastic backing.

My trip to Penn State in January 1945, when ORL was set up, introduced me to the transportation and weather problems of central Pennsylvania. My two previous visits to the land of the Nittany Lion—so named for a legendary cougar that inhabited Mount Nittany—were by automobile in fine weather. My 1945 trip, with Captains Jerry O'Donnell and Butch Parker, was by public transportation. Thus I learned the significance of former President Sparks's wisecrack: "Penn State was equally inaccessible from all parts of the state." I also learned that blizzards are more ferocious at Penn State's site in the Allegheny Mountains than at my boyhood hometown of Wrightsville in Pennsylvania's Piedmont.

The captains and I joined forces at Harrisburg, I coming from Boston and they from Washington, to complete the journey to Lewistown, a flagstop on the Pennsy's mainline. We detrained in the evening at a deserted station, surrounded by knee-deep snow. During our thirty-mile bus ride to the town of State College, we marveled that our driver could find the road through the driving snowflakes. A small elderly gent known as "Crummy," our driver regaled us with stories about how even football players quaked while being transported over the Seven Mountains in wintry weather. But the snow on the pines and hemlocks created a winter wonderland. At the Nittany Lion Inn we were welcomed by a roaring fire in the lobby fireplace, but the dining room was closed, so we retired hungry. Next morning we mushed to the president's residence, with Jerry O'Donnell breaking a path and Butch Parker stomping the snow. When the conference group had been assembled, Captain O'Donnell announced: "The secretary of the Navy had decided . . ." Later, after details of the ORL plan were ironed out—the broad outlines having been approved earlier by the college trustees—my serious work began. My first session on the building design, with the architect Paul Cret and my deputy Nelson Butz, lasted almost twenty-four continuous hours. Construction began on the Ordnance Research Laboratory as soon as weather permitted, and it was completed in 1946. Since Penn State is about two hundred miles from the sea and almost as far from Lake Erie, a field-testing station was established at a lake in Black Moshannon State Park, twenty miles from the lab. The cost of laboratory and station was half-a-million 1947 dollars. Meanwhile, a staff of engineers and scientists was being assembled—mostly from the Harvard Underwater Sound Laboratory—plus a technical staff partly from HUSL and partly from local talented artisans. The forty scientists and engineers were accorded faculty rank in the engineering school, pleasing that school's Dean Hammond but displeasing the deans of Penn State's seven other schools in varying degrees.

Most of the townspeople of State College were supportive of ORL, but a few were not and thereby contributed to the town-and-gown friction that afflicts most college communities. The contractor for the Black Moshannon station failed to meet specifications and has not received final payment to this day. This contractor failed to remove the specified amount of rock from the lake because, we gathered, his banker failed to give him an adequate equipment loan. Luckily, the properties of our calibration station turned out to be satisfactory because the lake's mud bottom was a good acoustic absorber. The problem of finding housing for one hundred and twenty families, brought to State College with ORL, could not be solved by a stroke of luck like a muddy lake bottom.

Housing was historically short around Penn State, and the postwar influx of married graduate students exacerbated the shortage. Dean Hammond could not offer me—the director— anything more than a two-room counselor's suite in Track House (the residence for track men), which I found too cramped for my wife and three-year-old daughter. The dean's assistant, Royal Gearhart, located only a few houses or apartments for rent or sale. With the Navy's approval, therefore, we started a crash building program. Commander McWilliams, the Navy resident officer, got housing priorities—needed because of postwar materials shortages—for forty $11,000.00 houses and eighty $6,000.00 houses. Although the Navy insured the college against loss on the project, financing had to be private. The contractor for the Black Moshannon station volunteered, suggesting his same banker as a source of funds. We turned down the builder, and the banker turned us down. In the end we used Philadelphia banks and a Manhattan contractor. Then the dog-in-the-manger local banker tried to shut off water from our housing project through his influence with two nearby water companies. Commander McWilliams demanded a public hearing, whereupon we got our water. But we had made an enemy. The local newspaper, controlled by the same hostile banker, said of our project: "The crowding of so many houses on so little land will create a slum." Now, two generations later, our houses still stand, well cared for and landscaped with lawns, trees, and ivy. So much for an uncooperative "towny"!

I soon learned why Dean Hammond had been so eager to bring me to Penn State from the time of our first meeting at Tufts in 1938. My research orientation was the key. The dean had commented favorably on my efforts to set up an electrical engineering lab at Tufts. He wanted me to do the same at Penn State. In 1945 he was getting not only my determination in setting up research facilities but also a well-staffed, fully operational ordnance lab. When I started poking around my new college, I found a very spotty research effort, especially in electrical engineering. The basic reason for this was that Penn State had always been on short rations since its founding in 1855 as the Farmer's High School, for reasons spelled out in Michael Bezilla's recent history. As a "state-related" rather than "State-created" institution, it got less tax money than state universities in the Midwest and West. Having fewer rich alumni than the privately endowed University of Pennsylvania, Lehigh, or Swarthmore, it got fewer big gifts. Penn State also suffered from the widespread misapprehension that its access to public funds was unlimited. In a real sense, it was in "the worst of two worlds"—a condition from which it has not entirely escaped even as late as the 1980s.

For perfectly understandable historical and administrative reasons, much engineering research at Penn Sate was not in the School of Engineering. It was in the School of Agriculture, which did significant research on farm equipment; in the School of Chemistry and Physics, which worked on such problems as the production of heavy water and penicillin; and in the School of Mineral Industries, which had such facilities as a model coal mine. The engineering school's research was concentrated in the Engineering Experiment Station, set up in 1911 by a professor of mechanical engineering, Louis Harding. This station focused on heat transfer and in the forties was pursuing efforts to develop more efficient insulation for houses, factories, and railway cars. Through the work of Paul Schweitzer, who came in 1930, Penn State had become a center for diesel engineering research, as was recognized by the Navy. The sum of the research effort in Dean Hammond's school was pretty much in two areas: insulation materials and diesel engines.

In the electrical engineering building, I found nearly all the research laboratories being used as storage rooms. The only research project was some calibration work in the low frequency broadcast range of radio waves. Here, I thought, was one missed opportunity, since Penn State had established a radio broadcasting station in 1921, less than a year after KDKA, the first commercial station, went on the air in Pittsburgh. Although the facilities and the staff of the EE department were not all they should be, they were respectable. My first challenge was to lead the department toward effective use of the resources at hand. I believe my efforts were successful during my four years as department head, with the help of faculty members like Art Waynick and Jack Mentzer. By the time I became dean, the Penn State EE department was ready to play its part in the nuclear computer age.

Investigations at the Ordnance Research Laboratory were primarily a continuation of classified wartime work on homing torpedoes. We did, however, take on assignments in other fields, including medical engineering. That started in 1947, when I had lunch with Isadore Radvin, the famous surgeon at the University of Pennsylvania. Dr. Radvin arranged for me to visit a leading distributor of devices used by the medical profession. What I saw shocked me by its primitive appearance. One device used for spreading a patient's ribs—a device used during chest operations—looked as though it had been designed by Ctesibus. The company's president conceded that the devices were archaic and gave this explanation: "When you sell only one or two of these gadgets a year, you can't change the design very often. Doctors are conservative; most of them keep their equipment until they retire and then sell it with the practice." Further conversation with Radvin highlighted this economic aspect of the engineering problem. While it is true that demand must exist to make any product economically feasible, that demand is usually created by engineers working with creative users. Engineers like me do not know what to invent, whereas physicians like Radvin do not know what they can reasonably ask for. A symbiotic process is needed to get demand and supply in phase.

A month later, Dr. Radvin phoned to say he wanted to come to Penn State to discuss a problem. I assembled some of the most capable staffers at ORL: experts, respectively, in electronics, acoustics, and hydraulics. Dr. Radvin arrived and pulled a quart jar from his backpack. From the jar he poured objects that he identified as gallstones, ranging from three inches in diameter to the size of marbles to the smallness of grains of sand. "When I operate," he explained, pointing with a pencil, "these larger stones cause no trouble. I can put my finger down the bile duct and remove the marble-sized ones, but I'm never sure I've gotten all the sand." He went on to explain that a first gall bladder operation is not highly dangerous but that the danger increases when a second incision is needed or when a catheter must be inserted in order to dissolve the sand with ether. "Now," he concluded, "if you chaps can devise a way to detect the sand, you will have contributed to a life-saving improvement in a surgical procedure."

After further discussion, I announced a competition for solutions to Dr. Radvin's problem. Five solutions were considered, two of which were selected for developmental work. One device emerged as optimal, one we called the coleolithophone. It consisted of a very small microphone mounted on a catheter thin enough to insert in the bile duct. When the microphone came into contact with a bit of sand, it produced a rasping noise in a speaker, or it could be used to switch on a light or ring a bell. Dr. Radvin tested it successfully and recommended it to the Pillings Company for distribution. For manufacturing the coleolithophone we formed Centre Electronic Company (Penn State is in Centre County). The coleolithophone was certainly what the French call a *succé d'estime*.

It was used successfully throughout the world, notably in the Soviet Union. The total sale of fifty instruments, however, was disappointing. Dr. Radvin explained that the coleolithophone was not seen by patients, since it was used in operating rooms, and thus did not profit from word-of-mouth advertising. Hospitals in the forties, moreover, were not yet conditioned to buy expensive equipment like EKG devices, dialysis machines, and CAT scanners. Our disappointment discouraged us from pursuing this line of research, to my regret.

Nonetheless, the coleolithophone resulted from one of the first applications of modern physical science to medicine. At ORL we were pioneers in the new and now thriving field of medical engineering. Indeed, the path we opened—the use of acoustics to detect gallstones—has been followed by a device called the lithotryptor, which uses acoustical energy to pulverize gallstones. I do not mean to detract from the achievement of the inventors of the lithotryptor but merely to record that we helped to blaze the trail they took.

The coleolithophone resulted from the nonmilitary use of a defense lab. But military research itself often has civilian applications. Nuclear energy is, obviously, used for the generation of energy and the treatment of cancer, as well as deterrence. At Penn State, Navy-supported research on ship welding at the Engineering Experiment Station, and on lubricants and hydraulic fluids at the Petroleum Refining Laboratory, are of clear value to the merchant marine. Study of the ionosphere, initiated by the Army's Watson Laboratories and later cosponsored by the Air Force and Penn State's electrical engineering department, is valuable to all radio broadcasters. Nonmilitary uses for research at the ORL's Garfield Thomas Water Tunnel, the largest one of its kind, seem almost sure to suggest themselves.

We needed a circulating water tunnel to test the shapes of torpedoes and other submersible ordnance, as well as the designs of hulls and propellers. Designing this facility was one of ORL's most exciting challenges during my third year at Penn State. Objects to be studied for hydrodynamic qualities can be placed in a section of the tunnel where water velocities up to sixty feet per second are attained with a 2,000-horsepower motor. Named for Lieutenant W. Garfield Thomas, Class of 1938, one of the first Penn State alumni to be killed in World War II, the water tunnel was dedicated on October 7, 1949. At the dedication, John T. Koehler, assistant secretary of the Navy, gave the rationale for ORL: "Investment of military dollars in scientific research in peace time pays dividends in the form of time saved in wartime, and saving time during a war means saving lives, material, and money." By 1949–50, the annual budget of the Ordnance Research Laboratory reached $1.4 million.

In balancing research and education, Penn State must give more weight to education than needs to be given by the prestigious private institutions. I mean especially undergraduate education. The citizenry of Pennsylvania demand a certain amount of useful research from their state-related institutions. But they make a more clamorous demand for vocational education–"education for careers," in fancy parlance. Penn State is politically able to maintain teaching and learning standards, but only within limits. In a sense, the burden of proof is on Penn State—in a manner unknown at Princeton, Chicago, or Cal Tech—to show that its standards are not unreasonably high. "Elitism" is a dirty word to many citizens, particularly when it is applied to any institution receiving a significant amount of public support (that is, *known* public support). To my way of thinking, however, there is a sense in which every profession or quasiprofession should be an elite. I refer to the maintenance of standards, not of privileges or status symbols. Of course, I take a particularly "elitist" view of the two fields I know best: engineering and science. When standards are at issue, I would rather err on the high side than the low side. Since I believe research is related to high standards of teaching and learning, I hardly ever find research excessive. My bias puts me in the hard-nosed camp in debates on educational policy. At the same time, I recognize that the softer proboscis symbolizing the opposite camp may provide needed balance.

Dean Harry P. Hammond represented the educational side of Penn State's engineering school, and he hired me to beef up the research side. When I came to Penn State, its engineering school was among the top ten nationally in undergraduate enrollment and had a minuscule graduate enrollment. Despite Penn State's widespread image as a "cow college," enrollment in engineering was larger than in agriculture and was second only to that of liberal arts, which then included business administration. Naturally, the dean was keenly aware of the needs of all these aspiring engineers. As it happened, Hammond's career was dedicated to those needs.

Although Brooklyn Poly, where Harry Hammond won his spurs, has produced excellent working engineers and researchers, he chose to confine his activities almost exclusively to engineering education. He did a hitch as a civil engineer under Arthur E. Morgan (later of TVA fame) in the early thirties, doing flood-control work on the Miami River in southern Ohio. Hammond's only doctorate was an honorary one, received late in his career, of which he was visibly proud. His chief forum was the Society for the Promotion of Engineering Education (SPEE), which in the late thirties evolved into the American Society for Engineering Education (ASEE). ASEE, wherein he served in various roles including president, was also Dean Hammond's claim to fame. He was the author of two of ASEE's widely publicized reports and the winner of its Lamme Award. When Dean Hammond visited me at Tufts in 1939, he persuaded me to become active in his cherished society. "Persuade" is not quite the apt word, since the dean's method of exerting influence was subtle: It was an osmosis by which his feelings seeped into one's pores.

Hammond had chaired as ASEE committee on the future of engineering education before the United States got into World War II. His hope of emulating the impact of the Flexner Report on medical education was dashed at Pearl Harbor. After V-J Day he resumed the project, with me on his committee. Ostensibly he wanted my expertise on electrical engineering and communications. In part, such was the true case. In larger part, the dean was drawing me into his political sphere. In saying so, I do not impugn his motives. We all want our protegés to share our political involvements. As time passed and I became increasingly aware that Hammond was grooming me to succeed him as dean, I felt a growing pressure to work for ASEE. (Parenthetically, I should note that my first inkling of my dean's ambitions for me came from the campus gossip mill.) Ultimately Dean Hammond told me straight out that every dean of engineering at Penn State had served at one time in his career as president of ASEE (or SPEE, its forebear): Q.E.D.

Thus began my involvement during almost a quarter century in a succession of engineering education studies. The postwar Hammond study was followed by the Grinter study of 1955 and the Burdell report of 1956. (It should be noted that the prewar Hammond study was preceded by the Mann report of 1918 and Wickenden's during the twenties.) Finally, over the period 1962–68, came the reports of the ASEE committee on the Goals of Engineering Education, chaired by me. There was considerable irony in my long association with these studies in light of two of my well-known biases. One was my "elitist" bias toward the engineering profession, which I think should be reserved for persons of exceptional intelligence. The other was my bias against long-standing committees.

Committees, like humans, in my view, tend to decline from the vigor of youth to the complacency of middle age to the weariness of old age.

One underlying theme of the SPEE/ASEE reports was the need for more math and basic science in the engineering curriculum. As the reports emphasized, scientists have done some fine engineering (one thinks of Faraday, Henry, and Urey), and, of course, the reverse is true (consider Marconi, Steinmetz, and Shockley). One of my themes in this book is that there must be a symbiosis between science and engineering. To say so, however, is a far cry from considering the two fields as identical. That would be tantamount to saying that studying economics and psychology is equivalent to learning business management. My quarrel with the engineering curriculum at Harvard was that it gave me no guidance in "putting handles" on scientific and mathematical knowledge. To be sure, employers vary in their expectations concerning entry-level employees. Smaller companies are likely to feel that they need people who can do hands-on jobs immediately. Such firms tend to consider basic science and math impractical. In fact, smaller firms may be unable to afford the luxury of on-the-job training, or they may merely be shortsighted and pennywise. The issue has no easy solution. All I know for sure is that the big progressive companies have always said, in effect, to engineering educators, "You teach your students how to think and, when we get them, we'll teach them the nuts and bolts of engineering." They always add, "And teach them to write a decent report." On the score of verbal communication, engineering education studies from 1918 to the 1940s had apparently had little effect.

I commend Dean Hammond's insistence, in his ASEE reports and elsewhere, that engineers should be literate. They should be able to write and speak in clear, accurate, and concise English (or whatever language has currency where they are working). Although engineers use various symbolic languages in their formulas and drawings, words are more important. Engineers must understand what they are being asked to do, usually in words, and must explain their plans verbally. An engineer who, no matter how brilliant, cannot communicate is not an engineer but just a designer.

Despite my approval of Dean Hammond's goal of literacy in engineering education, I found his proposed means bafflingly mid-Victorian. He thought engineering students should read Scott's *Ivanhoe* and learn to recite Bryant's *Thanatopsis*! In general, Dean Hammond—no one called him anything else—reminded me of the central character in *Life* *with Father*. He was a great respecter of authority: you did as your superiors told you, and your inferiors did as you told them. When he gave an order—with his stern visage and piercing eyes—he expected it to be followed without question. All the students and most of the faculty feared the dean. To his underlings he cut an imposing figure, though actually he was of middling height and only a bit on the stocky side.

To Dean Hammond's peers and betters, he was less than infallible, for they saw through his limitations and prejudices. One of his prejudices was against foreign faculty members, not because of any ethnic aversion but because of his difficulty in understanding their accents and their cultural outlooks. Joe Marin, head of the mechanics department, hired a number of foreign scholars for his department—the most theoretical, rigorous, and altogether distinguished ones in the school. Two members of Joe's department were Chinese professors named Hsioa and Hu, whom the dean obviously could not tell apart. Once when I was in his office, he answered his phone—as was his custom, since he found the intervention of a secretary both inefficient and impolite—with his usual "Hammond here."

I could hear through the receiver: "Hu speaking."

Hammond paused a while and repeated, "Hammond here."

From the other end: "Hu here."

Hammond: "Hammond speaking."

From the other end: "Hu speaking."

Hammond: "Who's speaking?"

The other end: "Hu speaking."

Hammond backed off, looked at the phone, and slammed it down. "Damn fool doesn't even know his own name," he said.

Stories about Dean Hammond's prejudices, his rigidity, and his propriety are legion. Because the university president drove a Cadillac, Hammond felt strongly that he and his fellow deans should drive nothing grander than Buicks. Although he would take an occasional cocktail, he would not do so in a public place like a hotel dining room. When a group of us were on a trip to Pittsburgh, Dean Hammond got into an argument with Professor Everett about the direction in which the Ohio River flows near the city. I tried to show that Everett was right by dropping a wad of paper into the current. But the dean simply reacted by saying, "Humph, must be the action of the wind."

Yet this stiff-necked man contributed to the quality of engineering education both at Penn State and nationally. Knowing that research administration was not his forte, he brought me to complement his efforts. Regarding me as his peer, he treated me with respect, relying more on hints than on commands to steer me in the direction he favored. He rarely tried to second-guess me about the activities of the Ordnance Research Laboratory, and I was careful to give him the fullest weekly reports I could so that he would not feel left out. As long as the dean was "in the know," he was likely to feel that he was in control.

Over time I discovered that Harry Hammond was afflicted with a loneliness that deserved my sympathy. He and his charming wife had virtually no social life. Once a group of us persuaded him to join our regular Friday afternoon golf game. Although the dean played decently and seemed to enjoy himself, he never came again. One day, when I was digging in my garden, I struck a one-inch layer of coal about twenty inches below the surface. Remembering Dean Hammond's remark that an overburden of more than ten to one makes coal mining unprofitable, I phoned him to report that a coal mine in my back yard would be uneconomical. Instead of a ha-ha, his reaction was a sober "That's interesting." A while later he phoned me to say that he had been misinformed and that a twenty-to-one overburden could be profitable. Was he pulling my leg, or was this another case of his notorious lack of a sense of humor? Belatedly I discovered that Dean Hammond could be forgiven for his sobersidedness, since his two sons were confined to an institution for the handicapped.

The dean divulged this personal information to me in the course of explaining why he had requested a two-year extension of his deanship beyond the then-normal retirement age of sixty-five. Because his salary was his chief means of providing for his handicapped sons, he could not afford to make way for me as soon as we had both expected. After the university trustees granted the dean's request, I decided to take a leave of absence. I felt that the electrical engineering department was running smoothly and had reached the highest level possible in the context of the Penn State engineering school as it was then constituted. The Ordnance Research Laboratory was also running smoothly and was as active as 1949 defense spending permitted. Most important, I felt drawn to accept an invitation to become executive secretary of the Research and Development Board of the U.S. Department of Defense.

The RDB was established by the first secretary of Defense, James Forrestal, with my friend Vannevar Bush as chairman. The board consisted of the top civilian and uniformed research officials from the three services—in the case of the Navy, the assistant secretary for research and development and the chief of the Bureau of Aeronautics. Through the work of some twenty committees made up of industry and university representatives in research areas from aeronautics to electronics to lubricants, the RDB was supposed to coordinate the research and development of the three armed services.

Some know-how in Washington politics had come my way ever since World War II through my service as chairman of the Committee on Undersea Warfare (of the National Academy of Science) and the Naval Research Advisory Committee (NRAC). I learned a lot when my friend Jerry O'Donnell, now an admiral, asked Penn State to take over the administration of an Anti-Submarine Warfare laboratory for NATO at La Spezia, Italy. The purpose of this lab was to share technical information with our allies while not disclosing our Navy's top secrets. The Navy had selected a company headed by a Boston Brahmin to staff and equip the lab. But Admiral Jerry was concerned about this outfit's "regal" expense accounts and wanted Penn State to take over the contract. We did so, prompting the company's president to phone and blast me for "a helluva way to treat a classmate." This proper Bostonian turned out to be a 1932 alumnus of Harvard College, though our paths had never crossed, since he was ensconced in his Gold Coast club while I was slinging hash.

Another celebrity with whom I had sparred a bit before I moved to the Pentagon was Admiral Rickover. In a report prepared for the assistant secretary of the navy by NRAC, when I was chairman, we had the gall to suggest that the nuclear power plants designed under the admiral's direction, though sound, were lagging behind the state of the art. Rickover, as I recount in my profile of him, exploded at our intervention. Although this altercation did me no political harm, it did embarrass me a bit because I was not sure of my ground.

During most of my first months at the Research and Development Board, I was involved in dividing a shrinking pie. Under Louis Johnson, who succeeded Forrestal as secretary of Defense in 1949, budget cuts had to be spread around. Chairman Van Bush, as I note in my profile of him, wanted to cut costs by assigning programs to the most appropriate service: for instance, guided missiles to the Air Force. But Van and the RDB lost on that approach because none of the services wanted to give up anything, so usually a flat percentage of reduction was applied across the board.

Suddenly the Korean War erupted, in June 1950, and the whole Pentagon was engaged in expansion. General George Marshall became secretary of defense. One of his first acts was to appoint "Engine Charlie" Wilson to GM as "missile czar." Although Wilson stepped on many toes, he streamlined the program. At RDB our new mission was to promote, stimulate, and guide research and development programs. The military welcomed our promotion and stimulation, especially if these resulted in more money, but were less hospitable to our guidance when it led them too far from trails they had already blazed. Politically, the RDB committees endowed the military with the claim that they were consulting the best brains in the country, but even that could boomerang. When testifying before a Congressional subcommittee one gung-ho officer was asked whether he had consulted his RDB advisory committee about a pet project. He gave a firm yes. "And what did they say?" asked the subcommittee chairman. "They said to drop it," replied the officer in a startling burst of candor. He failed to get his appropriation.

When a pet project of an admiral or a general produced a "pork barrel" for a Congressman, we at RDB faced a potent dual alliance. On one occasion, both the Navy and the Air Force proposed expansion of their parachute-testing facilities. I saw no reason for this duplication and since the Navy's "rehabilitation" would be the costlier one, I red-lined that item in the budget request from DOD to Congress. The secretary of the Navy warned me of dire consequences. Soon I got an indignant phone call from the Congressman representing the district that contained the Navy facility, demanding to know whether I had held hearings on the matter. Quickly I summoned members of the appropriate advisory committee and notified the services of their right to give testimony. The committee backed me, but the Congressman complained that none of his constituents had been invited to testify. Although I naively wondered what his constituents could say about the optimal location for a testing facility, I soon found out. Everyone from the mayor of the nearest town to the owner of the local radio station, as well as a nearby rancher, testified to the importance of the Navy facility to the economy of the district. They also emphasized the salubrious environment of the district for the facility's staff. I was prepared to resist the partisans of the Navy test facility, including the Congressmen. But I was summoned by the new secretary of Defense, Mr. Lovett. "I am putting that line back in the budget," he told me. "Better to waste a few million bucks on that foolishness than to lose the entire military appropriation bill. We need that guy's vote, so he gets his test station." I learned a pungent lesson in pork-barrel politics.

Vested interests evolve within the military sector as well as between the military and civilian sectors. I encountered a spicy example in a project of the Army Quartermaster Corps to develop synthetic pepper. Since this struck me as either a boondoggle or a job for private industry, I summoned the project officer. An eager young lieutenant fresh out of West Point, he argued eloquently that imports of pepper could be and recently had been cut off and that GIs need it for their morale. Although I decided not to red-line the quarter of a million dollars earmarked for the project, I warned the shavetail: "I'll let it go this time, but this is the end. Finish your research!" A few days later, I lunched with Sir Charles Wright, naval scientific attaché at the British Embassy, and told him the story. "Good Lord!" he exclaimed. "I believe our navy is working on that. I'll send a signal to find out." A week later Sir Charles walked into my office, slapped a brown envelope on my desk, and said, "There it is, my lad: pepper project completed with all necessary formulations. The British navy had outdone the American Army." When I reported this finding to our Quartermaster lieutenant, he went away crestfallen. But he soon returned to announce triumphantly: "Sir, you don't understand. Because of the British class system, their research is no use to us. They've developed a white pepper, which is what their officer class prefers. But over here everyone prefers black pepper." For want of a better answer, I could say only, "Try putting a little ground-up bran in the British white pepper; and anyway, don't come back for more money." Ten years later, the U.S. Army was still working on a synthetic black pepper.

Van Bush had resigned as chairman of RDB because of his frustration over the armed forces' inefficient handling of defense cuts in 1947–48. My frustration mounted over the military's inefficiency in handling the defense build-up of 1950–51. In the flush of expanding budgets, the generals and admirals undertook more than they could pay for—too often in superfluous projects like redundant parachute-testing facilities or pepper research. Because there is a limit to military appropriations even in wartime—especially when the war is not altogether popular, like Truman's "police action" in Korea—and because most career officers are not trained R&D specialists, serious long-range research and development get short-changed. Every evening while I was at the Pentagon, I took home a stack of R&D progress reports looking for projects to red-line so that there would be dollars for more constructive projects. My efforts were fruitless for *political* reasons already sketched in broad outlines and completed in fuller detail in my profile of Van Bush.

So I decided to return to Penn State and take over the engineering deanship from the retiring Harry Hammond. The new president, Milton Eisenhower, elected by the trustees in January 1950, had formally invited me, confirming my understanding with Dean Hammond. I had had one poignant encounter with the dean during my two years at the Pentagon. The Army Quartermaster Corps had been planning a laboratory for R&D on food, clothing, and housekeeping supplies. The plan called for location at a university, like that of the Ordnance Research Laboratory. Universities were asked to submit proposals, and the RDB was asked to evaluate them. Penn State submitted an excellent proposal but was handicapped by its lack of a mainline rail connection, a railroad siding, and a major airport—all deemed crucial by the QM Corps. I tried to explain Penn State's virtually nonexistent chances when I saw Dean Hammond during my visits to ORL. But characteristically, when he had made up his mind, he did not hear me. He finally asked for an audience with the chairman of RDB. Since the chairman chose to be out of town on the appointed day, it fell to my lot as executive secretary to become acting chairman. I met Hammond in his unfamiliar role as a petitioner from my elevated role behind a desk bearing a sign proclaiming "Chairman." The dean acted as if I were a stranger. Again I tried unsuccessfully to explain Penn State's decisive transportation deficiencies. There was no *communication*; he talked at my desk, and my replies went past him. I never heard Dean Hammond's reaction to the announcement that the Boston area had been chosen for the Quartermaster Lab. Indeed, I never saw him again between our surreal encounter in Washington and his death in October 1953.

My five years as dean of Penn State's engineering school were as fulfilling as my two years in the Pentagon were unfulfilling. To be sure, I was wiser about the pitfalls of politics and administration than I had been before my hitch in the nation's capital. Penn State's new president, Milton Eisenhower, eleven years my senior, knew how to surmount most political and administrative obstacles. Under his leadership, Penn State was granted better state appropriations during both Republican and Democratic administrations. The physical plant was expanded and improved on the main campus and on six branch campuses. The quality of the faculty and staff was upgraded. Finally, Penn State was officially named a university while its schools, including engineering, were designated as colleges.

Taking over the deanship was made easy for me by Dean Hammond's small but efficient staff, which I retained: Miss McGee, secretary to the dean; Royal Gearhart, part-time assistant dean; and Royal's secretary. In my second year as dean, I put all engineering research into the teaching departments and dismantled the old Engineering Experiment Station. I encouraged the electrical engineering department to step up research on computers, with the result that Penn State was to become one of the first universities to build and operate its own electronic computer, PENNSTAC (Penn State Automatic Computer). I worked with Ken Holderman, director of engineering extension, to launch two-year, full-time programs in engineering technology, since the old one-year technical institutes no longer met the ends of either students or employers, nor did part-time study. In 1953 the trustees approved the degree of associate in engineering for the two-year programs, a novelty at the time. Engineering technology programs were an immediate and lasting success. Most exciting and glamorous was the construction of the university's nuclear reactor.

Penn State's reactor was a product of luck and daring. After V-J Day the mystery of nuclear bombs gave way to the idea of using nuclear fission to create electric power. Demand was developing for engineers to design and operate nuclear power plants. By good fortune, we landed William Breazeale of the Oak Ridge National Laboratory to head a nuclear engineering department. Bill almost immediately began to convince me that we needed a reactor. Since I knew little about the field, he undertook to educate me. Quickly we found allies: physicists who were experimenting with radioactivity; plant geneticists who wanted to radiate seeds; materials researchers who wanted more refined tests. Bill assured me that a swimming-pool-type reactor presented no health hazards; he had designed and supervised the construction of such a reactor at Oak Ridge. What about cost? Well, Bill said, if we could design the reactor ourselves, if our architecture department could design the building, if we could build the controls in our own shop and use as much student help as possible, then the cost would be "only" about $208,000. Although my instinct is always to double a researcher's estimate of cost, I could find no flaws in Bill's estimate insofar as I was competent to judge it. But even such a modest amount is not to be found in a college dean's discretionary funds.

Then came a second stroke of good fortune. President Milton Eisenhower called me to his office to ask my confidential advice about using an unexpected fund of a quarter-million dollars. It was a surplus from operation of the university dining halls. Refunding it would be prohibitively complicated, and it was a one-time windfall because board bills were to be adjusted to avoid regular surplus. He wanted to propose to the trustees that the fund be spent on a project of benefit to the entire university. "And I want it to be visible, even spectacular," he added. To myself I said, "Thank my lucky stars"; to our prexy I said, "I've got just the thing." Selling him was a cinch, selling the trustees was no harder. The board had been yearning for something that would put Penn State out in front, something they would read about in the newspapers, something they could brag about.

Construction of the reactor began in May 1953. We explained to the university community and the townspeople of State College what Bill Breazeale had told me about the negligible risk in this kind of reactor, and no one doubted us. We built our reactor within budget and on schedule. There was one hitch: We needed enriched uranium to fuel the reactor, and the federal Atomic Energy Commission owned it all. Would the Commission provide some to a university? Of course, we had kept the AEC informed about what we were doing. I had had many dealings with AEC chairman Lewis Strauss during my Pentagon days, and one of his assistants was a friend of mine. McKay Donkin had become my friend when he was one of my senior staffers at RDB during the first two years of the Korean War, and he was later to join me as Penn State's vice president for finance after I had been elected president. Chairman Strauss approved our project, especially since it was in harmony with the "Atoms for Peace" program of the nation's new president, Dwight D. Eisenhower. But no one had ever pushed the AEC around or hurried it up.

Now came a third stroke of good fortune; President Ike accepted younger brother Milton's invitation to give the June 1955 commencement address at Penn State. Obviously, he should dedicate our reactor! Arranging a ribbon-cutting was easy, but we could not arrange for him to press the starting button, since we still had not been promised a fuel delivery. We reluctantly settled for a "symbolic button." The chief executive arrived by train, probably the last passenger train on the old Bellefonte Central Railroad, accompanied by favored trustees, Secret Service agents, and a corps of journalists and photographers. Bill Beazeale and I were impressed by Ike's genuine interest in the details of the reactor's construction and operation. At the dedication he asked, "Where is the button I press to start this thing?'' ''Mr. President,'' I blurted out, without thinking of the possible consequences, ''the button you have to press is in Washington.'' Ike looked puzzled and asked, ''What do you mean?'' Milton, my boss, looked somewhat annoyed and apprehensive. The journalists reached for their pencils.

Headlines in nearby papers announced: ''Engineering Dean Tells President to Push Button in Washington.'' Ike took the whole incident as fun, a few days later two big trucks arrived with the necessary uranium. Other problems remained to be solved, since the AEC staff had been preparing regulations for the first nongovernmental nuclear installation during the thirteen years since the Manhattan Project had produced the world's first self-sustaining nuclear reaction. Among other things, we were required to have a licensed operator for a swimming-pool reactor. Bill Breazeale ended up preparing and grading his own licensing examination. For many years Penn State was a center for the training of reactor operators.

Thus our university became the first institution of higher learning to have its own nuclear reactor. North Carolina State became the second, although its project had started a little before ours. Our reactor, with its output of 100 kilowatts, has served a host of faculty researchers with short-lived isotopes, beams of neutrons, radiation-damaged material, and various kinds of neutron flux.

President Ike asked me to chair a committee on research for small industry. One of the vice chairmen was Charles Kimball, president of the Midwest Research Institute. Since Charlie had been a member of our study group in Pierce Hall during Harvard graduate school days, I was pleased to be associated with him again. Although the committee's report was issued at a meeting addressed by the president with much fanfare, its useful ideas never got the currency they deserved. Belatedly, I learned the vital importance of merchandising with respect to reports, including research reports. Producing a report is just the beginning. Market research is needed to identify its audiences, advertising to reach them, order processing to get the report into those audiences' hands, and follow-up to make sure the whole set-up is working right. I served also as vice chairman of a presidential committee on scientists and engineers. Again, I fear that our merchandising was deficient and that the report is gathering dust.

My relations with the Eisenhower brothers was a source of pleasure and pride for me. As university president, Milton gave me a number of special assignments during my first four years as dean of engineering.

But he often gave me no formal authority to carry out his assignments, so I inevitably offended members of the staff and faculty, especially other deans. Finally, in late 1955, Milton told me he was going to recommend to the trustees that I be appointed vice president for research. When this impending appointment was announced, some of the science professors questioned my understanding of research in view of my engineering background. As for the liberal arts faculty, I suppose an "engineer," to many of them, was bound to be a barbarian. One English professor even wrote to Harvard to inquire whether I had the degrees I claimed. In May 1956, shortly before I was to start my vice presidential duties, Milton dropped a bombshell at a trustees' meeting by resigning.

I was quite content with my job as dean of engineering at Penn State and with my life in the town of State College. Our daughter and son were doing well in the local schools and appeared to be happy. My wife had turned our house into a home with her elegant decorating, and she seemed to be as comfortable there as I was. Most of my friends were nearby. My work was interesting and challenging, without making excessive demands. I knew what was required of me and felt that I was performing acceptably. On the national scene, I was keeping up my contacts in engineering, scientific, and governmental circles. This explanation of my outlook when I was in my early forties is offered for those of my admirers who feel I was not ambitious enough, as well as for those of my detractors who feel I was too ambitious. During this period, as it happened, I turned down or discouraged a number of flattering offers or overtures. These included the presidency of Georgia Tech and the vice presidency for research at IBM. Clearly, some potential employers considered me upwardly mobile; equally clearly, I was not "bucking" for advancement.

Milton Eisenhower, meanwhile, was increasingly pulled toward the White House. He had commuted there almost every weekend for more than three years since Ike's inauguration; he had a suite of offices in the old State Department building, and he had served as a special ambassador to Latin America. The two brothers had always been close friends, and their closeness grew after Milton's beloved wife, Helen, died in 1954 and Ike suffered a heart attack in 1955. Milton resigned from Penn State, clearly because he wanted to be near Ike and because he felt his effectiveness on our campus was lessened by the pull of Washington. The offer from Johns Hopkins to become president of that university came in July, two months after Milton had resigned from Penn State. He had turned down the presidency of Johns Hopkins a year earlier. Incidentally, when Milton died in 1985, he was buried in State College, Pennsylvania, beside Helen.

A telephone call in early July 1956, a few weeks after Milton Eisenhower's resignation, informed me that I had been elected president of Penn State. If the offer had come on a day when I was mentally down, I believe I might have refused it. Not only did I feel comfortable in my post as a dean and research administrator, and qualified to hold it, but I also felt doubt about my qualifications for the presidency and foresaw much discomfort in accepting it. But the call came on an early summer Saturday, when I was in a euphoric mood and was surrounded by my supportive family. So my answer was yes.

Discomfort came immediately. When my election was announced, strong opposition followed on the heels of congratulatory messages and reporters' interviews. Although the trustees' procedure in electing me was flattering, it made things tough for me. The board of trustees had broken precedent by skipping consultation with the faculty and by choosing the first president from within the university since 1866. I assume that the trustees had two motivations. First, I suppose they wanted to ease the shock of announcing Milton's sudden resignation by pairing it with the announcement of his successor's election. Second, they knew that Milton, besides nominating me as vice president for research, had relied on me to help him with many of the university's nonacademic tasks, such as negotiating with technical and service employees. The trustees knew these things, but the faculty did not. Furthermore, because I am an engineer, the scientists and social scientists doubted my understanding of research, while professors of the fine and liberal arts questioned my credentials as a humanist.

For my part, although I understood the corridors of power in Washington, I did not understand them in Harrisburg, Pennsylvania's capital. Nor was I acquainted with political relations among universities and their presidents. My only contact with Penn State's trustees had been to sell them on the nuclear reactor, since they had been sold on the Ordnance Research Laboratory by President Hetzel and Samuel Hostetter, the university's chief financial officer since 1935. I seriously doubted the depth of my political know-how for the role I had accepted. Unfortunately, Hostetter was to retire in July, the month I was elected, but I persuaded him to stay on for another year. Fortunately, too, Milton Eisenhower was not to start at Johns Hopkins until October 1956, so I had the whole summer to learn the ropes from him, to think about the job, to seek other advice, and to make plans.

Advice was readily available. Three of my wartime colleagues in research had become university presidents: Gaylord Harnwell at the University of Pennsylvania; Lee Dubridge at Cal Tech; and Bill Houston at Rice. Vannevar Bush, a wise mentor, contributed this advice: "Let me tell you, young fellow, there are three ways to build a famous university. You can put together a distinguished faculty, as they are doing at Stanford. You can build a lot of buildings, as they are at Illinois. Or you can get an unbeaten football team like Michigan State's." I told Van I was going to do all three—a little presumptuous, but that was my secret hope.

During my summer as president-elect of Penn State, I sometimes ruminated on how my life might have differed if I had taken the $100.00 scholarship to that institution instead of the $500.00 scholarship to Harvard. Let me sort out some of my ruminations under four convenient headings. I believe Harvard may have given me an edge in standing and stimulation, but I believe Penn State would have enhanced my sense of participation and partnership. Unquestionably, Harvard degrees have high standing, though I argue elsewhere that they do not rate as high as they once did. Harvard degrees certainly weigh heavily in the academic world, where I have spent most of my life; in some government departments such as State; and in some industries such as publishing. The reader should note, however, that the two latest presidents of Penn State are graduates of state universities; that Penn Staters are under no disadvantage in agriculture and dominate the Weather Bureau; and that the president of the world's largest publishing company, Doubleday, is an alumnus of Penn State. Stimulation is a hallmark of Harvard, but I have raised the question in other chapters of whether Harvard's stimulation is not overly focused on achievement of "right answers" or the "school solution." Penn State might have given me more scope to find my own way in engineering. Assuredly, I would have had more time and opportunity at Penn State to participate in extracurricular activities such as debate, track and field sports, and the campus newspaper. One of my challenges would have been to resist the temptation to neglect my studies in favor of more exciting things. There is no gainsaying the existence of a network of Harvard graduates with great influence. Until recent years, however, this network was heavily concentrated in the Boston area. One's standing as a Harvard alum, moreover, is related to one's overall "social" standing.

My social disadvantage was compounded by Harvard's abandonment of my undergraduate engineering school. Penn State alumni, by contrast, are on a social par and treat each other as partners. They are increasingly widespread around the globe, and a large number are engineers. Of course, speculation about what might have been is fruitless if it arouses only self-pity or self-satisfaction. But it can be useful as a guide to action, and it did affect my behavior as I took the helm at Penn State.

brief occupation of Old Main by a few dozen students. Six more students were arrested for disorderly conduct.

After the Kent State tragedy, I cancelled classes in memory of the slain students, and several thousand students held a peaceful march. After polling the students on May 11–12, the Undergraduate Student Government recommended that seminars and workshops on "relevant" topics might be substituted for regular classes till the end of the spring term. The university senate in effect approved the substitution. But what about the thirty-nine students who had been charged by the university with breaking the law during the "unrest"? Some do-gooders urged that the students be granted amnesty so that they would not have criminal records. I disagreed because I believed their behavior was criminal. On the advice of counsel, the university trustees appointed an investigative commission of three prominent attorneys: Robert E. Woodside, a former judge; Genevieve Blatt, a former state cabinet member; and William T. Coleman, later a member of President Ford's cabinet. I accepted the stricter recommendation of the first two commissioners, calling for expulsion of seven students, suspension for five, probation for nineteen, and dismissal of charges against eight. Seventeen of the students filed suit in U.S. district court to have their status restored as students in good standing. In September 1970, the judge ruled in favor of the university.

Student activism at Penn State evaporated after the spring of 1970 and began to do so nationally. U.S. involvement in Vietnam, which had triggered the unrest, did not end for three years. But student draft deferments were not terminated, and the armed forces adopted an all-volunteer policy in 1972. Other student objectives—liberalized parietal rules, participation in academic governance, outreach to blacks and other minorities—were achieved and would have been so anyway without the demonstrations because "their time had come." Did "activism" speed the pace of legitimate reform? I doubt that it did so significantly, but I admit the question is arguable. Where I differ with my "bleeding-heart" friends is on the cost of the unrest and the propriety of my response to it.

Addressing the university senate in March 1969, I said: "Dissent is one of our most precious rights and is basic to the system we have developed for living together. But confusing dissent with disruption not only cheapens it, but threatens the system itself." That is especially true when the disruption is caused by a small minority, abetted by outside agitators, as I am sure was the case at Penn State. Discussion is obviously healthy, but you cannot discuss "nonnegotiable demands." I think making that point has educational value. Indeed, there is educational value in disabusing students of the notion that talk is the solution for everything. Significantly, few if any members of the "Free Speech Movement" came from the colleges of agriculture, business, engineering, or mineral sciences. Students with clear vocational objectives know that talk can be simply "bull." I differ with colleagues who objected to my use of court injunctions and state police intervention, on the ground that I should not have "treated students like adults." At the heart of education is the concept that everyone must take responsibility for his actions, including criminal actions.

What has become of the activists of the sixties? Some have become professional agitators, and some have done nothing. A few, especially the nonviolent ones, have become respectable and even successful—though none in engineering, to my knowledge. To some extent, as I have said, I believe these students suffered from a generalized discontent, a sense of lack of purpose, and a feeling of inner emptiness. I confess to a shortage of sympathy with that psychological state because it is so alien to my experience and because it can be so harmful. When I run into one of the former activists and he slaps me on the back while prattling about the good old days of the sixties, I cannot forget the wasted time, the expense, and the disruption of the university's progress to which these one-time "idealists" contributed. But I remain civil and may in time even become sympathetic.

I did not retire from Penn State in 1970 because of campus unrest, contrary to widespread mythology. I never run away from difficulties, and the worst difficulties were over when I stepped down. When I accepted the presidency in 1956, I told the trustees that a decade would be long enough for me. At the end of ten years I felt the same way, but I was urged by the trustees to stay on. This process was repeated through my fourteenth year. I felt that I had given my best and that the university needed a change of command. I also felt that I needed a change, and I wanted to try my wings in the world of business and industry, as I have explained in my vignette on McKay Donkin. For most of us, moreover, every job develops some staleness after some period. To a degree, I felt that I was going around the same old circle at Penn State, trying to make people understand what they did not wish to understand. I admit that my mood was affected by a change in the national attitude from optimism to pessimism after the bright day in the fifties when I became a university president.

But I left my presidency of Penn State with a feeling of accomplishment, which I have not lost when I look back. I believe I met the boast I made to Van Bush: that I would preside over a winning football team, a distinguished faculty, and a lot of buildings. At my retirement dinner, dean Ben Euwema cracked, "Eric has an edifice complex." Okay, so I do—with the addendum that my urge is to build intangible structures as well as tangible ones.

PENN STATE'S POLITICAL FEATURES
Boosters and Bellyachers

The Pennsylvania State College (later University) was founded in 1855 as a state-related, not state-administered, institution. My autobiography is not the place to spell out all the consequences of the university's political position. (These are ably set forth in Michael Bezilla's *Penn State: An Illustrated History*.) But two points are crucial to my story. One is that the university has its own "legislature"—a board of trustees with a membership defined in the institutional charter. The other is that the university must go to the state government for much of its funding. Contrary to mythology, total receipts from Penn State's Federal land grant amounted to $439,000, a tidy sum in 1867 but hardly an admission to Easy Street. The bulk of the university's funds come from governmental and industrial research grants and from student fees. While there is a modest but growing endowment, the state must fund about a third of the university's budget. Thus the political context is a constant concern. The university's trustees, as defined by the charter and its amendments, represent organized agriculture, business, labor, and the alumni—along with the university president and the state governor, superintendent of public instruction, and secretary of agriculture. University micropolitics is interwoven with Pennsylvania macropolitics. As president, I had to be attentive to both the warp and the woof of this political pattern.

Happily, I had gained some political savvy from my years of government service. In addition, I had persuaded McKay Donkin to leave Washington so that I could benefit from his canny political counsel at Penn State. On the board of trustees, my chief ally was Roger W. "Cappy" Rowland, a true-blue university booster who was to become board president (1963–70). In the state government, my foremost ally was A. J. Caruso, executive director of the General State Authority, known to all Pennsylvanians—usually with affection—as "Karoose." Before I pay tribute to Cappy and Karoose as outstanding boosters, let me dispose of one typical and unnamed bellyacher.

A thug in Brooks Brothers clothing—that is how I remember my unfavorite trustee. Portly and pompous, he was a business executive in charge of the least successful division of a corporation destined for bankruptcy. Educated to be an engineer, he took a nonengineering job because

MCKAY DONKIN
My Unlikely Friend and Ally

When I first met him, I did not like him. I was first advised to hire him; I did not want to. In many ways I never really knew him. Yet he became my most trusted deputy and, for a number of years, my closest friend. The man was the late McKay Donkin, who served as my assistant at the Pentagon during the Korean War and as a key vice president during my presidency of Penn State.

My initial antipathy to McKay arose from a clash in our styles. My way is the engineering way: applying force in the most advantageous manner by means of leverage. Detractors consider my style rough and crass. McKay's way was the diplomatic way: knowing the other party to a transaction and taking advantage of his psychological qualities, whether strengths or weaknesses. Detractors consider McKay's style manipulative if not sneaky. Contrasts between our styles are heightened by the engineer's propensity to act openly and the diplomat's tendency to act in secret. Of course, a prudent engineer will learn when to keep his mouth shut, while a wise diplomat will know when to tell the whole story.

Diplomats throughout history have gained a reputation for being untrustworthy, and certainly a number of them have earned the stigma. I have concluded, however, that diplomats are not necessarily less trustworthy than engineers—or, for that matter, scientists, artists, entrepreneurs, or politicians. Trust, I believe, arises from loyalty, whether to a person, an institution, or an idea. McKay Donkin was loyal to me and to our country and, later, to Penn State and to the idea behind that public institution for higher education and research. McKay therefore quickly gained my trust and kept it until his untimely death in 1968 at the age of sixty-four.

Bill Webster, my boss at the Pentagon in 1950, told me to hire McKay. When I asked why, Bill said, "Well, he works for Lewis Strauss at the Atomic Energy Commission, and Lewis says the guy really knows his way around Washington. We need someone like that to tell us what's going on in the other agencies round town."

At this point we were trying to turn things around in the Department of Defense from a planned reduction in strength following World War

II to a rapid expansion in order to fight the Korean war. Bill Webster was chairman of the DOD Research and Development Board, and I was executive secretary. Unfortunately, Bill lacked the political clout of the two previous chairmen, Van Bush and Jim Killian, who had close personal associations with Presidents Roosevelt and Truman, respectively. And, certainly, I had no back door to the White House.

The mission of the Board was to monitor the research and development capability of the Department of Defense. We had two objections: on one hand, to avoid wasteful overlapping among projects of the Army, Navy, and Air Force; on the other hand, to effectuate projects that no one of the three services found exciting. Our staff of three hundred drew up situation reports intended to enlighten the Secretary of Defense, other members of the cabinet, and ultimately the president. Our aim was to match the armed forces' present and future needs with the nation's capability to meet those needs. Doing so required know-how about what was going on elsewhere in the government, as well as on the campuses and in industry. A score of committees in the academic and industrial sectors gave us input. But how would we get input from the governmental sector? That is where McKay Donkin—or was it Duncan McKay? (my boss was not sure)— came into the picture.

I had known my boss, Bill Webster, since the thirties. We had met when I was working at Tufts and the Doble Corporation and had served as a consultant to the New England Power Company on its transformer-insulation problems. Bill had risen to the presidency of the power company long before becoming chairman of the DOD Research and Development Board. When he hired me as executive secretary, meaning his chief of staff, he told me he was going to be Mr. Outside and I would be Mr. Inside. Both inside and outside operations, we soon learned, required know-how about the governmental environment.

Washington was in an agitated state of flux when the United States spearheaded the United Nations' "police action" in Korea. Civil servants —reflecting the uncertainties of politicians and the citizenry—were rushing around, expanding their programs, and making great plans, all with no clear sense of direction. Every agency was watching every other one, because we all wanted to know whether we were running with the pack, falling behind, or perhaps pursuing cold trails.

At the R&D Board, we could certainly make good use of information about other governmental activities. We never knew what Selective Service was planning or even what our parent, the Department of Defense, was trying to do. We had many questions focused on the Atomic Energy Commission and Congress's joint committee on atomic energy. Would we or would we not use the atom bomb in Korea, for instance?

Rumors abounded about the man my boss had tagged to be our central intelligence agent. One report was that he was independently rich, having made zillions in oil, and he did not need any job. Less friendly gossip cast him as a dilettante, with more interest in championship bridge tournaments than in his work. According to the most destructive rumor, he had suffered a nervous breakdown and was fighting his way back to mental health. McKay's reserve provided grist for the rumor mill. No one knew anything about him—his family background, his education, or his previous employment—except that he obviously felt at ease in the corridors of wealth and power.

The actual McKay Donkin proved to be small, intense, alert, and impeccably groomed. He wore a Brooks Brothers suit with a gold watch chain across his vest. He was short, lean, wiry, and personable. His actions were quick and abrupt, almost birdlike, as he frequently extracted and examined his gold watch. But his most noticeable feature was the way he used his bright, coal-black eyes, darting from one visual detail to another and taking them all in. I soon learned that his other senses were equally developed; also, he not only took in every datum but also recorded it in the digital computer in his mind.

McKay's mind was scary. It was too fast, too searching, too all encompassing for most mortals. When one asked him a question, one knew he had formulated an answer before the question was half asked. And his answer covered not just that question but also three or four logical corollaries. To most of us, McKay's mental processes were terrifying at first, and some individuals never recovered from the terror. As his prospective superior, I was put off also by his penchant for making organizational short circuits. If he could bypass several levels of the organizational chart when seeking information, why could he not bypass me? The answer lay in McKay's loyalty. When I found that he was ready to give his loyalty to the Board and its activities and to me, I realized that I could give him my full trust.

At the same time I understood why McKay Donkin was the subject of never-ending rumors. The reason was not his reserve alone but his whole personality. He had the ability to go to the heart of consequential matters, and he liked to use that ability. For that reason he was impatient with chitchat. Because of his self-confidence, he felt at ease with the high and mighty. He therefore had no need for the trappings of his position. All he wanted was a desk, reasonably close to the chairman's office and preferably with access through a back door. He said he did not need a secretary, a typewriter, or an adding machine. "If I am going to be your ear-to-the-ground department," he told Bill and me, "All I need is a pencil and a note pad."

Chairman Webster gave McKay his first assignment for the R&D Board: "Find out from Selective Service and the assistant secretary for personnel what they are going to do about scientists and engineers in the draft." We did not see much of McKay for the next few weeks, but we got regular reports on our agent's activities from our chairman in the course of staff meetings: "McKay reports that, since the Joint Chiefs predict a long war, we can expect . . ." Although I was not yet fully sold on the value of McKay's function, I somewhat reluctantly agreed that such input was useful. As the war went on, I became increasingly sold on the importance of McKay's reports about troop strengths, oil supply levels, or departmental vacancies.

Gradually we took the mysterious Mr. Donkin into the inner circle of the RDB management. We had to, because he knew so much that we needed to know. He attended the program directors' meetings, not sitting at the directors' table but at the back against the wall. He never volunteered any information, but, when he was asked a question, he either answered it directly and responsively or said, "I don't know the answer, but I will find out." Then he made a note, and soon the questioner would get an neat, penciled memo from McKay giving the desired information.

Finally, at my request, we invited Mckay to one of our "prebriefing sessions." These were conclaves where the staff prepared to confront departmental big shots at plenary sessions of the Board. Present, along with me and my deputy, a time-serving bureaucrat, were the program directors, the public information officer, the presentation coach, the speechwriter, and the legal counsel. The prebriefing sessions seemed to have two conflicting objectives: On the one hand, to make staff presentations clear, literate, and unambiguous; on the other hand, to make them high flown and prudential. Soon after my arrival at the Pentagon, I decided that these prebriefing sessions were a waste of time. Matters of substance may at times be worked out in brainstorming conclaves but not matters of presentation. My alternative solution was to ask each staff member to give appropriate input, and our speechwriter would pull it all ogether. My motive in inviting McKay was to get his reaction to my olution.

We had the talent for stunning presentations. Our program directors knew their stuff. Our public information officer, Paul Beall, a former Penn Stater, was a whiz at gimmicks. When one of our staffers was to make a presentation on germ warfare to the Joint Chiefs, plus DOD big wigs and key Congressional members, Paul coached him to carry a bright red fountain pen in the pocket of his white shirt and to place it on the podium during his talk. At his conclusion, he was to pick up the pen and say, "If this had been a capsule containing botulism germs and I had been inoculated, I would now be lecturing in a morgue." Offsetting Paul's flamboyance was our legal counsel, Jeff Korman, a canny Brooklynite. Jeff was the warden who kept us from saying "musts," since "should" was all our advisory role would permit. Mediating between our counsel's discretion and our publicist's audacity was our speechwriter, Leanna Embry. Leanna looked like a New England schoolteacher, and she acted the part when she stood her ground on a point of usage. My deputy added nothing to the group except a penchant for bureaucratic gobbledygook.

The format of a prebriefing session, which I inherited, resembled that of a Quaker meeting. We all sat in a circle and tried to reach consensus. No one spoke until the spirit moved him. This procedure, as I have suggested, is fine for certain substantive matters, but it is no way to prepare a presentation. I wanted to see whether McKay Donkin shared my perception. Of course, no one at the session asked him a question, since information-seeking was not the order of the day. When I was on the verge of nodding off, I caught the bored look on McKay's face. That confirmed my decision to phase out the institution of RDB prebriefing sessions.

By now I could read McKay's body language. His signs of interest or boredom, though subtle, were clear. Subtler but equally clear to me were his signs of approval or disapproval: little grins or twinkles in the eyes on the positive side, little frowns or shrugs on the negative. Invariably, his judgments were sound. When I saw that McKay disagreed with me, I knew I should reexamine the situation and probably alter my course. Rarely did McKay's disapproval call for a complete change of course, but it usually suggested corrections.

Because of our close association at the Pentagon, I soon learned the basic facts about McKay Donkin's life. In many ways our experiences had been similar. Like me, he had been a poor boy and had earned his way through college. Both of us had experienced the Great Depression in our youths, since McKay was born in 1904 and graduated from the Colorado School of Mines in 1927. Thus he was six years my senior but only five years ahead of me in becoming an engineer. Later, during World War II, we both did Navy service, he as a lieutenant and I as a civilian researcher. Unlike me, McKay never taught. But we both worked as engineers for colorful entrepreneurial characters. In his case, it was a Colonel Pomphrey, an oil wildcatter who sent McKay on prospecting missions in Alberta and Texas. Here the resemblance between the two of us stopped, since McKay became independently "well off" by writing his own oil leases in Texas. Being well off enabled McKay to pursue a "diplomatic" career in Washington rather than to work as an engineer. If I had become independently well to do at an early age, might I have taken the course that McKay took? Obviously I cannot be sure, but I doubt it. Diplomats, like engineers, are born and not made, according to my observations, though circumstances shape the particular outlines of their careers.

The Pentagon was not a happy place in 1951. General MacArthur was fighting not only the North Koreans and Chinese but also his commander-in-chief. In the end he got himself fired by President Truman. By April the war was at a stalemate. Many Americans were disgruntled both about the conduct of the war and the United Nations' role in it. Our military leaders were scrapping among themselves and with contractors supplying the military. Bill Webster and I saw no chance for the R&D Board to become effective, so he was preparing to return to the New England Power Company and I to Penn State.

During my last months at RDB, McKay Donkin gave me a striking demonstration of his presence by predicting the failure of my pet project. My brainchild was a fund whereby RDB could initiate promising programs for the Department of Defense when no one of the three services wanted to sponsor them. Such a fund, I felt, would give RDB the clout that Van Bush, its first chairman, had envisioned. Despite Chairman Webster's skepticism, Congress appropriated $50 million for the fund and Secretary Lovett called it a "great idea." But McKay said dolefully, "I wouldn't start the paperwork to spend the money until you have Lovett's signature. Wait until the services light their backfires." As McKay predicted, the R&D people at the Army, Navy, and Air Force put the heat on the secretary of defense. Lovett did not sign the authorization for an independent research and development budget at RDB. And Webster and I did not reconsider our decision to leave. When we left. McKay returned to the Atomic Energy Commission.

Although I saw McKay only a few times during my five years as dean of engineering at Penn State, his razor-sharp perceptions of human interactions were frequently on my mind. We met occasionally during this period on official business and once hunting ducks. Then, in the summer of 1956, I found myself in the role of university president-elect with three months to put together my staff. Most of my staff, especially the new members I recruited, were fairly close friends of mine. I make no apology for that fact. Of course, I chose them primarily for their competence, but an important secondary consideration was that I liked them. *Like* is not quite the right word; the important point is that I had confidence in them. My confidence was not merely of a negative sort—knowing they would not let me down, although that is important. My confidence was also a positive sense that 99 percent of the time they would know how I would handle a problem and, in the other 1 percent of cases, they would come to me for my input. When an executive has that kind of confidence in his staff, it makes not only for tranquility but also for efficiency.

Competence and confidence are the basic criteria in recruiting a staff, but I soon realized I was not getting enough nay-sayers. I felt a need for some internal critics, persons capable of cold, unbiased analysis. Although all of my appointees had held high-level jobs before, I got a feeling they were overly awed by the office of the presidency (hardly by me personally!).

One exception was Samuel K. Hostetter, who had been, in effect, Penn State's treasurer, controller, and business manager (under various titles) for twenty-one years under three presidents. Sam was generally regarded as the executive officer of the university, and he certainly was not awed by the presidency. Moreover, since he knew every operational detail and every rule and precedent under which the institution functioned, he was willing and able to say no on appropriate occasions. But Sam was beyond official retirement age and had consented to stay on the job during my first year, at my urgent request.

One of my first decisions was to separate the three functions that Sam Hostetter had been performing. Filling the posts of business manager and controller was relatively easy, but finding the right treasurer was a challenge. The university needed a healthy cash flow to support our constantly growing teaching and research needs. We also needed capital for expansion of our plant and equipment. Our sources of funds were diverse, including legislative appropriations, bond issues, short-term borrowings, federal grants, research contracts, and gifts, as well as student fees. Our treasurer must be as much an information analyst and a diplomat as a financial technician. More and more I thought of McKay Donkin as the right person. He had handled big money for his own ventures and for his entrepreneurial boss. He knew how to gather and analyze information. He could conduct diplomatic negotiations with anybody at any level. And, best of all, he was not afraid to be an internal critic and nay-sayer.

Could I persuade McKay and his wife to leave the glamour of Washington for the backwoods of central Pennsylvania? I doubted it, but I had two aces to play: his love of small-game hunting and his love of intellectual challenge. I had no trouble convincing McKay of the good hunting in our part of Appalachia; I emphasized the job's challenge by changing the title from treasurer to vice president for finance. To my pleasant surprise, McKay accepted—initially for one year but actually for the remaining eleven years of his life. The question of extending his appointment was never raised; he simply stuck with us out of loyalty to me and the university and to the idea behind our objectives.

When McKay Donkin took charge of Penn State's finances in 1957, he was unfamiliar with some devices such as bond issues. Fortunately, he had an excellent tutor in Charlie Oaks, one of our trustees and chairman of the board of the Pennsylvania Power and Light Company. McKay learned fast, as he always did under pressure. The pressure was intense, especially to build much-needed dormitories, funded through multimillion-dollar bond issues.

McKay not only learned the tricks of his adopted trade but devised some new ones. One of his tricks put the university in the money-lending business. As an "instrument of the state," the university could take out loans on which the lenders were not required to pay taxes on earned interest. McKay saw that we could use some of our "cheap" borrowed money to make profitable loans to private borrowers. In effect, we could borrow at four percent and lend at eight percent. Legislators soon closed this tax loophole. It was one instance among many of McKay's ingenuity in keeping our cash flowing when state appropriations were late and the federal government was slow in reimbursing our research expenses.

Legends about McKay's acumen soon multiplied. As he settled into our community, he dropped much of the reserve that he had cultivated on the perilous banks of the Potomac. He became known for his wry wit and pungent observations. I learned much about my friend's character when we teamed up for doubles games on the university tennis course or went on outings with our informal hunting and fishing club. He was a fierce competitor and loved to win, but he had the good grace to show restraint in victory. McKay had the reputation of being a killer, but I found him relentless only in his pursuit of small game, especially birds. Otherwise his philosophy seemed to be "take what you want, by force if necessary, but never destroy your opponent."

My first encounter with McKay's diplomatic restraint arose during our Pentagon days. I wanted to fire an assistant for incompetence. Although the man had an honorable record in the armed forces and the civil service, he simply was not doing the job. McKay's advice was to "move him to the end of the corridor," where he would have no responsibility but could be useful for his experience with government machinery. "Besides," added McKay, "If the outs became the ins, he might become important."

Once we were trying to buy a farm for the university, and the owner kept trying to bid up what we considered fair offers. When we thought $80,000 was fair, he asked for $90,000. Next year, whenever we offered $100,000, he wanted $106,000. Suddenly the owner offered to sell for $90,000. By discreet inquiry, McKay discovered that the man was over-extended and needed cash desperately. My reaction: "Tell him our offer now is $60,000." McKay counselled against my hard line of advising, "Give him his $90,000." When I asked why, he replied, "Never break the peasant's rice bowl." Then, responding to my raised eyebrow, he explained, "It's an old Chinese proverb. If you break his rice bowl, he has no means to feed himself or even to beg."

Occasionally, McKay went trout-fishing with fellow club members but never deer-hunting. Trout-fishing strained his patience, and deer-hunting exceeded it. The frenetic activity of bird-shooting suited his supercharged personality. McKay missed some great moments. The trout steams in Spruce Creek Valley have attracted presidents from Teddy Roosevelt to Jimmy Carter. One of our members, Bob Harpster, owned a fishing camp there, while another member, George Harvey, taught fly casting at the university. Don Kepler, a great all-around woodsman in our club, helped me bag my first deer. We had camped at Bear Hollow for several days without seeing any deer except does, which were out of season. On one glorious December day, Don promised me that I would get a buck by doing some "still hunting." He posted me and Ernie McCoy, dean of physical education and club founder, at two well-used deer crossings. After waiting for several hours, I pulled a paperback from pocket and started reading. I became so immersed in the whodunit that I almost missed a rustle in the mountain laurel. There I beheld a magnificent head topped by a rack of antlers. Before I could pick up my gun, the buck withdrew into the laurel. Shortly he reappeared and galloped in my direction. I feared the onset of buck fever, but I squeezed the trigger in a textbook manner. Down came my first deer, and then my buck fever hit. When I recovered from the shakes, I started running toward Don, my mentor. Ernie later reported that I passed him without seeing him: "Hell your feet weren't touching the ground!" Ed Czekaj, a member from our athletic department and a superb cook, dished up a celebratory feast.

Unlike deer-hunting, bird-shooting suited McKay Donkin's restless and competitive nature. His reputation soon spread within our circle of friends at Penn State. This elegant gentleman from the nation's capital was no mere avid hunter; he was always out for the biggest bag. Two things supported his popularity in our shirt sleeve club: One was our enjoyment of McKay's high spirits, even when he got a little carried away; the other was our realization that we all did better when McKay was in the group. When he was with us, we knew we would have plenty of shooting and would bag an above-average amount of game.

One of our fall sports was dove-hunting. Doves are hard to hit. They seem to tease you. A huge flock will fly right at you and, just as you pull the trigger, veer to one side. Predators are more skilled at stealing doves' eggs or their young than hunters are at shooting them. That's why the dove population stays high in central Pennsylvania.

McKay was determined to get his share, or more, of doves. During his first autumn in the club, we gathered in a cornfield that Don, our chief scout, had chosen. Most of us had shotguns inherited from our fathers or purchased in chain shores or second-hand shops. But McKay appeared with a beautifully engraved and carved shooting arm that must have been made for a duke. Somehow he had managed to get a corner position where he could get doves coming both ways, though there was a large pine tree about thirty yards in front of him that interfered with his view. Soon the shots from McKay's position came so thick and fast that we might have been next to an army regiment on maneuvers. Most of us got our limits, including McKay, but he had fired many more shells per bird than the rest of us. When we kidded him a bit about his high expenditure of ammunition, he blamed it on the tree in his line of sight. Don stated, however, that the doves used the tree for cover. When we returned to the field a year later, the pine tree was gone. No one doubted that McKay had something to do with the tree's disappearance.

McKay's impatience thwarted George Harvey's experiment in scientific crow-hunting. Crows still flew over cornfields in huge flocks in those days, before DDT made them an endangered species. George's experiment had two parts. Since crows have distinctive calls to convey various kinds of information— mating, distress, danger, feeding—George set up a record player in a cornfield to broadcast a recorded feeding call. Nearby, because crows will attack owls, he placed a stuffed owl on a pole. Then he instructed his fellow club members: "when the first crow dive-bombs the stuffed owl, hold your fire. Eventually others will follow the leader, and the whole flock will get so excited that they won't notice our shooting. I'll fire the first shot as a signal to begin."

When George played his record, the crows behaved as he had predicted. They descended on the cornfield from nearby woods like fleas emerging from a rug. Soon a large crow peeled off from the circling flock and swooped down toward the owl. There was a loud bang from McKay's direction. The number-one crow fell to the ground, and the rest of the flock flew away, screeching and cawing. After George and the other hunters had expressed their displeasure to McKay, with varying degrees of vehemence, we moved down the valley and tried once more. Again McKay fired prematurely, but this time he missed. That ended the crow-shooting. They were not to be fooled a third time. McKay had gotten the only crow of the day. We forgave him because of his likeable qualities and because of our understanding of his impatience. We knew McKay was sincere when he said meekly, "I just couldn't keep my finger from squeezing the trigger."

Although McKay Donkin mastered the art of diplomacy, he could at times be very annoying and sometimes ungentlemanly. He learned most of the tricks of dress and manner, but he did not pick up the habit of self-restraint that is the true hallmark of inbread gentility. Perhaps it was lack of exposure to the Boston Brahmin types. McKay was a product of the Colorado School of Miens and the Canadian and Texas oil fields. Even in the Navy and Washington, McKay had encountered more rough-and-ready types than social registerites. But even that is not a sufficient explanation. I suspect that he just did not want to be completely genteel.

He clung to his habits of eager competitiveness and blunt frankness. His friends forgave him. "That's McKay for you."

McKay's lack of complete gentility was shown when we were guests at the exclusive Rolling Rock Club in the Pittsburgh area. The club raised thousands of pheasants and ducks, which were released on ranges parceled out among selected parties of hunters. On one occasion McKay and I were guests of Colonel Hodge, chairman of Pittsburgh Forgings, a perfect gentleman and a deadly shot. The day was ideal for pheasant hunting—sunny, with snow on the ground. The colonel guided us down the course while the dogs pointed the birds, never flushing them until ordered to do so. Between us, McKay and I bagged more than twenty birds in two hours. Then the colonel halted, and the dogs came to heel. I realized we had returned to our starting point near the holding pens, to which missed pheasants returned. Club members considered shooting within a hundred yards of the pens to be unsporting, but McKay was oblivious to such unwritten laws. Off he went in pursuit of a bird on a nearby knoll and then down the other side. The colonel and I stood motionless, he with a small smile. Now I realized that my friend had committed a second faux pas: He was poaching on someone else's territory. The colonel, gentleman to the core, would not call McKay back. When he returned, his sheepish expression showed that he had realized his gaffe. Any embarrassment McKay felt, however, was outweighed by his obvious pride in having bagged two more pheasants.

We were invited to shoot ducks at Rolling Rock on another occasion. Each gunner was assigned by lot to a blind on the shore of an artificial lake. When the ducks were released by gamekeepers, they headed for the lake. A gunner was supposed to confine his fire to a portion of air space in front of his blind. McKay drew a choice blind in the morning, on the middle of the lake shore. Being a good shot, he downed a couple of ducks before his neighbors got any. As he became increasingly excited, he widened the angle of his attack until he was clearly encroaching on other hunters' air space. At lunch McKay was subjected to a little joshing about "letting me have a shot once in awhile." In the afternoon, by some sleight of hand, McKay drew the blind on the end of the flyway. His fellow hunters made sure no birds even got to his assigned air space. At dinner he was forced to say, "Since lunchtime I haven't even had a shot," to which one hunter wryly remarked, "Guess you've learned your lesson."

McKay ended his bird-hunting career by shooting grouse in Scotland

and Spain. He went abroad with his beautiful guns, his sharp eyes, and his quick trigger finger. The trips were a disappointment. The European nobility and gentry were less forbearing toward McKay's over enthusiasm than the executives at Rolling Rock, let alone the shirt sleeve club members at State College, Pennsylvania. To the European grouse-hunters, McKay must have seemed tense, eager, and too competitive. To him, they seemed effete and blasé. As he said to me on his return, "They produce nothing, do nothing useful, and are terrible bores into the bargain. It wasn't much fun."

Working with McKay Donkin in Penn State's Old Main Building *was* fun, because he was never boring and because he got things done—useful things. I have described, in my vignette on Penn State politics, McKay's role in getting a second golf course on the campus to keep ahead of recreational needs, at minimal cost to the university. A more outstanding example of his diplomatic ingenuity was his acquisition of a high-grade airplane for next to nothing.

My constant flights all over the east, in all kinds of weather, made McKay nervous. The fact that I had a pilot's license and shared flying duties with the university pilot simply increased McKay's concern. Finally, he was dissatisfied with our tiny single-engine Cessna 182. In sum, it was preposterous in his mind that the university president was regularly "risking his life" in the air. Vainly I countered that we did not take unnecessary chances, that our safety record was perfect, and that skyways are safer than highways. McKay's last word was that at least we should get a bigger, more powerful plane. Impatiently, I said we could not afford it. All he said to that was "Let me try."

One day McKay announced that he had accepted the gift of an old DC-3 from George Deike, chairman of the university's board of trustees. We could not conceivably afford to fly that, I said. (Our average flying costs then were about eighteen dollars per hour and a hundred dollars per trip.) "But we can sell it for $25,000," he explained, "And, incidentally, Deike's company will get a tax break for the gift." During the next few months I was vaguely aware that McKay was talking with Bill Piper, president of the Piper Aircraft Company, whose headquarters were in nearby Lock Haven, and with Al Rockwell, a Penn State alumnus and president of Rockwell Corporation. By now the university operated a fleet of half a dozen small planes, because we were conducting flight-training programs for the armed forces and giving flying instruction to anyone who wanted to pay for it. Consequently, we had a sizeable staff of pilots and ground personnel, as well as a modest but fully equipped airport.

The culmination of McKay's activities came one day when he drove me to the university airport and confronted me with a shiny new two-engine Rockwell Aerocommander, with PSU-1 painted on the tail. "It's beautiful!" I exclaimed. "But," I then expostulated, "How can we afford it?" The marginal operating costs would be minimal, McKay assured me, because of the overall scale of Penn State's aviation operations. As for the capital investment, that would be negligible because of some happy arrangements. Of course I asked, "How can that be?"

"Well," said McKay, "I sold the DC-3 for $25,000. Then I went to Bill Piper and asked him if he would let me have a used demonstrator for that amount, plus a $50,000 gift to the university. I pointed out that it was not right for the president of Penn State to be flying in a Cessna made in Kansas, while the great Piper planes were being made right here in our state, so we got a $75,000 twin-engine Piper. Then I told Al Rockwell that our prexy should be flying in a plane made by a Penn State graduate. So I traded the Piper demonstrator to Al for the Aerocommander, with the price difference acknowledged as a gift to the university."

I was horrified, partly because of McKay's tutelage in the art of diplomacy. At one time I might have thought that it is okay to take maximum advantage of the other party in a negotiation, but my friend had taught me otherwise. "What about Deike and Piper?" I asked. "How do you think they feel? And didn't you take Rockwell for a ride?" "Wait a minute," said McKay. "You don't understand about such deals. Everyone comes out smelling like a rose. George Deike got rid of a white elephant, got a tax deduction and got credit for a gift to the university. Bill Piper got $25,000 and a tax deduction, plus a lot of favorable publicity in the state where he operates because he made a gift to Penn State. Al Rockwell got a $75,000 trade-in that he can easily sell—a two-engine Piper Cherokee. He also got a tax break. Above all, he can keep on boasting about his gift to his alma mater and can tell everyone that *his* university has one of the first Aerocommanders. All three of them are happy, so you should be happy, too. Go ahead and enjoy your new plane!"

McKay's service to the university was not confined to outside negotiations. He also helped me to expedite constructive action on the inside. One of his inventions was "the little green outhouse." That was a device to keep the Penn State trustees both happy and productive. The university's "legislature" was large, diverse, and colorful, being a mix of business, farm, labor, and professional people of varying degrees of acumen. Some were positive influences and some were negative: boosters and bellyachers, as I have called them in my vignette on Penn State politics. On another dimension, some were builders and some were drones. The builders wanted to get on with the job at hand, while the drones wanted to talk it to death. Usually the builders were informed and quick witted, while the drones were ignorant and dull. Unfortunately, the builders were often modest and reticent, thereby giving the drones a chance to parade their ignorance. Committee meetings, held in advance of full sessions of the board, typically functioned efficiently. The committee chairmen and key members were appointed for their expertise on particular matters such as buildings, finance, and academic affairs. At most committee meetings, the drones kept their mouths shut because the builders obviously had things moving smoothly. But at plenary sessions, the drones insisted on having their say, introducing irrelevances, misstatements of fact, and bombast. Some board meetings degenerated into shouting matches, with a great deal of acrimony. In the end, thanks to the builders, the board would close ranks, complete their business, and adjourn in an unexpected display of goodwill.

McKay looked for a way in which the university's executive officers could influence the behavior of the trustees in order to make their meetings more constructive. He found his solution in our preparation of the board's agenda. It was a characteristically Donkinesque application of shrewd observation and psychological insight. McKay's inspiration came at a meeting during which, for several hours, things struck him as going too well. Approval after approval of major items was stacked up: building plans, bond issues, new curriculums. Yet tension was building, McKay saw, because there was too little discussion. Too many opinions were dying unvoiced. The drones had too little opportunity to hear themselves as they proudly displayed their ignorance.

Then came a small item on which almost every trustee had an extensive opinion. It was a request for approval of the design of a women's athletic storage facility. The item was minuscule in comparison with the millions previously approved for dormitories, libraries, and classrooms. A mere hut for the storage of hockey sticks and such, it was to cost less than $4,000. But the architect had let his imagination run wild, resulting in a design resembling that of a Japanese pagoda. Since most of the trustees considered themselves experts on both architecture and athletics, the debate raged furiously. Was Japanese architecture appropriate for an athletic building and for that part, of the campus, and was the design authentic, and so on and on? Finally, there was a motion to table the subject indefinitely. Remarkably, the motion passed easily. More remarkably, the trustees now got back to serious business and approved the balance of the agenda expeditiously. McKay had not overlooked the flow of events.

When he and I began to put together the agenda for the next board meeting, McKay said we should follow the principle of "the little green outhouse." Upon my inquiry, he explained that a trivial item—like the hut for athletic equipment—should be placed conspicuously between batches of important matters. In a certain psychological sense, the "little green outhouse" would be the centerpiece of the agenda. "This centerpiece," McKay asserted, "should be an item on which every trustee has an opinion and a strong emotional bias but on which the university administration can afford to lose." He had developed the principle, he said, by observing the three phases of the trustees' latest meeting: the first phase, when they were still relaxed and were therefore able to listen to reason on significant matters; the second phase, when they got relief from developing tension by debate over a "little green outhouse"; and the third phase, when—with their tension relived—they could again listen to reason. Thereafter we followed McKay's principle in preparing the agenda for board meetings; we inserted a little green outhouse, and it usually got the desired results.

Once during my presidency of Penn State, I delivered a homily to my staff about the publics to which I was beholden. I counted thirteen, as I recall, including the students, alumni, and faculty of the university; the citizenry of Pennsylvania, their legislature, and their governor and cabinet; the Congress and executive branch of the federal government; the worldwide academic community through various organizations such as the Pennsylvania Association of Colleges and Universities and the American Association of Universities (an elected group of forty outstanding universities to which Pen State was proud to belong); and the occupational groupings of farmers, wage-workers, professional people, and business managers. My staff and I discussed briefly how I could maintain contact with this baker's dozen of publics and agreed that responsibility for helping me should be divided appropriately. Some responsibilities were obvious—for instance, Chuck Lewis, vice president for student affairs; J. R. Rackley, vice president for academic affairs (embracing the