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by VANNEVAR BUSH

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Contents

Foreword

For the title of this book, I have drawn on the wealth of the vocabulary of the youth of our times. Theirs is a pungent stock of words, and action marks most of them. In my time, it has been my good fortune to have a piece of the action here and there in varied circumstances. It has been a pleasant experience for me to review some of the more rugged of these, and some of the more serene.

Do birds sing for the joy of singing? I believe they do. The complexity of their songs is far greater than is needed for recognition or for marking of reserved areas. I have become acquainted with a catbird who obviously derives pleasure as he tries out little phrases on his own. Moreover, I believe that evolution produced birdsongs, and the joy that goes with them, because of the survival value they bestow.

He who struggles with joy in his heart struggles the more keenly because of that joy. Gloom dulls, and blunts the attack. We are not the first to face problems, and as we face them we can hold our heads high. In such spirit was this book written.

VANNEVAR BUSH

Belmont, Massachusetts
January 31, 1970

Pieces of the Action

I

Sixty Years

A LOT has happened in the past sixty years.

Today, as I write, I find people are worried. What do we worry about? Well, just about everything. We fear a third war with A-bombs or, worse, with biological attacks. Youth is on a rampage. Youth has always been in rebellion, and should be if society is not to become static, but this time its rebelliousness takes on especially disagreeable forms. The salutary growth of power of unionized labor has proceeded to the absurdity of the absolute right to strike, so that a few truck drivers in a great city cut off fuel and heat as winter comes and as influenza spreads, while teachers shut down the schools. We strive to assure full employment and avoid inflation, and as I write it looks as though we would lose the first or succumb to the second. We have gotten ourselves into an absurd war on the mainland of Asia, and do not know how to get out. Respect for us overseas has dwindled, respect for our morality as well as our judgment and our military skill. The international monetary system comes to the brink of disaster, quiets for a time, but presents no solution. The Supreme Court legislates, and hazards the one thing upon which it fully depends, the respect of the thinking public. Understanding and justice between the black and white races, making a long delayed start toward decency, halt and become confused. Riots, with no real basis or motivation, are just riots. Crime rises, three of our great men are assassinated, the streets are no longer safe; we stumble in our response.

Young men and women, a few, revel in dirt and sex and drugs. There are too many people in our cities; we do not know how to abolish squalor among them. We pollute our air and our water, and apparently must face some catastrophe before we become sufficiently aroused. Why go on?

Yet, as I look back, we have always been worried about something, and often about the same things we worry about now. We are in the midst of gloom, with real reason. But we do not need to become submerged in the stuff.

We are prosperous, business is good. I can hear at once snorts from idealists, I would say from liberals except that I do not now know what a liberal is. What matters increasing affluence if we forget the problems of the distressed or of the cities? We need to clean up the slums, build good housing, provide jobs for the able and job training for the presently unemployable. Of course we do. But all this can be done only if the country is prosperous, and it is. If we would pursue our ideals we had better keep it so. Prosperity is a means to an end. Those who see the desired ends most clearly are often the very ones whose vision on the means is blurred. Our growth as a nation during the sixty years or so that I have watched it grow has been beyond anything the world has witnessed. In 1919, three out of ten of our citizens lived on farms. Now the figure is about one in twenty, yet the population has doubled and there is plenty of food for all. The food doesn't get to all who need it, but we are really at work on that problem, whereas we paid it almost no attention sixty years ago. Manufactured products have been multiplied by four, although working hours in industry have increased only 50 per cent, and yet unemployment is near an all-time low. People have more of the things they want and more leisure to enjoy them, or to sit and watch Westerns and baseball on TV.

More important, in 1919 half of the adult population had attended school less than eight years; today, half have been through high school, and almost half between the ages of eighteen and twenty-one are attending college. True, some 2 per cent of those attending are also trying to disrupt the system by harassing those who operate it, and several hundred colleges are looking for new presidents, with few takers. Still there are a lot of youngsters who want an education and intend to get it. The country as a whole is acquiring better understanding of its problems; I fully believe our youngsters are far better informed, far more able to judge the world they are entering than they were when I first wondered how I was going to disturb it in my small way. There is every chance that our pace will not bog down now.*

I remember when I was told that the frontier had been occupied, that all of man's wants had been met, that science had come to the end of a trail, that future growth would depend only on increase of population. Well, we have had growth all right. I was also told that all industry was in the hands of a few great companies, that no innovator could break their grip. Today there are thousands of small companies making money on new things right in the shadow of the giants. Some of them may make the same sort of stir as Xerox, or Polaroid, or International Business Machines, all of whom are making products unheard of when I was young. Our scientists and engineers are putting on a great show. I do not expect that things will be drab or monotonous.

We have come a long way in the development of a welfare state, with child labor laws, social security, medicare, unemployment insurance, pension systems in industry, housing programs, support of education. There are two very important aspects of this great program which are essential to its success but which are often overlooked by the starry-eyed. First, we have done all this with a minimum of scandal and skulduggery. Of course there is thievery when great sums are funneled through a political system to individuals; we must expect it even as we strive to minimize it. We hear of scandals, but very little about the honest, hardworking, hardheaded majority. Could we have put on such a program when I was young without its becoming completely bogged down in filth? I don't think so. Second, we have done all this without wrecking the industrial system on which it depends for its support. We could cause wreckage if we went too far or too fast. The care of the old, the disabled, the unintelligent, the crowded, has to go hand in hand with prosperous business which pays the bills, directly and by distributing wealth in the form of wages and profits which can be taxed. The illusion that it can be done just by soaking the rich is an illusion only and it is an illusion from which we

* For the statistical base of this argument I have relied on Drucker, Peter F., ed., *Preparing Tomorrow's Business Leaders*. Englewood Cliffs, N.J., Prentice-Hall, 1969.

have largely recovered. It is the great middle class which furnishes the wherewithal. That middle class has grown enormously in my time; today it has the political power in its hands, along with its automobiles and bathtubs and television. It has been willing to be taxed to an extraordinary extent to help its fellow citizens, just as it was willing to be taxed to support the Marshall Plan to aid Europe to recovery after the war. Don't think it does not know what it is doing; it does. This is a glorious record. Beside its glory the gripes that the system is not yet perfect fade.

We have not abolished poverty in this country. We are a long way from abolishing it in the world. But we have made much more progress since World War II than in all the previous years of the Republic. Some years ago, on Cape Cod, I watched a dignified old couple die, I feel sure, of malnutrition. They were too proud to accept aid from their neighbors, much too proud to go to the poorhouse. Today they would receive a check in the mail. Of course the system has had its foolishness and its abuses. But who would go back to the old days? Perhaps we are overdoing the welfare state. Perhaps we will go so far that the system which supplies the funds will bog down. Perhaps the great middle class, which pays the bills, will call a halt. But one cannot view the whole movement without feeling a bit more confidence that man is learning how to govern himself, and that a political system with a growing prosperous middle class which knows its power still aims to care for the weak. So there is no cause for gloom as I read over and over the faults of the system, and read little of its laudable humanity.

Business is good; but, far more important, it today makes sense. Back in the late twenties there was a ballyhoo that business was good, and it wasn't. In the White House was Calvin Coolidge, who wouldn't have been there if television had been developed earlier, who didn't know what was going on, didn't try to learn, and couldn't have been taught if he had. For most of the great men of business and most of the economists were nearly as ignorant as he was. We did all sorts of fool things. We set up a tariff which prevented Europe from paying its debts to us. We pumped money into the system and much of it came to be used to gamble in the stock market on margin, with the gamblers paying 20 per cent for their money. We produced goods at an explosive pace, for business could borrow plenty of money with which to extend plants, and introduce mechanization to lift production. And we failed to lift the income of those that might buy the product. Stock prices went to absurd heights, fed by rosy predictions that we were all going to be rich. Then the whole false edifice came crashing down. I got nicked along with the rest of the population, but not much, for there wasn't much there to nick. But I learned something, and so did everyone else. A long and very tough depression ensued. Roosevelt did some of the right things, in getting a bit of relief to the twenty million unemployed. But he didn't get business rolling again, for he didn't understand business; he failed to grasp that business is founded on confidence, specifically on confidence in its government. Only when war started in Europe did we really pull out.

Those who are running the country today are mostly young men who do not remember those grim days. A quarter of our labor force was out of work or on part time, not just for a few months but for years. Haggard men were selling apples in the street. The alphabetical agencies of the New Deal brought some help—by adding twenty billion dollars to our national debt, equivalent to about four times that today—but touched only part of the distress. Roosevelt juggled the money system, welshed on government contracts involving gold, and was supported by the Supreme Court in doing so, thus throwing international trade into confusion and scaring business out of such wits as it had, but without noticeable effect on recovery. The N.R.A. invited industry to get together behind the barn and fix monopoly prices, which it did, until the Court squashed it and General Johnson [1] left under "a hail of dead cats." Huey Long [2] ruled Louisiana as a military dictator, made a bid to do the same thing nationally, and might have succeeded if a young physician had not shot him down. About the worst aspect of the whole affair was that business ethics was at a low ebb. Richard Whitney,[3] highly respected in financial circles, stole money from the trust accounts of his bank's customers, gambled on the stock market, and finally went to Sing Sing. Insull [4] built holding companies in utilities so complex that no one could untangle them, passed himself a block of stock at eight dollars a share for "promotion," and then juggled it to $115 on the market. Al Capone [5] built an empire in Chicago to which small business contributed millions for protection. Throughout the country, and for years, men lost faith, not only in their leaders, but also in the system under which their fathers had prospered, or at least had avoided abject poverty. It is well, in these days when complaint, and stark pessimism, and grim prophecies of the end of the democratic system are rampant, to review the days of the thirties from time to time. For the system survived, and moreover it cleansed itself of much of its grosser chicanery and cruelty. Our business today is prosperous. More important, it is now on a far more honest and humane basis.

Now I do not intend to write a history of the past sixty years. I agree it should be written, with the skill and insight of Frederick Lewis Allen.[6] But here will be found only a small touch of history, to fill a few gaps in the story of the way in which science was organized during World War II.

There are good reasons for studying history, but it can also trip one up. On the one hand, Santayana [7] argues that those who ignore history will be condemned to repeat it. On another, the philosophic pitcher Satchel Paige [8] advises, "Don't look back; someone may be gaining on you." I don't go completely for either one. I don't seem to care much whether Nero actually stroked his fiddle as Rome burned. I'm inclined to agree with Napoleon that much of history is a fable agreed upon. But I do think, for example, that some of us ought to get a correct picture of how well our armies were operated, for instance, in the African campaign of 1943, especially in their relations with allies, if we are to approach judgment on how well they are now operating in Vietnam, and that the memoirs of Eisenhower, Bradley,[9] Montgomery,[10] and a dozen others certainly fill gaps and thus aid in our evaluations. The study of history can aid in avoiding mistakes, provided it is recent history and accurate, that is, before it has degenerated into myth.

Learned Hand [11] remarked that while both men and monkeys are curious and monkey around, man remembers and transmits his experiences to the next generation, and the monkey does not. So, in the end, man keeps the monkey in a cage. The essence of civilization is the transmission of the findings of each generation to the next. From this comes the thought that, as a man gets older, he has a duty to pass on his conclusions to youth. Leave out that word "duty." I think that old men have earned the privilege of discarding at last that obligation. To do so is a privilege, one of the many privileges of age, such as sitting in the shade and watching the young men sweat. I would put it on a different basis. There is no doubt that older men often enjoy reminiscing and that for them to reminisce is legitimate provided they do not force anyone to listen to them. And the opposite is true. Many young men like to get the oldsters talking, provided the talk helps them to pick up a better grasp of human nature, to discern neat ways around obstacles, and to realize how the formal relations of men are intertwined with personal relations. So it may well be worthwhile to recite a few events here and there with the thought that through them some chap coming along a similar road may learn something—not be taught, but learn something—that will ease his journey.

In the long years one has had comrades, and it is good to sit down and review together, not for any moral purpose, not to plan next to strive for an objective, for the days of striving are over, but to sit down and review together just to look again at the erratic, and sometimes joyful, ways in which men of all sorts behave in this queer world. And this can even be done in a book. I have had many friends. I have even had comrades in arms, not on the fighting front, but in the rear echelons where we strived to give the boys better weapons to fight with. I salute these old comrades and hope to bring back to them a few memories, not all of them unpleasant ones.

But I have strong doubts about autobiography. There will be no ordered account of my life in this book. Rather I am going to examine a few points where I think there is misunderstanding, and where it is worthwhile to try to clarify; in so doing I will necessarily use illustrations from my own experience. There will be a few comments on my fellow men along the way.

The first thing to do, in the chapter which follows, is to fill in a few gaps as to what happened as we worked on weapons and military medicine during World War II. It is well to review this for we may have to do it again. In fact we are doing it now in these days of half peace, and not all of us are satisfied that we are doing it as well as we should. The form that military and political organizations take is important, certainly, but far more important is the extent to which scientists, engineers, military men learn to understand one another, and hence to work effectively toward a common objective. We started off rather badly in some areas as the great war started, but we ended up with an effective partnership which revolutionized the art of war. The forms of organization then used will never be repeated.

But the atmosphere of mutual respect and understanding then attained should never be lost. In discussing this, I do not review in any detail the drive toward control of the atom, or the birth of radar, for these have been described many times. If I were to expand upon any single war program I would pick the efforts of wartime medicine, for here the tale has not yet been told so that it is well understood, and for another reason which grows out of the immediate present and to which I shall come in a moment. For the first time medical science was adequately supported and encouraged. Out of it came the conquest of malaria, a temporary conquest, it is true, for the lower organisms which prey upon us exhibit agility in evading our chemicals. But the pattern of attack was the important thing and it continued into the peace. It sparked the creation of cortisone and the family of steroids. It put penicillin at our service, as could have been done ten years before had there been ample effort, and thus introduced the wide range of antibiotics. All this was important; it raised the possibility of healthy life on the planet even as war brought distress. But its importance was more than this. The war effort taught us the power of adequately supported research for our comfort, our security, our prosperity. And, in the decades of peace that followed, we carried on, doing foolish things at times, with some waste, but on the whole wisely. We indeed avoided allowing scientific effort to become a political football, or a form of patronage. We supported sincere effort, not gadgetry. We have had no vituperative contests for the federal dollar. We have done well in this whole field, and that fact is something we can be proud of. Here is the second reason I mentioned above: we need, today, something we can be genuinely proud of. It should help to dissipate the gloom. For we have been losing our pride of accomplishment in these recent days. Pride of the right sort does not go before a fall; pride of accomplishment leads to greater accomplishment.

One of the most striking changes that have occurred in this country in the past sixty years is this. The country has become prosperous, so prosperous that we have by far the highest standard of living the world has ever seen. It has also become powerful and is slowly learning how to use its strength sanely. And it has become a welfare state. We have been able to care for the old, the unfortunate, the neglected because we have become rich enough so that we can afford it. Of course we have by no means become perfect in this regard; we have a long way to go. And, if we are really going to abolish poverty, there are several things we must do. One is to render our political system capable of controlling those who would disrupt it, or those who would fatten themselves on its imperfections. Another is to strive toward a lasting peace in the world, and in this connection make up our minds on how much of our national effort has to be devoted to military matters, and how it should be done. And, finally, we need to keep our internal progress continually advancing, for we cannot help the weak unless we are strong. Therefore, much of this book will be devoted to how we can keep our business rolling—in the right direction.

This evolution of business has long intrigued me. Much has been written about it, and more soon will be. I have included some thoughts on aspects which seem to me of interest and importance, but which are seldom treated explicitly. One is the presence of roadblocks, ways in which programs become stymied. These appear in military organizations, but also in industry. I remark on some of them in Chapter III, and on others in Chapter VI. There is another aspect which deserves far more attention than it has been given. In any complex organization, and our modern forms are certainly complex, there will appear at times an individual who knows enough about the system to avoid its restraints and who proceeds to seize authority that is not his and to raise hell with it. This may be a president of a company who avoids his board, or creates a supine one, and plunges his company into adventure. It is more likely to be a young hellion who steals the authority of his boss. For peace and progress and morale this latter type should be exterminated. I contribute my bit toward this objective in Chapter IV. Then there is one more aspect, the contribution of the inventor to our system. I have to write about him, for I was one, and his contributions should be better understood, so I do so in Chapter V. I even have something to say about our patent laws, and about commissions as agencies of government.

I hear that by the year 2000 there will be so many people on earth that they can't possibly be fed. I don't believe it, unless indeed we become immersed in ignorance. There is certainly a race on between the basic instinct of man to procreate and the arts of contraception and of food technology. But this is to oversimplify the problem. Certainly we are threatened by overpopulation, but we will not get out of the jam just by producing more food.

Right at the moment food technology is proceeding at an enormous pace, so fast indeed that it is certainly outpacing growth in developed countries, and could outpace that growth in every country if it were fully taken advantage of. Just increasing the production and distribution of fertilizer would solve the food problem for a long time to come. For this there is needed local political stability and wisdom, and what we witness in young countries is not encouraging. Methods of controlling population by technical means have also noticeably advanced and will advance further. Here the roadblocks lie in age-old traditions and practices. The time when a real population logjam occurs can be much postponed, perhaps long enough for the world to acquire a bit of common sense.

We are learning to make food in great quantities and cheaply. Ten ears of corn grow where one grew before. Research during a five-year period has made possible the doubling or even tripling of the yields of rice on which the nutrition of Asia depends. In the United States today, about one twentieth of our population is busy on the farm making food for the rest, where over half of us were there busy some time ago. We can make cheap food from fish flour. We can even produce proteins from organisms that grow well on petroleum, and the remarkable point about this is that we could thus make protein enough for all the world and use in the process only 5 per cent of the petroleum we now use for heat and power. This is just the beginning. Possibly, quite possibly, enough progress can be made in this way to meet the problems of exploding population for a long time to come. It is not all easy going. Some of the new enormous yields are possible only with heavy nitrate fertilization, and there is some evidence that, if this gets into our streams and hence into our drinking water, it can raise hob. Another problem appears with insecticides. We need these to control insects which could cancel out any improvement; yet there is evidence that we have already gone too far with DDT. Prejudice also stands in the way—people everywhere have queer ideas about food. There is many a New Englander who had rather starve than eat a snail. Yet, if some of our more intelligent advertisers were turned loose, backed by the vigor of men seeking a profit, who knows, even prejudices might be overcome. If we proceed vigorously and wisely, we may still have time before a curtain descends.

But the threat is greater than just a matter of food. The more there are of us the more rapidly we use up our natural resources and pollute our environment. On top of this another factor enters. One man with a high standard of living causes more damage than a hundred at a bare level of existence. Our technology is bound to produce relative prosperity all over the world in time, unless we act with utter absurdity. So we have to face up to many problems, and they are tough ones: pollution, waste disposal, traffic jams, crowding of the unskilled into ghettos, destruction of scenic beauty, crime under crowded conditions. Worst of all, we have to face the question whether we can maintain our moral fiber in the midst of affluence. I don't worry about food supply. But I certainly do not become optimistic just for that reason.

For thousands of years the population of the earth has been held in check by poverty, famine, war, and epidemics. We have our population problem because of our success, thus far, with these basic causes. I have seen much of the whole growth of modern medicine and have had the privilege of participating in it a little. I have certainly received its benefits, or I wouldn't be writing this book. Many of the great scourges of mankind have already been conquered, and more soon will be. We are not far from the advent of general antiviral agents, to banish ills we now tackle piecemeal, and with them incidentally the common cold. The conquest of cancer is further off; no sudden inspiration will overcome it, only the patient advance of the understanding of life in all its mystery and complexity. Heart disease? Well, I don't think we are going to be rescued by swapping hearts. Nor do I believe it would be altogether a blessing to banish heart disease for old people; we have to die sometime, and this is usually a benign way. For the young, yes, we will learn how to keep their arteries from getting plugged, and their clocks from ceasing to tick. It is a great adventure, this proposal of man to regulate, and probably in minor ways to reconstruct, his own organism. It leads to overpopulation, but we do not deplore the medical advance for that reason.

Much of our advance toward better health depends on having clean surroundings. The problem of pollution does not trouble me greatly, for we have at last waked up to it. We have to quit dumping waste stuff into the air and water, and we now know it. I well remember when we didn't. In those days, the threat of typhoid was real and grim in our land. We have mastered it, as we can master the abuse of rivers by industrial wastes—and we are on the way to doing so. Our air pollution comes from many sources, but the automobile is the most threatening offender, for we insist in this country on multitudes of cars, and also on cars with far larger engines than there is any real need for. They emit carbon monoxide to corrupt our blood, sulphur dioxide to form acid to gnaw at our buildings, nitrogen compounds to make us weep. Apparently the great automobile companies would have calmly let us go on weeping, while they fed us chrome and gadgetry, except that the mild, easygoing public finally became aroused. Now the manufacturers are trying hard to tame their engines so that they won't misbehave so badly, and they may succeed. There are engines that do not pollute, for example, the steam engine. We started to use it and then pushed it aside in the early days when there was a far larger volume of air per engine for us to breathe, in the days when we thought the atmosphere was so huge we couldn't harm it. The whole subject of engines is fascinating, to me at least, so I include a chapter on them, number VI. This is a pleasant excursion for me, for it leads me into the subject of hobbies. An author should be allowed to put in some things merely because they interest him.

The cure for pollution of water is merely a matter of costs. We know how to clean up water before dumping it into streams; there are other ways of disposing of wastes. Apparently, in this strange country we just continue practices which injure our neighbors until enough people get mad and make us quit. I believe we will, before long, stop polluting our surrounding air and the water we must use.

Why this optimistic? Well, let us trace for a moment a parallel problem on which we have indeed made progress. We were once busily engaged in destroying the land on which we live. We cut off the woods, and rain gullied our fields. We cut the sod and reaped a dust bowl. Today we contour plow, we build reservoirs to control our streams, we are in a fair way toward perpetuating rather than just consuming our great forests. We are even becoming insistent that the beauty of our countryside must be preserved. Certainly we have ugly roads plastered with billboards. But we also have great highways winding through the hills that are things of beauty. I agree that many of those who travel them are blind to all that is not directly useful. But I do not agree that it is impossible to combine utility and genuine charm even on roads. I believe we are doing it increasingly, and I believe this is because that is the way the public wants it. So, if we want clean air and clean water, we will get those too.

We are not at the end of war. But we may indeed be at the end of world wars, with A-bombs, chemical warfare, biological warfare. This is by no means sure, of course, and there is little doubt what such a war would mean. Yet it is not just wishful thinking to believe we may escape. It is based on one of the primary instincts of the race, the central urge for self-preservation. Nations, rulers, do not commit suicide knowing that they are doing so. There is not much doubt, with modern communication, that they will know. There will be secondary wars, wars with conventional weapons. They will be disastrous and absurd. But they will not hold the population in check. We again replace one problem with another. It is a good exchange.

The advent of the A-bomb is generally regarded as a catastrophe for civilization. I am not convinced that it was. With the pace of science in this present century it was inevitable that means of mass destruction should appear. Since the concept of one world under law is far in the future, it was also inevitable that great states should face one another thus armed. If there were no A-bombs the confrontation would still have occurred, and the means might well have been to spread among a people a disease or a chemical that would kill or render impotent the whole population. History may well conclude, if history is written a century from now, that it was well that the inevitable confrontation came in a spectacular way that all could recognize, rather than in a subtle form which might tempt aggression through ignorance. At least we all know, we and the rest of the world, that there are A-bombs and what they can do. And other devilish forms of assault are pushed into the background, for one does not pick up a rock in the presence of an antagonist who carries a gun.

Youth is in rebellion. That is the nature of youth. Today its rebellion takes queer forms, for it occurs all over the world, so that one cannot ascribe a cause which applies only in one country. One might even postulate that it is in the nature of men to have a great war once a generation or else to hunt for a substitute. More likely the inborn tendency of vigorous young chaps to stir up the oldsters just finds it easier to do so as our social interrelations become more complex, as television renders it readily possible to gain an audience, as the automobile facilitates gatherings. Young men take a look at the world and the way it is run, and they don't like what they see. Who does? But they propose to do something about it, and the wrong way is ready at hand. We are prone to lose perspective as we watch. A thousand young rioters stop a university from operating. Half of them are from off the campus. Many are motivated by no more than a passion for notoriety. A small minority honestly objects to the way the university is run, and may be right. But, at the same time, twenty thousand other students go quietly about their studies or try to. And in that university are also those who will remodel the university of the future, working within the system; they are now just beginning to do so. The whole picture is not shown. My bets are on the small minority who will be our leaders of the next generation, who will govern us and run our affairs. I doubt if they are making much noise at the moment. But they are there. And they are far more important, far more worthy of our attention, than the minority which yells and destroys.

Of course this whole sad business of riots is allied with the great problem of crime in the streets, and both appear because of a breakdown of the old disciplines, some of which we could well discard, but some of which have been essential to our security, to our continuous control of our own affairs. We meet here a dilemma. On the one hand we certainly wish no trend toward a police state, or any arrogant suppression of freedom. We can hardly miss grasping this danger with Czechoslovakia starkly before us. Yet on the other hand we view the ascending crime rate with alarm, and rightly so. When young hoodlums heave rocks at firemen trying to put out a fire we have gone pretty far. When a wise Negro leader, a great President, a senator, are shot down, when citizens cannot safely walk the streets, we propose to do something about it. The danger is that we will do the wrong things. There are those who argue that we must clean the slums, make jobs for all, and then everything will be well. Of course we wish to do these things and should. There is no doubt that doing them would help. But don't expect these measures to solve the whole crime problem or the riot problem. Alexis de Tocqueville wrote, "The evils which are endured with patience so long as they are incurable, seem intolerable as soon as hope can be entertained of escaping from them." When Watts was harassed many, perhaps most, of the rioters were men with jobs. The rioters at Berkeley were not from the underprivileged. By all means let us remove the blots on our civilization, and clean up the slums, but it will take more than these remedies to cure crime.

I believe that we must face up to the fact that a part of our crime wave cannot be cured by the orderly processes of the law and the courts. With this must come the realization that the administration of justice can never be absolute. As we protect the citizen against the abuse of power by his government, we must realize that if we attempt to do this absolutely, we will fail to protect the citizen against some of his fellow citizens. This balance in objectives is hard to achieve; it is not grasped even among some of our judiciary at the highest levels. It is easy to be misunderstood on this point, so let me elaborate. We need to improve our methods of administering justice at the lower levels, to make our processes more just, to protect the innocent, to wipe out some of the inequities between the haves and the have-nots, to make the process more rapid and take out some of the queer kinks that have become part of it. This will help enormously. But it will not do the whole job.

The young hoodlum who throws a rock at a fireman, or through the window of a train, or who throws obscene things at a cordon of police in Chicago, will not be deterred because he may be arrested, provided with a lawyer, tried before a jury, enabled to secure bail and make a succession of appeals. He will refrain only if he is afraid of the police. Do we have to face up to this, and is it a step toward a police state? We have to face up to it all right, or else sit by and watch crime grow unbearable. But it need be no step toward absolutism of any sort. We have in general underpaid our police and left them in a position where temptation is severe. We have not made the police career a life of dignity and satisfaction. We have not supplied adequate training. Above all we have not given the police solid public support as they protect us. We better had. We spend some billions to get to the moon, as a matter of pride and in the spirit of adventure. But we have not produced except in some notable instances a fine police force in which we can take pride. I hasten to say that, in spite of all our indifference in this regard, we have produced many able, honorable, devoted policemen. But our enjoyment of freedom, and of affluence, has led us to the brink of disaster, and we need to get realistic—and tough. We will not bring crime under control until we have produced a police force that is truly feared—feared in the way a kind father is feared by his young son—feared because of its power, because of the public support behind it, and because of its pride in its performance. I recognize that fear often brings hate, and hate brings violence. But it depends upon the kind of fear. When fear appears only in company with wrongdoing, when fear is joined with respect, it brings with it no hate; it brings peace and order.

This leads me to a subject which has caused me more worry than just the rise of crime, but on which I begin to see a bit of light. The success of our democratic system of government, not by any means yet assured, depends primarily upon two things. One is that we learn, impress, insist upon the difference between liberty and license, between freedom to pursue one's own way, and infringement upon the rights of others to do the same. Apparently this was once well taught in the home, reinforced by the influence of religion, insisted upon in the schools. The home influence seems to have lost its grip, perhaps because youth now has too much too early. Under the impact of proliferating science religion has, quite illogically, lost much of its control. Far too many of our educators are galloping about with isms of one sort or another and shirking their primary duty. No wonder youth is wild. But this is, I believe, a passing phase. In every civilization, at some time, there has been confusion, with young men doing foolish things, with the great body of the public inert or yearning to be led somewhere, anywhere, following the demagogue or the man on a white horse. Yet always there has been a small minority, intelligent, comprehending the current political system, scorning both the flighty radical and the protesting reactionary groups by which it was surrounded. It is this central core that ruled our last generation, its business, its churches, its government. Amid the tumult, the hippies, the prophets of doom, we have today a group that understands and that will rule in the next generation. I am not saying that this outstanding group has always ruled us well in the past, or will rule as well in the future—I merely say there is a group that will rule. We do not need to worry too much about the ones that harass us with their insanities; as they become older they will be controlled. But we need to think more about the solid, keen, presently undemonstrative youths who will build our system of government and industry of the future, and who will build it not as we dictate, but as we transmit to them, as best we may, the wisdom to do it well. This wisdom, if it is transmitted at all, is conveyed in subtle ways and principally in our colleges and universities. Tons of print have appeared on education. I add my bit in Chapter VII.

The other thing upon which the success of democracy depends is the nature and stability of the system itself. Our forefathers were wise men. The wisest thing they did, as they formulated the political system under which we live, was to divide it into three independent branches: an executive, to manage our affairs, a legislative, to make our laws, and a judiciary to apply them. We hear about checks and balances between them. The central question is whether the system is stable, whether it contains within itself forces which come into play, when it is distorted, to restore its equilibrium. There are bound to be distortions, for able and ambitious men are always prone to invade one another's territory. So it has been going on ever since the Republic was founded. We are in one phase of it today. It worries thoughtful men. The Supreme Court has been invading the territory properly reserved for the legislature. Worse, in the opinion of many of our most eminent men in the profession of law, it has, at the same time, been doing its own work badly, and creating confusion in the body of precedent upon which the orderly process of law depends.

Charles Evans Hughes [12] said, "We are under a Constitution, but the Constitution is what the judges say it is." This, broadly interpreted, is highly dangerous doctrine. The boldest use of it occurred when the Court ruled that the allotment of members of the legislature of Tennessee among the state's counties was unconstitutional. This was the "one man, one vote" decision. I do not find any eminent lawyers who support this just on the basis that it was a proper interpretation of a constitutional clause. I do find those who support it on the basis of necessity, since the regular legislative process would not correct an obviously unfair situation. But this is saying in effect that the democratic system will not work, and that it must be supplemented by absolute power in the hands of a group of men independent of the public will. This is a hazardous concept indeed, and not a necessary one. Britain has no written constitution, no court with power to overrule Parliament when it adheres to a different philosophy of government. Yet Parliament has many times altered the suffrage and reduced the inequities in the system. Are we to assume that the British people can govern themselves through their duly elected and responsible representatives in Parliament, whereas the American people cannot, and must be looked after by a Court not thus duly responsible?

This is a harsh question. Does our system contain a means by which such departures as the one I have cited will become checked and reversed? I believe it does, and that it lies in the power of informed public opinion. We have seen the correction work. During the New Deal we witnessed a Court blocking the will of the Executive by a stretched interpretation of the commerce clause. The Executive, Franklin Delano Roosevelt, attempted to prevail by packing the Court. We all know the result; it was the greatest mistake F.D.R. ever made, and it backfired, for enough of the populace understood what was going on. Today we see the Court, among other things, stretching that same clause unduly in the other direction. The reaction has not yet arrived, but it will come.

A constitutional amendment proposed by Congress or by a constitutional convention, and ratified by the states, is an appropriate method of correction, but not one to be welcomed. When the Court sustained the attack by F.D.R., but realized it did not have solid public confidence, it proceeded to mend its ways. This is a far more healthy form of correction, and it may happen this time. The so-called common people are not as common as they once were. They may not understand the intricacies of the law. But they understand two things clearly. The first is that the courts protect them from arbitrary acts by their government, and understanding this, they propose to support the Court. But they also understand that they intend to be ruled only by those who are responsive to their nod of approval. So I do not worry unduly. It will take time. But the present aberration, which worries many thinking men exceedingly, will ultimately be corrected by that subtle, that little understood, that potent force, the power of public opinion.

There is another subject of which I have watched the evolution, and which is allied with the above. When I was young I was involved in a rough strike, and my sympathies were all with the strikers. Yet I also watched the Boston police strike, and my sympathy was all with Calvin Coolidge and the Commissioner of Police. Recently I have witnessed a strike which shut down the mental hospitals of a city. The right to strike, while once nonexistent, is now beginning to be regarded as absolute. How far do we have to go before there is a reaction? And will that reaction swing us back to the dark days when the word freedom had a hollow ring for the man who labored? I do not think so. And the reason I don't think so is that I have confidence in the good sense of the rank and file, of the man who works for a wage. It is one thing to see a strike which puts a crimp in the power of a great industrial company, cuts its profits, cuts its dividends to its stockholders. It is quite another thing to see a strike which spoils kids' schooling, or threatens their health from uncollected garbage, or which destroys the morale of a fire-fighting organization. What do I expect? I expect to see the pendulum swing back. I believe there are enough statesmen among the leaders of labor, where there have always been many, to ensure that it swings back smoothly, and swings soon enough to avoid a real catastrophe in a big city, and a reaction that is bloody. Am I an incurable optimist? Well, Britain had a general strike, so did France, and we did not. Our labor has been rough at times, it has taken us to the brink of a national shutdown, but when the issue became clear it has not in the end challenged the political power. It has not long challenged public opinion. I don't think it will. Our skilled laborers today are men of property. Men protect their property when they understand that it is threatened. Moreover we have a far better organization of labor in this country than there is in Great Britain, and I believe personally that this is primarily due to the fact that our leaders of labor have shown they have a lot of good sense. They have not become completely identified with one political party; they have maintained their independence so that both parties bid for their favor. They have so organized that their control allows few wildcat strikes, and have worked with industry to establish arbitration methods to handle the minor things which cause them.

They evidently understand, as fully as anyone else, how inflation can cancel the gains of labor. They have not, with some notable exceptions, blocked the introduction of more effective methods of production. With all this I fully expect that they themselves will curb the tendency to carry the right to strike so far that it produces a violent public reaction. I just hope they will do it soon enough.

No industrial or political system can be made to operate without differences in rank and also differences in standard of living. The concept "From each according to his abilities, to each according to his needs" has been thoroughly exploded, for it has never been made to operate throughout history, even in isolated utopias, and the most recent example is the Russian experience. I have two points to make on this subject; both involve technological progress, and both contain a note of cheer.

Throughout any organization nearly every individual both receives and gives orders. Even the President of the United States gets his orders at times from the Congress or the Court. At the lowest level there may be those who give none, but if any such individual is capable in any degree of controlling the acts of his fellows, he will not long remain in the basement. It is essential that such a structure exist; otherwise there would be chaos. But it makes a great deal of difference, from the standpoint of political stability, and of human dignity, how the orders are given, and whether they are followed cheerfully. In the last half century I testify we have made genuine progress in this regard. The two points are interconnected; if orders are given reasonably and to free men there is a far greater chance that they will be executed with more than sullen acquiescence. In the old mill town the owner could, and sometimes did, fire the father of a family on a whim and without notice, leaving that family in utter distress. He could do so for he took orders from no one, not from his board or his stockholders, not from the local government or courts which he controlled, not from laws really protecting the worker, for there were none. He could do so because of the existence of a caste system, where a few families controlled for generations, and because of his conviction that that system must be preserved and emphasized, for his own good, and as a sublimated conviction, for the good of society.

This has nearly disappeared. I have written a few words about the power of labor, the ways in which it has been salutary or the reverse, the confusion it has often caused, its excesses. So let us take a look at some of the technological reasons that have made the change inevitable. The mechanic, our hypothetical victim, is no longer a citizen of a small town; his radio and TV, his magazine, his commentators, give him a far broader view if he will accept it. When he is in trouble of any sort he can pick up his telephone and talk to his neighbors. Or he can pack his family into his car and head for some place where he can sell his services. The result is that, instead of arbitrary action, there is reference to an arbitrator, who can, and does, issue an order to the mill manager. Inexpensive printing and mailing, transportation which makes wide gatherings possible, have made the evolution inevitable. I do not overlook its crudities, or the hazard of arbitrary exercise of power by those who control the movement. I just say that there is far more human dignity at every level, that this is worth having even at the price we pay, and that it has come about largely because of technological advance.

It is often claimed that mass production reduces the worker to an automaton, whose contribution is merely that he can make certain motions with his hands that the machine cannot duplicate, and none with his brain. There is not much doubt that this sort of thing happened when the automatic loom entered the textile industry. But, as mechanization proceeded and became highly complex, there has been a transformation, and some, not by any means all, of present development has been in the opposite direction. If I go into some highly developed machine shop today I find little dull repetitive work indeed. The machinist operating a tape-controlled milling machine is certainly paid primarily for his intellectual skills; so was the man who built the machine in the first place. Even on a fully mechanized production line, where parts move along and each operator does just one act, the men are chosen, not because they can duly perform when all goes well, but because they can overcome obstacles when it doesn't. Certainly we have a lot—too much—of monotonous, repetitive work, but I believe less and less. For the nature of the machine is that it can replace men only when the performance is repetitive. And there is a considerable fraction of the population that can do nothing more than simple repetitive acts, and another fraction that wishes to do no more. I remember once, in a plant, seeing a woman operating a noisy punch press. I said to the superintendent, "You ought to automate that punch or at least quiet it down." He suggested that I walk to the other side and look at the woman running it. I did, and retired from my objections. She was at work, and content, at the only sort of work she could do, and the noise bothered her not at all. I also interviewed a group of girls winding coils for relays on a machine, an operation highly repetitive, calling apparently for much skill and keen eyesight. They told me they could carry on the operation in the dark. We tried it, and they did. Were they bored? Apparently not; they were daydreaming, reviewing the latest movie, apparently quite content. But the burden of my argument is not that automation does not have its distressing aspects, it does; rather it is that we need more, not less of it, if we would create a system in which every individual capable of using his mental processes finds the opportunity to do so. We do not need to fear that this will produce more goods than can be purchased, and lead us into a glut and a depression. One thing we have learned after sad experience is that public purchasing power has to be maintained, and the inclination to purchase maintained, or we will fall flat on our faces, no matter how skilled we may become in production.

Now the other side of the matter is that an industrial or political system, if it is to work, inevitably involves a wide difference in rewards of a material sort, a large difference in standard of living between those at the top and those at the bottom of the pyramid. I grant that differences are inevitable, for the reason that those in control will just take more of the product of industry for themselves. I also contend that a difference is desirable, for men with large responsibility can in general work better if they live better. But I also contend that in this country the ratio between the top and the bottom is far less than it used to be, and that the diminution of the ratio is largely due to technical progress. I also contend that a small ratio works just as well as a large one. In the world at large the ratio has been increasing, for the successful citizen of a developed country lives far better than he did, in a material sense, in health and comfort, and material freedom and opportunity, while the lowest level in the backward areas has moved forward hardly at all. But the thing that strikes me is that the skilled mechanic, today, in this country, lives just about as well as his boss, and sometimes better. The boss drives a more expensive car, but it really isn't much better in looks and performance. His food is not much more attractive, certainly it need be no more tasty and balanced, in these days of modern refrigeration and transportation. His house is warmed and cooled; so is the machinist's. The boss takes his holiday at Palm Beach instead of in the Maine woods. I had rather be in the woods. There is still difference of course. He can have more stimulating conversations, if he wants them and avoids cocktail fights. His surroundings give him a better chance to think, if he knows the meaning of the word. But the gulf is far narrower than it was. And it can be narrower still if we keep our technology moving.

I am not going to write about the race problem—just add a note. It was before my time when the Irish in this country were looked down upon, pushed aside, regarded as ditchdiggers, no more. I have watched them take over the government of Boston, and we have had an Irish President who might have been one of our greatest since Lincoln had he lived, for he was learning fast. At one time the Jews in the East Side of New York were discriminated against fully as much as the Negroes were in the South, although in different ways. They, and Jews from other lands, have moved on to provide us with far more than their share of professional men: physicians, lawyers, artists, scientists. I have now witnessed the beginnings of the emergence of the Negro race. I watched, in Massachusetts, the election of a Negro senator, a Republican, when his party otherwise went down to resounding defeat, and he won on sheer merit. I am concerned, as every citizen should be, about our race problem. There is still bigotry about. It will take another generation at least for us to arrive at justice and sanity. But I am not disheartened.

The success of any political system whatever depends upon the caliber of the men who rise through it and govern. We certainly have an absurd system. Judgeships are handed out as political patronage. We elect Presidents through a process which costs millions, and someone has to put up the millions. Still, by some mysterious process, able men rise. We obtain far better leaders than we deserve. In Chapter VIII I comment on my relations with a few of these.

My conclusion is that things are not so bad as they seem. And from this I draw two thoughts.

The first is that we need a revival of the essence of the old pioneer spirit which conquered the forest and the plains, which looked at its difficulties with a steady eye, labored and fought, and left its thinking and its philosophy for later and quieter times. This is not to call for optimism; it is to call for determination. Caryl Haskins [13] writes, "Clearly there exist powerful bulwarks in our society against these dangers. They inhere primarily in those vital qualities of hope and devotion to movement and expansion that have always been so much at the forefront of our thought and feeling." *

Do we have crime in the streets? Let us proceed to suppress it without sacrificing our hard-earned freedoms. Is there abject poverty which we have been late in recognizing? We have already made progress against it; let us continue, and bar the jackals that would feed on our benevolence. Are there riots? Let it be known that this is a democracy, that the people control its power, and that none may defy it. Is the world headed for overpopulation and famine; do we pollute our air and water, and plunge toward the exhaustion of our resources and the defilement of the beauty of our land? In our strength, with the ingenuity and skills which are ours let us bend to the task of preserving the fair land we have inherited. Is there bigotry and malice abroad? Let men of good will, of whatever race, join to further at last the brotherhood of man, which has been dreamed of, and never reached, since the first men made axes of stone and joined together in the hunt. Will there be wars, a holocaust before there appears in some distant day one world under law? Let us maintain, so that there may be no doubt, our strength and with it our decency and our sanity in a divided world.

And the second thought. We all of us can list catalogs of things that annoy, or disturb, or even distress. There are a number in the current scene that irk me: managers who let a young pitcher become weary and lose the game, and then take him out; drivers who tailgate ten feet behind my car, all set to run me down if I stop; television cameramen who don't bother to adjust their cameras so that I can see what is going on in the shadows; people who ring the wrong telephone number, and then say, "Who is this?"; advertisers who put blinking shows on TV to injure my eyesight, or who step up volume to rattle my ears; nice ladies who signal for a left turn and then turn

* *Report of the President,* Carnegie Institution of Washington, 1967–68, p. 36.

right; writers who distort facts as they argue; men who tell their secretaries to get me on the phone, and then leave the office; TV advertisers who present kids eating gooey messes; parents with spoiled kids who yell at meetings; spoiled kids generally; automobilists who heave things out of their cars; politicians who indulge in personal abuse; companies that flood my mail with junk I don't want—you complete the list if you wish.

But there is a more general aspect of the current scene which disturbs me more than these. We take ourselves too seriously these days. Something sad appears to have happened to our sense of humor. It is true that our outlook is grim; we face many tough problems. We have to tackle them with determination, and we will do a better job at it if we do not let them get us down—pitch us into gloom and frantic despair. Have we lost our sense of humor? I don't think so. But I sorely miss Will Rogers,[14] who could remind us of our absurdities, and do so without rancor. One new Will Rogers would do us more good than a dozen economics professors lecturing us on our sins. I have been looking for him, and have not found him. I shall continue the search. I miss his touch, as I miss that of Charlie Chaplin,[15] and that of Ernie Pyle,[16] who could find the sustaining strength of banter even among those who fought in the trenches. Let us remember when the Battle of Britain was on, when pilots mounted their fighters and flew out to protect their fellows, knowing that their chance of survival was small indeed, that they went with a smile, and with a light quip to those on the ground, and they fought better as they did so. Life can be gay even as it is grim. Let us then revive our sense of humor, face our difficulties with lifted spirit, and enjoy the life we live. For, after all, there is happiness in the world, in this cruel and confused world, if we but seek it, and welcome it when it appears.

II

Of Organizations

WHEN Eve joined Adam there was formed the first organization in history. It was a simple one, yet its essential relations and the regulations governing it have not even today been fully worked out. And ever since Eden man has been building more and more complex organizations with which to carry on his affairs. The advent of machine methods of production greatly accelerated the process; for subdivision of labor, aided by mechanization, improved efficiency and turned out great masses of goods. The advent of devices to do man's mental drudgery will lead to even more complex organization. This change will render us more prosperous and perhaps it may make us happier in our relations, but I doubt the latter. Only the subtle art of human understanding will do that, and that art is beyond the machine.

A good organization for a research laboratory would not work well for a combat regiment in the field. I have worked in organizations of many sorts, academic, governmental, business, and have studied them all assiduously. I have also read books about organizations, but have learned little that way. I have noted, for example, that good organizations for small and large businesses are about as far apart as those for colleges and banks. Yet organization is a subject that every one of us has to deal with in one way or another. It starts in the crib, when one joins a family, and it never ends. The most interesting of all its forms is political organization; that is the only form I have succeeded, or nearly succeeded, in keeping out of.

We are here concerned mostly with organization for waging war, and in discussing it I will review some of the organizational relations of World War II. This is not to suggest that they should be repeated if we should again engage in unlimited war, for conditions change, and what fits well today may not fit at all tomorrow. I have already cited Santayana's aphorism about the study of history. It is equally true that he who is not interested in the ways in which his forebears occasionally got themselves out of an organizational snarl is likely to miss the exit when his own human relations get all balled up. Review of this sort does not prescribe repetition; with experience we should reasonably expect to improve.

In making war we are concerned with the military and civilian sectors and with their interrelationships. Military organizations are usually pyramidal, with lines of authority explicitly clear and positively enforced. The object is to ensure that every need for a decision promptly finds an individual who can and must decide, but that no commander shall thus become burdened with more than he can handle. This condition is essential when the battle is joined; it should be adhered to as preparation for battle is made. Yet many necessary functions of a military structure do not fit well into such a scheme, and here is the rub.

There are two primary ways in which to lose a battle or a campaign, assuming nearly equal antagonists as far as equipment, morale, and size of forces are concerned. One is to have confused lines of authority. The other is to have a top commander with poor judgment. History is full of examples. Charles XII of Sweden was overwhelmed by Peter the Great at Poltava in 1709 largely because he used bad judgment in dividing his forces, thus confusing his lines of authority and failing to protect his lines of communication.

And we can find examples in our own recent history. As is discussed in Chapter IV, we nearly got into serious trouble at the Battle for Leyte Gulf, in October 1944, because we had two independent commands in the field, coordinated only by Washington, and there coordinated by a committee, namely, the Joint Chiefs of Staff. On the other hand, the campaign in Normandy and France was thoroughly in order, for Eisenhower was in supreme command and insisted on remaining so, although he had that most difficult task of coordinating allies in the field, and although he had to fend off occasional attempts to interfere with his authority.

We should congratulate ourselves on having an excellent military organization, even with its remaining imperfections. Yet look how it was put together! It was built by a succession of Commanders in Chief, each laboring with a legislature and the democratic process. That process has built us an overall governmental structure which is a monstrosity, with overlapping authority, swelled bureaucracies, agencies with no base at all, muddled lines of command. About its worst feature is the way it heads up to the President, loading him with far more subordinates than he can possibly handle or for that matter, know at first hand. Not only does Parkinson's law ensure obesity; agencies that are created nearly at random fail to disappear when obsolete. But out of this confusion has emerged a military organization in which we can in general take pride! So, if that organization still has faults or limitations, we should certainly forbear harsh criticism of those who operate it. Moreover, generally good military organization has evolved, within a democratic atmosphere where very little else is well organized, largely because organization is at the heart of military performance, and professional military men learn this early in their careers.

The essence of a sound military organization is that it should be tight. But a tight organization does not lend itself to innovations in the technology of warfare. This fact, rather than the conservatism of military men, which they merely share with most of the rest of the population, accounts for much of the sometimes extraordinary time lag on weapons which we witness throughout the history of warfare, including that which was to be seen occasionally in World War II. But there have also at times been reactionary opposition and plain obtuseness as some explanation for the lag.

There are plenty of historical examples of reactionary stubbornness. The English during the Middle Ages went to Asia Minor on several crusades and were there outranged and harassed by the Saracens, who had a far better bow with which they punctured their foes from a distance. The English even took back to England with them samples of these bows made of bone and animal sinew. Yet for several hundred years they continued to use the old English longbow without changing its material, shape, or dimensions in the slightest degree.

The English overcame the Spanish Armada for a number of reasons, but notably because their ships could sail faster and higher on the wind, and had more long-barreled guns that enabled them to harass the enemy while still out of reach of the short-range ship killers that made up the enemy's principal armament.* Yet two hundred years later, when French and British fleets were contending for mastery of the West Indies, both fleets were armed with essentially short-range guns, and their speed had not picked up much. There was some use of coppering of the bottoms of ships, which increased speed and made it tough for worms. But this was not general, and one slow ship in the line of battle slowed up the whole line.

The rifle was apparently introduced before the year 1500, and the production of spiral grooves inside the barrel, which distinguish a rifle from the smoothbore and give it far greater accuracy, called for mechanical skill of a high order. Breech-loading rifles were used to some extent during the American Revolution. Yet it was only in the 1840's that the needle gun was adopted by the Prussian Army. It was no more than a well designed breech-loading rifle, with its comparatively rapid rate of fire, yet it gave the Prussian Army a distinct advantage, to the surprise and dismay of the rest of the world. Matters were no better nearer home, where authority reports, "In the Indian wars of the second half of the nineteenth century our skeletonized army stationed throughout the prairie and desert areas of the West was fighting with single shot weapons long after the Indians had succeeded in acquiring effective repeating rifles." †

It is pleasant to turn to situations where conservatism, or lethargy, was overcome by far-seeing, energetic individuals. The Oerlikon gun, by far the finest aircraft weapon then and for many years, was produced in Switzerland in 1932. Germany and Japan promptly placed orders for it. Britain struggled along for many years with essentially obsolete guns. The Oerlikon was exhibited to the Royal Navy, but they found it did not meet one of their somewhat artificial rules and frowned on it, all through 1937 and 1938, while war was clearly impending. Only because Lord Louis Mountbatten,[1] with his energy and prestige, broke the deadlock did the new and better gun get adopted and manufactured. It certainly did—before the end of the

* Mattingly, Garrett, *The Armada*. Boston, Houghton Mifflin, 1959, p. *xvi*.

† Eisenhower, Dwight D., *Waging Peace, 1956–1961*. Garden City, N.Y., Doubleday, 1965, p. 253.

war the United States spent some two billion dollars making it and was turning out thirty million rounds of ammunition for it every month.

We all know the story of radar and the Battle of Britain. When war broke out in Europe there was a considerable body of opinion that the Luftwaffe would be invincible, that it would overwhelm England. I was then associated with Charles Lindbergh, on the National Advisory Committee for Aeronautics. He believed this, sincerely, and argued vigorously that we should draw into our shells and prepare. He was far from alone; our ambassador at the Court of St. James's, Joseph P. Kennedy, felt the same way, much to our embarrassment. Vigorous on the other side were a few men such as Wild Bill Donovan,[2] and without question Franklin Delano Roosevelt. When the test of arms came the Luftwaffe was defeated, by a thin margin, and after great damage. It was defeated by the skill and spirit of the British pilots, to whom Churchill paid, for all of us, a just tribute. But it was also defeated by the presence of radar, and this was due to the wisdom and forcefulness of a number of British scientists, working with, or through, or at times in spite of, the British military system. The name Sir Henry Tizard [3] stands out, to me, most clearly in this connection. But he was far from alone, and, in his famous tribute, Churchill undoubtedly had these rugged pioneers of radar in mind. What are we to conclude from all of this? First, that military organization needs to be close-knit if it is to fight well. And loosening it in time of war, with the idea of making it able to progress more rapidly on weapons, would be fraught with the danger that the loosening might be in the wrong places and lead to a lot of damage. Second, that there should be close collaboration between the military and some external organization, made loose in its structure on purpose. And the relationship should be a cordial one, assured to be so by the supreme command.

A military organization can become less tight in time of peace, and it can then innovate. It can, but that it will is not at all sure. It suffers from a disease that permeates all governmental, that is, all politically controlled, organizations—the daft belief that if one does nothing one does not make mistakes, and the drab system of seniority and promotion will proceed on its deadening way. There is also the danger that, if the military organization is loosened during peace, it will not be sufficiently tight should a great war suddenly descend on us. So, even in peace, there need to be channels by which the freedom and initiative always present in academic circles and in well-managed industry can be joined with the military in a partnership of effort. The obstructionism of military systems, as it existed for a thousand years, ended with the last great war. It is far more possible today to maintain a productive collaboration between military men on the one hand and civilian scientists and engineers on the other than it ever was before. The scene is changed. It changed when the A-bomb exploded.

It is not my intention to examine here the organization for the development of novel weapons as it now exists in the United States; for one reason, because we are not now at peace, nor are we engaged in a major war, and, for another reason, because to do so would require a volume in itself. Rather, I propose to trace the origins of the organization as it evolved for such purposes before and during the last great war. There was then no doubt that something in the way of a new form of organization was acutely needed.

The National Defense Research Committee was launched over a year before we entered World War II, as a civilian organization of scientists and engineers for the purpose of developing new weapons for military use. A year later it became part of the Office of Scientific Research and Development, which included research on military medicine.* It had two very important aspects: It reported directly to the President of the United States rather than through military channels, and it had its own funds with which to work. In discussing it, I have no thought of prescribing an ideal organization for the future, since later organizations, if we need them for war, will certainly be far different, nor of writing history, since that has already been well written. I want merely to fill in a few gaps and to develop further the theme that, in war, military and civilian partnership is possible and essential.

There were those who protested that the action of setting up N.D.R.C. was an end run, a grab by which a small company of scientists and engineers, acting outside established channels, got hold

* Baxter, James Phinney III, *Scientists Against Time*. Boston, Atlantic-Little, Brown, 1946. This official history of the O.S.R.D., reissued in paperback by the M.I.T. Press, 1968, tells the full story.

of the authority and money for the program of developing new weapons. That, in fact, is exactly what it was. Moreover, it was the only way in which a broad program could be launched rapidly and on an adequate scale. To operate through established channels would have involved delays—and the hazard that independence might have been lost, that independence which was the central feature of the organization's success. The one thing that made launching it at all possible was the realization by the President that it was needed.

The beginnings of the effort date back to meetings of the Committee on Scientific Aids to Learning which the National Research Council had created in 1937. Among its membership and its numerous consultants that committee numbered many men competent in science and in engineering who were also keenly concerned with the trend of international affairs and the mounting threat posed by Adolf Hitler. Discussions of the problem cropped up whenever the group gathered for committee business, and at other times as well. Frank Jewett,[4] Jim Conant,[5] Dick Tolman,[6] Karl Compton,[7] and others of similar convictions thus gathered frequently in 1939 and early in 1940. The men I have named were indeed an extraordinary group. Conant and I had become acquainted through a couple of arguments while he was president of Harvard and I was once representing Tufts and once M.I.T. They were good, vigorous battles from which we emerged friends. Apparently we had thus gotten controversy out of our systems, for I do not remember that we disagreed again throughout the war. I had served for six years as Compton's second-in-command, as vice-president of the Massachusetts Institute of Technology. Then he served five years in an organization of which I was formally chief. It is not every man who can adapt wholeheartedly to such a change. But Karl Compton was a rare individual. To those who remember him, I do not need to enlarge on this. To those who do not, it is almost impossible to convey his full charm and effectiveness. He was an able administrator, yet he had few of the characteristics that one usually considers essential for tough leadership. He merely inspired those about him with loyalty and sincere friendship. No man in academic life was ever more genuinely loved. Dick Tolman was a theoretical physicist as well as a dean at California Institute of Technology, one incidentally who never received his full due as a pioneer in physics who molded the rapidly altering thought in that field. As soon as the war began in Europe, he just packed up and moved to Washington, to be at the center of things, and to seek a way in which he could help. Frank Jewett was a bit older than the rest of us. He had been responsible for the growth and caliber of the Bell Telephone Laboratories, then and now the finest industrial laboratory in the world. When we gathered, he was president of the National Academy of Sciences. For an industrialist to be elected to head that notable and sometimes stuffy organization reveals that it is, after all, not always as stuffy as its general reputation leads many to believe.

We were all drawn together early in 1940 by one thing we deeply shared—worry. It was during the period of the "phony" war. We were agreed that the war was bound to break out into an intense struggle, that America was sure to get into it in one way or another sooner or later, that it would be a highly technical struggle, that we were by no means prepared in this regard, and finally and most importantly, that the military system as it existed, and as it had operated during the first world war, which we all remembered, would never fully produce the new instrumentalities which we would certainly need, and which were possible because of the state of science as it then stood.

Let me review, as far as it can be done after a long span of years, the pattern of our thinking. When we first met, the war was in a restrained phase. French and British armies behind the Maginot Line faced the Germans, but neither side was mounting an offensive. It looked as though matters might settle into a stalemate as had the first world war. Many in this country—probably a majority—were convinced that we could keep out of it, and that if the United States thoroughly supported the Allies with every sort of material assistance from its enormous productive capacity, the Allies could force a decision, and could do so long before the United States could raise, train, and transport an army of its own. There were also many who would have nothing whatever to do with the war—in fact, Congress had put a law on the books which would prevent us from rendering any aid whatever. Such were some of the contrary views. Nonetheless, we knew the struggle would be a tough one, but it appeared that our contribution would be in materials rather than in men. As I remember it, this was the view of nearly all the thoughtful men with whom I conferred during that strange period when war had begun but not started. The general opinion was that it would be a short war. Later, as I came to know Franklin D. Roosevelt, I was sure that that had been his thinking also during the calm before the storm.

It was not long before the whole scene changed. Germany swept across France, conquered it with ease, and nearly trapped the whole British army. Russia stood aside, to wait for the rest of Europe to exhaust itself and then to pounce. Nazi submarines threatened to cut off Britain's food supplies and starve her out. Hitler's might had been vastly underestimated. It looked as though later we might face, alone, a Nazi enemy master of all the production facilities of Europe, allied with Japan to conquer the world, and in a fair way to do it.

Thus, long before Pearl Harbor, no doubt remained in the minds of the group I have referred to. It was our fight, and we would inevitably be in it, no longer on the sidelines, in with all we could muster. And it was evidently going to be a highly technical war, one in which techniques might indeed determine the outcome. If we had any doubt on that score, it was resolved as we pondered the possibility of an A-bomb, in Nazi hands or in ours. And it was clear that the United States was by no means prepared for that sort of war, in technology, in organization, or in the minds of the people and the Congress. So whenever two of us met we reasoned together as to what we might do.

This is not to say that ours was the only group working; there was, for example, work going on to improve fighting aircraft and to design new tanks. Moreover, the British were well ahead of us on such matters as radar, for they had been fighting while we were gradually becoming convinced that it was our fight as well. But there was a great gap to be filled if we were to fight a modern war in a modern manner. Moreover, although we hardly grasped the revolutionary possibilities at the time, the threat of a possible atomic bomb was in all our minds, and time might well determine whether it became ours or a means for our enslavement.

It was natural that the group should depend on me to take the lead in trying to get action. I was resident in Washington. I was chairman of the National Advisory Committee for Aeronautics, and as such had had some relations with the White House and with appropriations committees of Congress, and under the tutelage of such men as John Victory [8] had learned quite a bit of the mysterious ways in which one operates in the Washington maze.

John Victory was secretary of the National Advisory Committee for Aeronautics, had been, in fact, since its inception. He knew his way about Washington through the intricacies of the political scene, and he taught me many of the ropes. He had to the full that loyalty which is one of the heartening characteristics of the Irish. He was a fine, devoted, able, unsung public servant. We have many such or the show could not go on. I would have stubbed my toe many times if it had not been for his guidance. As the head of an agency, I had to sign many documents filled with figures in millions and certify them as correct. If John Victory told me they were correct, I could sign with assurance and sleep at night. To any man carrying heavy responsibility, especially in the maze of government, my advice is clear. Find yourself a John Victory and relax.

Oscar Cox,[9] an attorney, who was one of a young group that did chores for F.D.R., was an extraordinary man. It was said that he was the real author of Lend-Lease; certainly he did much to get this crucial plan launched. The first of many things he did to help me was to introduce me to Harry Hopkins.[10]

The fact that Harry and I hit it off is among the minor miracles. I had grown up with deep-seated distrust of most social innovators, whom I regarded as a bunch of long-haired idealists or do-gooders. That prejudice has long since become mellowed, and I think the tribe has also matured, but my prejudice then was fairly intense. Harry, however, was different from the caricature I had built up in my mind. I think that what attracted me most was his utter loyalty to his chief and his complete suppression of personal ambition. Harry had similar doubts about men who were geared into the current industrial scene, and I was certainly in his eyes a bit of a Tory. He was a New Dealer and I was far from it. Yet something meshed, and we found we spoke the same language.

He had a tough hide, as should anyone who seeks political solutions to sociological problems. I once went to his office, a ten-foot-square cubicle in the White House basement. On his desk was a copy of *The New Yorker*, open to a "Profile" about Harry Hopkins which was far from complimentary. So I asked him how he liked it. "Oh, not bad," he said, "but they should have been able to do much more with the ammunition they had."

Another time, late in the war, he called me down to see him. He had before him a letter from Churchill to Roosevelt, and he read me a few sentences from it. I do not remember the exact language, but it intimated that the war would go better if I were to be personally dropped into the middle of the Potomac River. Then he kidded me about it, having lots of fun doing so, and wanted to know what I had done to rouse the ire of the Prime Minister. I told him about a recent session in London when the Prime Minister had objected, with characteristic vigor, to British-American interchange provisions on atomic energy. Then Hopkins became serious and said, "I do not think the Chief needs to see this." Of course I do not know what then occurred, but I did not hear of the matter again. I strongly suspect that Hopkins called Churchill on the phone and told him he was deleting that paragraph. He was fully capable of doing just that.

I well remember our first discussion in 1940. Each of us was trying to sell something to the other. I was promoting the scheme that became N.D.R.C. He had before him a plan for an Inventors Council, of which more later, which he wanted me to take over. I won. He later took the inventors into his Department of Commerce, and he agreed that N.D.R.C. made sense.

A legal framework was needed, and Oscar Cox found a structure convenient for N.D.R.C. During the first war, there had been established by law a Council of National Defense; the law had never been repealed, and the Council, made up of Cabinet members, could still act. Of course these members quite likely did not know that they existed as a council. But they would readily sign something that had "OK–FDR" on it. So N.D.R.C. was to be launched as a creation of the Council. As such it would become a part of the Executive Office of the President, and it could draw on the funds of that office, the White House willing.

Harry and I then went in to see the President. It was the first time I had met Franklin D. Roosevelt, although I had done some small job under his orders in N.A.C.A. I had the plan for N.D.R.C. in four short paragraphs in the middle of a sheet of paper. The whole audience lasted less than ten minutes (Harry had no doubt been there before me). I came out with my "OK–FDR" and all the wheels began to turn. The Council signatures were obtained, the executive order issued, we found out how to get money, and we organized in a hurry. The order specified that the president of the National Academy of Sciences, the Commissioner of Patents, and a representative each of the Army and Navy were to serve on the Committee. I had to make the nominations of the other members. That was easy; they were the group that I have named above, and they were soon Presidential appointees. Conway P. Coe,[11] the Commissioner of Patents, Brigadier General George V. Strong,[12] representing the Army, and Rear Admiral Harold G. Bowen,[13] representing the Navy, with them constituted the Committee.[14] Then we had our first formal meeting.

There was one very important policy decision at this meeting, one that established policy which continued throughout the war. It was decided to build a pyramidal organization, with broad delegation downward, and full facility for programs to move up. So each independent member took over a division of operation, Compton on radar and allied matters, Conant on chemistry and explosives, Jewett on communications and transportation, Tolman on armor and ordnance, and Coe on patents and inventions. Each then built under him a system of sections to deal with explicit problems, and each recruited his personnel for the purpose. Soon these sections became groups of specialists. They contained the keenest scientists and engineers that could be found in the universities, scientific institutions, and industry. A very important point was that they gathered with them, as time went on, able young officers from Army, Navy, and Air, men who knew the needs in the field intimately, and later men of combat experience whose contribution was essential to practical results. This system of military members applied all the way at every level. I shall have more to say about one of the military members of N.D.R.C. a bit later.

At early meetings there were two more important steps. The first was the decision to put all business and governmental relations in a separate office reporting to the chairman, thus leaving divisions and sections free to get to work without worrying about terms of contracts, patent matters, relations with the Bureau of the Budget, the Civil Service Commission, or a million other details. Fortunately we knew just the right man for this purpose and we got him promptly. Irvin Stewart,[15] a lawyer, who had been a member of the Federal Communications Commission, and who in 1937 had become director of the Committee on Scientific Aids to Learning, was a keen student of governmental organization. He rendered yeoman service throughout the war and ran all the business affairs and most of the relations with other agencies of government. Before he finished he had some hundreds of contracts to administer.

A second decision proved to be important, not only for the war years but also for the postwar period. In fact it set a pattern that meant a great deal eventually to advanced education in this country. We decided that we would make contracts for research directly with universities, not with individuals therein. And we decided that, in so doing, we would pay the full costs of the programs.

This does not sound like a very radical departure from previous practice, but it was. We did not propose to make a grant to an individual, leaving his university holding the bag on all the extra cost it involved, and leaving him with the burden of the business affairs inevitably part of the job, on such things as taking into account patent policy or security. We proposed to contract with the university itself, thus placing on it the responsibility for all such matters, and also giving it the authority necessary for proper performance. In return we proposed to pay its overhead costs, the portion of its general expenses properly attributable to the added operation. And we decided to do this on an overall, not a marginal, basis.

This sounds reasonable. It certainly did to the university executives. But not to old hands in the Washington bureaus. They did not pay much attention to it at first—probably didn't know it was going on. Later, when things began really rolling, I was visited by representatives of the Bureau of the Budget, the General Accounting Office, the Civil Service Commission, etc. They accused me of being in a plot to fatten up the universities at government expense. But by then there was not much they could do about it. I just told the Appropriations Committee of the House what I was doing and how I was doing it. They seemed to think it was all right, so that was that. After all, not even the Budget gets gay with that committee. Right through the war the Budget was disturbed that I seemed to be outside its control, and it occasionally tried to do something about it. The committee members, I think, were occasionally amused by these efforts, as they pried them out of me (it is not considered polite to volunteer fiscal information to Congress without Budget assent, and agencies seldom look for trouble with Budget). At such times the committee would even insert a few words in an appropriation bill to ease my trials.

Rigid control by Budget is of course necessary in time of peace. In war it often must be avoided.

The pattern of contracting we thus initiated has had lasting and decidedly beneficial effects. Just after the war ended, it was literally a lifesaver for the universities. Had it not been in force, one of two things would have happened. Either the universities rather generally would have gone bankrupt, or, more probably, the scale of research in this country would have been cut back with such stringency that we would not have been able to support the great surge in industry that later occurred, or to train men to keep it going. Over the longer range, the consequences of our innovation are equally significant, I believe, for they have clarified and stabilized the relations between the government and the universities, focusing responsibility and enhancing stature.

There were few battles with universities over contracts. I remember one with pleasure. It was between William H. Claflin, then treasurer of Harvard University, and Irvin Stewart, the contracting officer of O.S.R.D. They were both friends of mine. The issue wasn't very serious, just a question of how funds were to be handled. So I just sat on the sidelines and enjoyed it. Stewart won. I am sure Claflin would deny this, but in any case it is too late to revise the contract.

In general things went very smoothly indeed. In a time of true national emergency men who understand cease to worry about salaries or the details of contracts, especially if they are associated with the military, where draftees are serving on a pittance and risking their lives, and officers are carrying far more responsibility than do the presidents of large corporations and also risking their lives, for there is no safe sanctuary in modern war. Little things become submerged when great things are dominant.

To get back to the early meetings, I remember that we discussed how much money we should ask for initially, and I suggested five million. It shows how rapidly things can change when there is a war on. Frank Jewett thought we could not possibly spend such an amount promptly and effectively. It did not take us long to get over that idea. Soon we were talking in hundreds of millions. This is one reason a new independent organization was needed; no existing organization could have secured millions without becoming entangled in red tape or legal restraint. At first the money was available through the

Executive Office of the President. After the formation of O.S.R.D., later to be discussed, it came from appropriations by Congress, and we had magnificent support by the Appropriations Committee of the House of Representatives and its chairman, Clarence Cannon.[16] Never once did we ask for funds and fail to secure them promptly.

It will be noted that the scheme of organization I have outlined left the chairman with nothing to do on internal affairs. That is correct, and it is the heart of good organization. My job, and this continued throughout, was to handle relations with the President, Congress, and the military services, and to try to keep the lines free and money available so that others could do some work.

Later on I will tell something of the extent to which I was, or was not, brought into the subject of war planning. At this point, however, I would like to get two things clear: First, I had plenty to do. Second, I made no technical contribution whatever to the war effort. Not a single technical idea of mine ever amounted to shucks. At times I have been called an "atomic scientist." It would be fully as accurate to call me a child psychologist, and I will speak of one such elsewhere. There have been times when I have been credited with new ideas on antisubmarine warfare or radar. This is a complete myth. The key ideas arose in the special groups that were working for months and years with the problems as they appeared to those who knew the practical aspects of a phase of the war effort, and they arose nowhere else. One time Isidor Rabi[17] came to see me, and from his manner I knew he was going to ask for something unusual. So I said, "You know, Isidor, you chaps get all the fun; you see the stuff develop and get into use. You can witness concrete results as they appear in the field, and all I can do is sit behind a desk and talk to chaps like you who come in to pull my leg." "Well," he said, "you asked for it, didn't you?" That's right, I asked for it, and I got it. But it removed me far from the areas where accomplishments occurred which gave a type of satisfaction that I missed.

My own office was a small one, and remained that way. I had a few young aides about me, and some remarkable ones. It has been heartening to see them seize responsibility and go on to fine careers. One man who served as general counsel, Jack Connor,[18] got the itch and left me to join the Marines. He later became Secretary of Commerce. He was succeeded by Oscar Ruebhausen,[19] now one of the most respected attorneys in New York. But my prime captive was Carroll Wilson.[20] I borrowed him in the first days from Research Corporation of New York. After a few months I thought to call up Howard Poillon,[21] president of that organization, to thank him. He said without any reproach, "I had as much to do with loaning you Carroll Wilson as Queen Wilhelmina had to do with loaning Holland to the Germans."

This is typical of how N.D.R.C. was manned. A need appeared, the man came, and formalities were worked out later. Carroll spoke in my name throughout the organization, even when it attained its full complexity, on any subject, and he was the only one who did so. He had that rare tact which enabled him to do so without ever getting anyone mad. He was much younger than most of the men he was dealing with, and strange though it will appear to some, this difference was one of the things that enabled him to do what he did. At M.I.T., when Karl Compton was president, we had a useful practice of taking an outstanding student for a couple of years after graduation as an aide in the president's office. That is where Wilson and I became acquainted, and he knew me like a book. When there was a tangle somewhere he would know all about it, and he would post me if I needed to know. But time after time that tangle would become resolved without my getting into it at all. We did not have many controversies, but there were bound to be some. I always suspected that Carroll's technique was to persuade the contestants that it was better for them to get together than to take a chance on what I might rule. Of course a corollary to this is that there should never be, throughout an organization, any doubt as to where authority for making decisions resides, or any doubt that they will be promptly made. I remember one time when a section walked into my office and resigned as a body. I still do not know quite what the row was about. So I just told them, "One does not resign in time of war. You chaps get the hell out of here and get back to work, and I will look into it." They did, and I did, but Carroll soon reported that the row was all over. Needless to say, after the war he had an extraordinary career.

But the present story has to do with organization and how it came about, and it has not yet all been told. Organization tended to become more complex throughout the war, as new needs and methods arose. This was true in the United States, but it was still more true in the United Kingdom. In fact I never did get British organization clear in my mind. Fortunately, Conant did, and this fact was to stand him in good stead when, after delays which both of us regretted, we opened a London office of the N.D.R.C. Conant went over to London to get the office established and affairs in running order. He was accompanied by Carroll Wilson, and by Fred Hovde,[22] who remained to operate the office and did a grand job. There was no doubt whatever that Conant should be the one to start the affair—the prestige of Harvard in Britain was great and he had many friends there. I hesitated for a while and then agreed he should go. I have some difficulty in recalling the way we were thinking in those days; we had not then gotten into our stride where personal danger was disregarded. London was being bombed, air flights over the Atlantic were hazardous; to agree to subject the president of Harvard to real danger held me up for a while. Later there would have been no hesitation whatever. The decision for Conant to go was absolutely correct. He did a magnificent job, a far better one than could have been done by anyone else. He wasn't bombed very much, had the flu a couple of times, talked with Churchill and King George VI, and got us off to a good start in relations that were to prove of great importance.

Those were critical times. The Luftwaffe was by no means defeated, nor could one discount the possibility of an invasion of Britain. The United States was still operating under an absurd neutrality law which was supposed to prevent our extending any aid to Britain whatever as it stood alone against the might of Hitler. Lend-Lease was before the Senate, and its passage was by no means assured. Conant had spoken out strongly and many times to urge that we come to our senses before it was too late. He was the ideal emissary, and his reception signified far more than just an exchange on weapons. To the sorely pressed British he symbolized the might of America pausing on the brink before plunging in to aid a struggle to suppress an assault that threatened all of life as it had been built up over the centuries in the two countries with a common speech and common ideals.

By the spring of 1941 it had become evident a change in organization was needed. For one thing we needed money in real amounts, and it was evident this should be obtained directly from the Congress. Also we needed to have the status of a formally established agency, rather than of just a part of the President's office. The opportunity came because of efforts to get something rolling on medical research for war purposes, and this produced a weird story indeed, for there were a number of conflicting interests in the field.

It appeared that plans for a Committee on Medical Research were being taken to the President through various channels. Felix Frankfurter, Archie MacLeish,[23] Mrs. Roosevelt, were mentioned. My attitude was that this was none of my business, that we had plenty to do on weapons, that we would be glad to help by making our experience available, but that we much wished to keep out of medical matters entirely.

But I got into the subject personally and very definitely before long. My bird dogs reported that the President was about to appoint a Committee on Medical Research made up of some of the prime movers in the American Medical Association. Like many of my friends, I had a deep-seated distrust of that organization, so the next morning I breakfasted with Harry Hopkins in the Lincoln Room at the White House. I told him that the Great White Father was about to put his foot in it, and when he asked how, I said that he was about to appoint a Committee on Medical Research, that three of the men he was to appoint were under criminal indictment in the District of Columbia, and that the columnists were going to have a field day. This was no mere personal opinion. There had been a move toward group medicine in the District of Columbia, the A.M.A. in opposition had started boycotting hospitals, and the Department of Justice had brought criminal indictments under the anti-trust laws. Things exploded at once. Harry got Missy LeHand [24] on the phone, found that the executive order had not been signed, and told her not to let the chief sign it. So it never happened. I do not think anyone knew just who put the monkey wrench in the gearbox, but I understand a prominent official of the A.M.A., known to be loquacious at times, was so sure the plan was approved that he announced the great step forward that evening in Chicago. And I am sure that my willingness to hustle to keep my chief from a misstep helped in our later relations.

The next event also had its elements of humor. A different and excellent group of medical men went with a new plan to see Harry at the White House. As with many of the events reported in this book, the details here are based solidly on hearsay, and since the story is on that insecure basis the participants are unnamed. As they did not know their way around very well, one of them got out of a taxi, went up on the porch, and there to his surprise met Harry. He started to unfold his plan, but Harry interrupted with, "What does Bush think of it?" "Why, Bush, he does not know anything about it and says it is none of his business." "Well, there is no use taking it up with me unless Bush approves it." So Harry left and a very bewildered emissary got back in the taxi with his colleagues and they went home.

The next morning I had an office full of medical men. They told me Harry insisted that I approve the plan. I told them there must be some mistake, that I had nothing to do with medical research, and did not want to have, and that my opinion would hardly be worth anything. But they insisted that it would be of great help if I would listen to their plan and comment in a letter to them. So I listened and then wrote a letter which said that the plan seemed to parallel the N.D.R.C., that the latter worked, and that to this extent the plan seemed to me sound. So they went off happy.

But just about that time, apparently, the President became weary of hearing about medical research. So he sent a message to the Bureau of the Budget, which drew up executive orders, saying that he wanted this medical show put under Bush, and that he did not want to hear a damned thing more about it. I never found out whether this message was in writing or just over the phone—at any rate I never saw it. But things began to pop all over. Out of it came the Committee on Medical Research which, with N.D.R.C., composed the new Office of Scientific Research and Development.

Sam Rosenman [25] and I drafted the order establishing the O.S.R.D. —mostly Rosenman with me hanging hard on the outskirts. The order assigned N.D.R.C. as one component of the new office, and that assignment brought the only change in the civilian membership of N.D.R.C. to occur during its lifetime. I was relieved of the chairmanship in order to become director, O.S.R.D. Jim Conant became chairman of the N.D.R.C., and he was succeeded as a member by Roger Adams,[26] professor of chemistry at the University of Illinois, who had served ably with Conant in Division B of N.D.R.C., which was concerned with chemical problems, bombs, fuels, and gases. Besides making this assignment and providing for an Advisory Council, the order created the Committee on Medical Research, specifying that it should consist of a chairman and three members to be appointed by the

President and three other members to be designated by the Secretary of War, the Secretary of the Navy, and the Administrator of the Federal Security Agency, these three to be selected from the staffs of the Surgeons General of the Army, the Navy, and the Public Health Service. Rosenman and I got in the essential points in the drafting. But there was one slip. I wanted to insert a careful statement of the relations between the new Office of Scientific Research and Development and the National Academy of Sciences and its operating arm, the National Research Council. I was given a day's delay to get it in, but no delay occurred; the order was issued before I got a chance to revise it, and it issued with no mention of the Academy. I was worried about this, for we had rushed in where the Academy itself, because of its historical background, might well have been called upon. I drafted an explanatory letter from F.D.R. to Frank Jewett, president of the Academy, and the President understood at once and signed it. This was essential, for on medical matters we needed to work closely with the Research Council. With that slip corrected, the problem of appointment was next to be faced.

Setting up the Committee on Medical Research was a far different task from starting N.D.R.C. Soon after the executive order creating O.S.R.D. was issued, I told F.D.R. that he had handed me a hot potato, as a few of the medicos were taking pot shots at one another in the press, and I might have to bump some heads together. I remember well his answer. He said, "You go ahead and bump, and I will back you up." Like many a man from New England, I had snorted at the New Deal, and I had been appalled at some of F.D.R.'s political theory and practice. Together with almost every man I worked with, in uniform or out, when war came I took the attitude that loyalty to the chief must be absolute and untarnished. But there was more than this. He gave me a tough job to do, never interfered with me, and always backed me up in a pinch. What more could one ask? Loyalty of a formal sort gradually gave way to something far deeper and lasting.

The kind of backing I was to have soon became demonstrated. Not only the A.M.A. but also the Surgeons General as a group had each sought to set up a medical research committee under its control. And here was a C.M.R. created by executive order as part of O.S.R.D. Clearly this was a somewhat complicated matter. Hence I thought it wise to get in touch directly with the Surgeons General in seeking suggestions for individuals whom I might recommend to the President for appointment. And in doing so I told them I did not propose to recommend to the President that they or their representatives should select the full membership. I had to be sure that the new committee did not become an arena for military service versus public health contests or similar battles. They did not seem to like this much. The rest of the story is rank hearsay; it would never be admitted in court. I got it from a White House aide who has since died. But it sounds so much like F.D.R. that I think it is probably accurate. As the story goes, Admiral Ross T. McIntire, Surgeon General of the Navy, and the President's personal physician,[27] went to the President to protest. F.D.R. was signing a stack of letters, which the aide had brought, and he continued to sign them as the protest continued. After a while F.D.R. looked up and said, "Mac, what are you talking about?" "Why, Bush and this new committee. He is wrecking the scheme at the outset." A long pause and more signing of letters, then, "Look, Mac, I put that in Bush's hands. He's running it, and you get the hell out of here."

One reason I am inclined to believe something like this happened is the fact that I had no further difficulties in relations with the Surgeons General, and another is an incident which occurred a few days later. Lew Weed[28] insisted that he must be chairman of the C.M.R. and indicated that, if he was not, he would not participate. I certainly could not blame him. He was chairman of the Medical Division of the National Research Council, and he had there built up a system of committees which were working on military medicine. It was the primary effective link between civilian medicine and the armed services. Its only limitation was that it had no direct means of securing government funds for its purposes, and hence could not be highly independent. But Weed, for all his undoubted ability, was just not the man who could give the leadership to C.M.R. that was essential; leading medical men is far tougher than leading scientists or engineers. Still, C.M.R. would get to work rapidly only if it could use those N.R.C. committees.

So I explained my quandary to F.D.R. I did this in an elapsed time of perhaps two minutes, for I had already learned that he liked it short and to the point. He looked at the ceiling. Then he recited a rather formal statement. It expressed appreciation of all Weed had accomplished (of which he probably knew nothing beyond what I had told him). But he added that, for reasons he was not ready to state, he did not wish Weed to take on the burden of chairman of C.M.R. Then he grinned at me and said, "I'll give you that in writing if you need it." Of course when I repeated this verbatim to Weed, that settled the matter, and Weed went on to work ably and effectively as a member and vice-chairman of C.M.R.

It soon became evident that the one man for chairman was A. Newton Richards.[29] He had a distinguished record in medical research. But, more important, he was a wise man, trusted by all who knew him. It was a fortunate choice. Many years later, for he lived to be ninety, I concluded that, of all the able men I have known, of all the men of science I have known, he was the most fully respected, yes, the most beloved by his colleagues and by everyone who knew him.

I called him in Philadelphia and told him that the next day I had to give nominations to the President for the chairman and members of C.M.R. I told him there was one individual who had been recommended as chairman by almost everyone I had consulted, and that I proposed to submit his name unless there was some strong reason to the contrary. "Who is he?" "His name," I said, "is A. Newton Richards." There was a long pause, then, "Christ!" I forget my answer—it was no doubt something flippant. Another long pause, then, "I'm coming down to see you." He did, and the next day C.M.R. was launched. I do not need to give the full structure of C.M.R. or other parts of O.S.R.D., as Irvin Stewart has done so.*

My relations with C.M.R. were harmonious, no doubt because I was careful not to interfere with professional judgments. Nothing raises the hair of medical men more thoroughly than that. We did have one minor jam, on the subject of bovine mastitis. C.M.R. recommended a program on this and I turned it down. The reason I did so was clear enough to me, but apparently not to them, for we went through this process several times. The Appropriations Committee of the House quizzed me occasionally to find out whether O.S.R.D. was stepping beyond its proper boundaries, in particular whether we were doing research which had been turned down within one of

* Stewart, Irvin, *Organizing Scientific Research for War.* Boston, Little, Brown & Co., 1948.

the regular government departments and picked up by us. There is nothing that irritates an Appropriations Committee more than to be circumvented. This proposed bovine mastitis program looked like that sort of thing to me, and I did not propose to get in Dutch with an appropriations committee, even if a lot of cows continued to have mastitis.

There were also a few times, not many, when I had to come to Richards' aid. (Medical men seem to have more feuds than the rest of the population.) But this was easy. All I had to do was to find out what Newton actually thought and back him up in doing just that. One irate researcher gave me an oration on the iniquities and ignorance of C.M.R. which took half an hour. At the end of it I told him that if Newton cancelled his contracts they were going to stay cancelled. I found later that this statement evoked cheers in a number of places. In any event, the net result was peace and lots of progress. Much of this was due to Dr. Chester S. Keefer,[30] Newton's executive officer: Newton was no manager and knew it, but Dr. Keefer was. He has had a long and distinguished history, not only in research on Asian diseases, but also as an administrator, among other things as the president of the American College of Physicians.

I have noted that most of the worthwhile programs of N.D.R.C. originated at the grass roots, in the sections where civilians who had specialized intensely met with military officers who knew the problem in the field and who often, as time went on and mutual respect developed, saw to it that the civilians got their own impressions in the field. When once a project got batted into form which the section would approve, with the object clearly defined, the research men selected, a location found where they could best work, and so on, prompt action followed. Within a week N.D.R.C. could review the project. The next day the director could authorize, the business office could send out a letter of intent, and the actual work could start. In fact it often got going, especially when a group program was supplemented, before there was any formal authorization.

With these provisions for quick action when it was needed, the general pattern of operation of O.S.R.D. as a whole was tripartite. Wherever possible, existing laboratories were utilized; if a group was at work in a general area where either N.D.R.C. or C.M.R. saw possibilities or recognized a need, that group, assuming competence, was called upon to go ahead with work under contract. Sometimes, however, the swift advances necessary called for the creation of new major groupings of scientists and engineers, and for the provision of suitable facilities for their use. Such, for example, was the Radio Research Laboratory at Harvard, where able work was done on radar countermeasures under the direction of Dr. Frederick E. Terman [31] of Stanford. Work on the development of better radar meantime went ahead apace in the Radiation Laboratory established at the Massachusetts Institute of Technology where with Dr. Lee A. DuBridge,[32] then of the University of Rochester, as director, and Dr. F. Wheeler Loomis [33] of the University of Illinois as associate director, teams led by such brilliant men as Ivan Getting,[34] Luis Alvarez,[35] and many others sped our knowledge of microwave radar to constantly greater usefulness. At the California Institute of Technology, a continent away, research on rockets and manufacture of rocket motors was being pushed, and basic studies then made have their impact today in the exploration of space.

The third form of operation in O.S.R.D. was especially true of the work of C.M.R. This was the central coordination of programs on a focal problem being carried forward in a large number of laboratories. The great search for drugs to be employed against malaria is a case in point. The program, in which C.M.R. and the National Research Council collaborated, was directed by an independent board, of which Dr. Robert F. Loeb [36] was chairman. Research laboratories, agricultural experiment stations, pharmaceutical manufacturers, inmates of penitentiaries who volunteered their help—all pulled together. Some fifteen thousand new chemical compounds were synthesized in this endeavor and tested in animals and some of them in men before, finally, one superior to atabrine was achieved. Similar in scope and effectiveness was the program for the production of penicillin, in which major pharmaceutical companies merged their efforts, the work being coordinated by C.M.R. When at last success was accomplished, the precious small supply had to be rationed—the medication used to treat the first service case had cost about $50,000—and rationing was managed with consummate skill by Dr. Keefer.

As I look back on it, it seems to me that this swiftness in getting things started, combined with the flexible scheme of operations, was an important ingredient in the total program of O.S.R.D. It meant essentially that, as things moved readily up in the pyramid, the inverse also occurred. I was, for one thing, F.D.R.'s scientific adviser, as far as civilian scientific matters were concerned, and sometimes more broadly. He did not often call for advice, but when he did, he certainly got it. I did make sure that my answers represented, not my personal judgment, but that of the best qualified scientists or engineers to be found. Sometimes I had a chance to check up before answering. More often I had to answer at once, follow this up by going to the right place in O.S.R.D., and, if necessary, correct or supplement my remarks to F.D.R. at the first opportunity. There is no doubt that this attitude, which was shared by all the top echelon of O.S.R.D. in their contact with military men, helped greatly in relations with men in uniform. It was in contrast to the attitude of Cherwell [37] in Britain who felt he was bound to give Churchill his personal judgment on all such matters. In my opinion this procedure, which certainly annoyed British scientists, was as much the fault of Churchill as of Cherwell. The point is that I regarded myself as a link between the President and American science and technology, and not as an oracle or an expert on all matters scientific. I know F.D.R. recognized this, although I never explained it to him. He asked a question, and he got an answer. Sometimes the questions were on touchy or political matters. But he always got an answer, and then I scurried around to make sure I had made sense. F.D.R. no doubt followed this course with others. It had dangers, serious ones. But he found out what was going on.

As I noted, the order establishing O.S.R.D. called for the institution of an Advisory Council, of which as director I served as chairman. The Council brought together the chairman of the National Advisory Committee for Aeronautics, the chairmen of N.D.R.C. and C.M.R., and the top military men from the O.S.R.D. organization to go over policies—for example, the constantly harassing problem of deferments from the draft. I cannot pay tribute to all who there contributed, but I do wish to mention Admiral Furer.[38] Relations with the Navy were often troubled; I did not have the cordial understanding there that I had with Secretary Stimson.[39] Admiral Furer bridged many a chasm and earned the respect of all of us.

The establishment of the Council was one example of the way the organization adapted as needs required. I want to mention a couple of others. Once N.D.R.C. had gone into action, and developments began to come out of the laboratories and demand for them grew in the field, some means of getting devices quickly into production became needed. The first step was a special group headed by Fred Gordon [40] which had the responsibility of speeding contracts by the armed forces for new instrumentalities as they appeared. Known at first as "few-quick," it was the precursor of the Engineering and Transition Office of O.S.R.D., which Gordon ably headed until November 1943, when Paul A. Scherer [41] became its skillful chief. The Office, which was concerned with information-gathering, knowledge of production facilities and supplies of strategic materials, and cognate problems arising under the wartime pressures of industry, carried on to the end of hostilities.

Just as contacts at home between the laboratories and industrial suppliers had to be maintained, so, it soon became clear, for the best utilization of new devices in the field we would often have to send scientifically qualified men overseas to explain new equipment and facilitate its getting into use quickly and effectively. Collaboration with the service using the new device was essential. Hence in the fall of 1943 we set up an additional subdivision of O.S.R.D., the Office of Field Service. Its task was double—to see that the proper field personnel were available at the right place, and to help in the broader question of the best use of new equipment in actual fighting. Karl Compton became the first chief of O.F.S. A mission to the Pacific theater of operations, in which he conferred with the Commanders in Chief and other officers of both services, showed that there was plenty of need for the sort of help O.F.S. could supply. In this work Compton's skill and address were invaluable, and he was ably seconded by his deputy chief, Alan Waterman,[42] who was to succeed him as chief in midsummer of 1945, when Karl became director of the Pacific Branch of O.S.R.D.

Innovations such as these, which took place within N.D.R.C. and O.S.R.D. themselves, were testimony to the swiftly changing needs of war. There was another organization, apart from O.S.R.D., importantly concerned in the whole subject of new weapons. This was the Joint New Weapons Committee of the Joint Chiefs of Staff. It was set up originally because Secretary Stimson felt that there was a need for machinery to correlate the work of the services and civilians. I served as chairman, and the services were at first represented by Brigadier General R. G. Moses [43] and Rear Admiral W. A. Lee, Jr.[44] * The committee was, of course, entirely independent of O.S.R.D., thus I wore two hats. In fact, I wore three after the Manhattan District was established, as will later appear. Since I was director of O.S.R.D. and also chairman of J.N.W., a useful channel was opened for all sorts of matters. In spite of its somewhat grandiose name, J.N.W. did not accomplish much in the way of resolving differences between services. The reason was simple. If the Joint New Weapons Committee disagreed, all it could do was to refer the matter to the Joint Chiefs, who would proceed to disagree on just the same basis. But the Committee did form a link between civilian effort and top echelons of the services, for it had a notable succession of military members. It also had a succession of brilliant secretaries. Its subcommittees did a great deal to bring civilian and military thinking together on tough problems. I noted that, in general, in the type of argument that occurred, reasoning about new and sometimes complex equipment, the admiral was likely to be faster on his feet than the general, especially if the latter was an Air general. Part of this was no doubt due to the fact that the Navy had for years merged complex equipment into its practice. Some of it, I think, was due to the transformation then going on in the Air Force to an emphasis on team work rather than on individual skill. It was also clear that the secretaries, all of whom were exceedingly able younger officers, and who often had some difficulty in keeping an utterly expressionless countenance when discussion became a bit turbulent, had great and salutary influence in bringing the right men together about a table, especially in subcommittees when such subjects as guided missiles were considered. I had a grand succession of these young officers. They were, in order of their periods of service, Colonel K. Maertens, Colonel B. L. Lucas, Colonel Edwin Cox, Lieutenant Commander H. L. Vanderford, and Colonel Harold P. Gibson. Every one of them wanted to get out of Washington and into the field, and they all eventually did. But to

* They were succeeded, for the Army and Air Forces, by Col. Oscar Krupp, Major General S. G. Henry, Brigadier General Grandison Gardner, Brigadier General William A. Borden, Major General Curtis E. LeMay, Colonel G. W. Trichel, and Major General H. S. Aurand; and for the Navy, by Rear Admiral W. R. Purnell, Rear Admiral W. S. DeLany, Rear Admiral L. B. Richardson, and Rear Admiral Jerauld Wright.

each one I said that I would put no obstacles in the way of their leaving, provided they found me an equally good officer to succeed them, and this they always accomplished.

I want to tell a story about one of these secretaries, Colonel Cox. He was from Virginia. He left me as we were working on the proximity fuze, as I shall relate elsewhere. He was a Virginian, all right; when I visited him after the war I saw in his dining room the flags of the First Regiment of Virginia of which he was colonel, previous colonels having included George Washington and Henry Clay. The flags had on them battle streamers going back to the French and Indian wars. Just at the end of World War II Cox had an extraordinary experience. He was then down by the Danube with a major and a sergeant, examining a chemical plant. Drawn up on the shore of the river was the whole Danube Flotilla of enemy patrol boats. The commander of this fleet, anxious to surrender, and seeing an American officer, came ashore and proceeded to surrender to Cox. Now Colonel Cox says he was in doubt whether an American Army officer should take the surrender of an enemy navy, but he took it. And, in token of surrender, the German officer handed over a ceremonial dagger, a beautifully constructed affair with the swastika and the Austrian eagle. Some years later Cox presented this dagger to me, and it hangs in my study with a suitably inscribed plaque. It is my only physical memento of the war.

The relationship between civilian and military personnel went through an evolution during the war. At first there was a gulf between them. Finally they became partners with mutual respect. At first scientists were received at least with tolerance; they had interesting ideas and not enough practicality to be embarrassing perhaps, but engineers were something else again. Among older military men the engineer was at first regarded as in all probability a thinly disguised salesman, and hence to be kept at arm's length.

Our military people were by no means alone in this mistaken view, as may be suggested by this anecdote: Some time after the war, Prince Philip came over to this country and paid an unexpected visit to the National Academy of Sciences. Dr. Detlev Bronk,[45] who was then president of the Academy, hastily tried to get a group together to greet him. I hustled down to the Academy, and as we waited for the others, the Prince and Bronk and I had a very congenial discussion about general matters, including current technological developments. Just what it was that triggered the change I can't at this distance in time recall, and indeed it may well be that I then read into the episode something that was not actually there. Anyway, there came a moment when the congeniality chilled, gave place to courtesy with less warmth, and that moment occurred when the discussion brought out the fact that I was an engineer. As I remember it, I thought with a sudden rush of disbelief, "Can it be that the low valuation that the British usually allow the engineer has reached even the mind of this versatile man?" For it was then true that to the British—and they have not quite recovered from it yet—the engineer was a kind of second-class citizen compared to the scientist.

This is the way things were at first in our relations with the military in our war effort. So all O.S.R.D. personnel promptly became scientists. I remember Hartley Rowe [46] protesting against any such designation; he was chief engineer of the United Fruit Company and was used to tough construction jobs in the tropics. I told him he might as well relax and enjoy it, so he became Dr. Rowe (which incidentally was quite correct) and a scientist. The business of elevating the scientist on a pedestal probably started with this move, and it has certainly persisted and misled many a youth. Even recently when we sent the first astronauts to the moon, the press hailed it as a great scientific achievement. Of course it was nothing of the sort; it was a marvelously skillful engineering job. Now that there is a National Academy of Engineering, perhaps the title of engineer will recover its just recognition.

Having mentioned the moon, I should elaborate a bit and admit to a partial change of opinion. I opposed the moon race as it began. I did so quietly before a Senate committee, and then held my peace. My opposition rested on two points. One was that the scientific results expected by no means justified the enormous expense involved, for the program called for spending money we badly needed for other things. The other was that a race to the moon against Russia made little sense to me in terms of our national security. But we succeeded in that great engineering effort, and we got to the moon. It seems to me that it is now folly to repeat that effort time and again, for one thing because of the hazard that we may get men into space and not be able to get them back, so that they would die there in full view of television. And such a disaster would cancel completely the subtle great benefit we received from the whole program. Years ago, Lindbergh flew alone across the Atlantic—a stunt pilot contesting for a money prize—but he was the first man to fly it alone. That flight gave us a great lift, gave the whole world in fact a boost in morale when it was badly needed. We had then been wallowing in filth, the newspapers had been filled with the sordid details of murder trials, evil had been rampant in high places and duly spread before us. Then came Lindbergh, and his dignity and modesty caused us again to believe in our fellow men. So with the landing on the moon: In the midst of gloom and petty wrangling we suddenly became convinced that man could accomplish great feats of danger and skill. It was worth the effort if it caused us, once again, to have confidence in man's ability to overcome rugged obstacles, and to rise above the sordid, the petty, the commonplace, and the wails of those who tell us we are doomed. It was a great, and badly needed, success. But not in the way in which it is generally viewed.

To return to our story, it was not long before civilians and military officers began to understand one another. The officers found that "scientists" could bring together subtle physics and chemistry, but could also do it in an exceedingly practical and hard-boiled manner. The "scientists" found that the officers had something which was new to them and admirable—utter loyalty, the ability to operate smoothly in a rigid system, and the art of command. This tendency toward mutual understanding and respect was greatly furthered by a few individuals. I remember with what joy I learned that Tony McAuliffe,[47] back for a time from the field, was spending evenings in rugged arguments with a group of keen men out of the laboratories.

A good illustration of the extent to which men of diverse backgrounds learned to work together occurred in connection with the proximity fuze. This was one of the great achievements of the war, comparable in the overcoming of technical obstacles with radar or the atomic bomb. It will appear in this account in other connections. When the fuze first arrived at full-scale production, the building of millions of shells thus equipped in a score of factories, I told the Navy that it was time for them to take over. They said they would make the production contracts, but they urged me to keep the technical control in the team that had done the development. I asked what would happen if this freewheeling group wrote a tough letter directly to the Secretary of the Navy. The admiral I was talking to, not here identified, said, "We would intercept it." So I agreed to keep the affair in O.S.R.D. provided they would attach one damned good officer to my office for liaison. They certainly did—first Captain C. L. Tyler [48] and then Commander W. S. Parsons,[49] two of the finest men I ever worked with. And the fuze went on to cut down kamikaze attacks in the Pacific, to share in the answer to the buzz bomb in Britain and Antwerp, and very decidedly to aid in turning the tide at the Battle of the Bulge.

The greatest example of the merging of efforts of science, engineering, technology, industry, labor, finance, and the military is, of course, that which brought about the atomic bomb. In scale relative to the scale of its time, the building of the Pyramids offers a possible comparison. Never before in modern history, however, had anything of the sort been seen—from the extreme reconditeness of the basic science involved through the marshaling of thousands upon thousands of workers of hundreds upon hundreds of skills, the creation of vast communities, the building of laboratories and manufacturing plants of a complexity never before tackled, to the final feverish pressure of the Alamogordo test, the assembling of the attack bomb, and the explosions over Hiroshima and Nagasaki that ended the war. That story has been well told from various points of view by General Groves [50] and by others concerned, and is being recorded in official history as I write this. Hence I do not undertake a record of the detailed course of those events. Rather, I want to review some aspects of the whole affair—those having to do primarily with the development of policy, with some of the interpersonal relations involved, and with the decisions that had to be taken as to who should do what and why.

The whole affair started in January of 1939, and it started with a rush. Niels Bohr [51] came from Denmark to confer in this country about experiments by two colleagues, Otto Frisch [52] and Lise Meitner,[53] which indicated that a neutron could knock a uranium atom into relatively large pieces and release great energy in the process. In fact, Otto Hahn [54] and Friedrich Strassmann [55] in Germany had just shown that one of these pieces was undoubtedly barium. Germany thus might have been in a position to have a decided head start. That it did not was due to several reasons. One, very important, was that Frisch and Meitner, his aunt, were refugees from German persecution; the race mania of the Nazis backfired on this occasion and on many others. A second point is that the ruling group in Germany knew nothing about science and had no sound relations with those who did. This is well shown by the fact that German scientific results then flowed freely across international boundaries, even though Hitler was clearly headed toward war. Of course there was a long, long road from the demonstration of fission to an atomic bomb, but Hitler, fortunately for the free world, at that time saw no need for scientists. It is pleasant to record, in this same connection, that after Denmark was invaded, Bohr escaped and again came to this country to help.

The whole concept of fission promptly came to a head in a conference on theoretical physics sponsored by George Washington University and the Carnegie Institution of Washington on January 26, 1939. At this meeting Enrico Fermi,[56] who had himself very nearly arrived at the full concept of fission in 1934, suggested that, when a neutron knocked uranium apart, more neutrons might be emitted in the process. If so there was a possibility of a chain reaction, the release of atomic energy, and a bomb.

After this meeting there was great activity and excitement among physicists. The key experiments were repeated within forty-eight hours in several laboratories. But it is important to recognize several aspects of the situation as it then stood. First, there was no need to prod the physicists, or to subsidize their efforts with government funds during a considerable period that followed. They automatically went ahead vigorously on their own. Second, the possibility of a chain reaction was by no means proved; it depended on how many neutrons emerged from the first atomic fission: if only one appeared, there could be no mass release of energy. In fact, during the year that followed, there was one group who initially felt that no chain reaction was possible and tried to prove it. If they had succeeded, this would now be a different world, but I am not at all sure a better one. Third, there was no real grasp on the part of anyone of the enormous engineering job that would be required if success were to be attained; actually it was a long time before this became fully clear. And, even today, there is a lack of understanding of the essential and courageous contribution of the engineers to the entire program, made clear by General Groves, and by Groueff and others.* The development of the atomic bomb falls into two phases: first the development of the underlying theory by the physicists, and the supporting and extending of that theory by ingenious resourceful experiment, and then the reduction to practice, at enormous expense and with the overcoming of rugged obstacles by the engineers. And, be it noted, in this second phase, the chemists and chemical engineers made full and essential contribution.

As the intense work in the field of physics proceeded, an advisory committee was set up to coordinate efforts and exchange information, under the chairmanship of Lyman Briggs,[57] then director of the National Bureau of Standards. It included prominent physicists and representatives of the Army, Navy, and Bureau of Standards. It resulted from numerous conferences of those who were then thinking of possible military uses. Also there was a letter written to President Roosevelt and signed by Einstein urging support. How much this had to do with formation of the committee is not important; the committee was formed and did good work. The letter may have stirred the President's interest; I just do not know. He never mentioned it to me, and I feel that he did not really grasp what might be involved until much later. When he did grasp it, he certainly supported the effort vigorously.

When N.D.R.C. was formed in June 1940, the advisory committee was transferred to become a part of that organization. This was done by a letter signed by F.D.R. In his book *Roosevelt and Hopkins*,† Sherwood [58] cites it and says, "Bush probably wrote that letter himself." Of course I did. The step was generally welcomed and the nature of the work of the committee was supplemented rather than altered by the transfer. It gave the committee access to funds without the necessity of going through the sometimes circuitous chan-

* Able accounts are given by Major General Leslie R. Groves, *Now It Can Be Told* (New York: Harper & Bros., 1952); Stephane Groueff, *Manhattan Project* (Boston: Little, Brown, 1967); William L. Laurence, *Men and Atoms* (New York: Simon and Schuster, 1959). Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World* (Pennsylvania State University Press, 1962), Vol. I of a history of the United States Atomic Energy Commission, is carefully researched and rich in detail.

† Sherwood, Robert E., *Roosevelt and Hopkins*. New York, Harper & Brothers, 1950.

nels of the military organization. When O.S.R.D. was formed and I became its director, the committee, later called Section S1, became an operating rather than just an advisory body, and Conant,* acting as my deputy, took over its supervision for me. No great expenditures were needed at first, but they soon mounted. Conant assumed the full burden of administration of the effort, and this became a heavy burden indeed as the program ramified.

I do not need to review the long series of studies and reports on atomic energy during this preliminary period when it was decided to go full-steam ahead. This has been well treated by Hewlett and Anderson, and more briefly by Smyth.† I wish to add only my personal attitude as the subject progressed, an attitude which I believe was shared by my colleagues. I had no illusion as to the gravity of the decision. I knew that the effort would be expensive, that it might interfere seriously with other war work. But the overriding consideration was this: I had great respect for German science. If a bomb were possible, if it turned out to have enormous power, the result in the hands of Hitler might indeed enable him to enslave the world. It was essential to get there first, if an all-out American effort could accomplish the difficult task.

Many, I imagine, think that the great decision must have been made as follows: The scientists prepared a program, they presented it to the President and the senior military command, there were a series of top-line conferences, and it was approved and launched. No such thing occurred, nor could it occur. If there had been such meetings the only thing the scientists could have said was, "Competent groups think a bomb can probably be built, but do not know the real prospects of success, or the probable cost." If they had proceeded beyond this into the physics of the subject they would not have been understood. Neither the President nor the Joint Chiefs of Staff understood a great deal of modern physics. What actually happened was that the scientists and engineers went ahead on their own, slowly at first, but with gradually increasing momentum, and were supported by the President who had confidence in them. They were also sup-

* See the account in his comprehensive *My Several Lives.* New York, Harper and Row, 1970.

† Smyth, Henry de Wolf, *Atomic Energy for Military Purposes.* Princeton, Princeton University Press, 1946.

ported by Secretary Stimson for the same reason. Roosevelt did not attempt to delve into the subject, to balance one piece of advice against the other. As far as I know, he never discussed the subject with anyone besides Mr. Stimson, General Groves, and myself until it began to enter its final phases. But he certainly did understand, as we approached use, the implications for civilization.

There is just one point I should make clear to supplement the record, which has been adequately presented. The studies of possibilities, the examination of severe technical obstacles in the battle for success, centered in Section S1. Since I am no atomic physicist, most of this was over my head, although I could understand some of the technical problems. In my summary presentations to Mr. Stimson and to President Roosevelt, my job, as usual, was to furnish the best, most reliable, opinions of those in a position to analyze fully. Both men knew that this was what was being done and were thus satisfied without probing beyond me. The converse was also true; those from whom I sought these opinions for relaying also found the system satisfactory. There were one or two impulsive actions—impulsive but well intended—by one or two people, for instance, Ernest Lawrence;[59] but no difficulty ensued. Alfred Loomis'[60] kinship with Mr. Stimson (they were cousins) might easily have created a problem but for Alfred's care to avoid it. In general the whole team was content to stay in channels, as long as the members knew that the channels were not blocked and that there was no assumption of omniscience at the top.

As the program mounted in volume and intensity, I began to be a bit worried about this simple relationship, where on the one hand I received through Conant the developing plans and increasing costs and, on the other hand, had opportunity to tell F.D.R. only the bare outlines. So one day I said to him that I would feel more comfortable if I shared responsibility, if I had something like a board of directors. "Fine," said he, "we will set it up." And he did so then and there in the form of a Top Policy Committee, made up of Mr. Stimson, General Marshall,[61] Henry Wallace,[62] Jim Conant, and myself. Formal meetings were necessarily infrequent, for all were busy men. After the appointment, when I presented recommendations on the program to F.D.R., I would usually say, "This report has been approved by your Top Policy Committee." Actually, Mr. Stimson usually told me that I could assume General Marshall's approval, and this, coming from the Secretary of War, was not something I would care to examine further. I had no real discussions with General Marshall on atomic energy or on any other aspect of new weapons. He was just too busy to spread his efforts beyond the range he covered. He made great contributions to the war effort, in relations with the Allies, in placing the right men in high command, but he did not try to grasp the trends in novel weapons.

By December 1941 the program had arrived at the point of engineering and actual construction of plants for producing material for a bomb. I asked that a competent officer be made familiar with the program with a view to turning over the construction work to the Corps of Engineers. General Marshall appointed Major General W. D. Styer [63] to the task. He was competent all right; I never had the pleasure of working with a more competent and broad-minded officer. He was with the matter throughout, and the greatest contribution he made was in finding General Groves to head the Manhattan District. In his book Groves tells with a nice bit of humor how he visited me, thinking he had been appointed when he had not, and got a cool reception. And he also tells that we worked together cordially and became friends. Styer was right when he insisted that Groves was the man for the job.

Groves has told the story of the creation of the Manhattan District. It was set up for two principal reasons. First, it made no sense whatever for O.S.R.D. to try to create the enormous engineering and construction organization that would soon be needed, and the Corps of Engineers was fully competent to do so. Second, we were rapidly running into the expenditure of really large amounts of money, and it was far easier to obtain these through War Department channels. When the transfer of the project to the military was made, it was made completely. O.S.R.D. might have retained the scientific work and turned over just the engineering and construction, but it was far better to have the whole thing in one package. Scientists chafed a bit at times at being in a military organization, but on the whole the setup functioned well.

Part of the plan for the Manhattan District, as I worked it out with Mr. Stimson, was the creation of a Military Policy Committee. This consisted of myself as chairman, with Conant as my deputy, General

Styer, and Admiral Purnell.[64] It served as a sort of board of directors for General Groves. It met frequently, had no staff or secretary present, kept no formal records, but provided a point at which every important move could be discussed and closely examined. I know its existence helped Groves. No one who has not been placed in a post of heavy responsibility can realize what a lonesome feeling it is when there is no equivalent of a board present, and one reports to a chief who is rarely accessible. I felt this keenly in O.S.R.D., under the order establishing which all authority resided in the director and was exercised by his delegation, in contrast with N.D.R.C., where the authority resided in the Committee itself. True, in O.S.R.D. I had the Advisory Council with whom to go over broad plans. But this is not the same as appearing before a controlling board, where one is called on to present and substantiate his plans, and from which one can emerge with a feeling of renewed confidence. The atmosphere in the Military Policy Committee was a friendly one. But it went into tough problems in a tough manner and ducked no issues. For example, with full information before it, it reviewed the plan to place Robert Oppenheimer in charge at Los Alamos. Time, in spite of some incidents that do us no credit as a country, has shown well that no error was made in his appointment.

This completes the account of the main line of organization concerned with new weapons during the war. There were other elements that appeared when it was a question of whether and how to use the bomb itself. Much has been written on this,* but there are a few further words to set down.

Again this is not intended as an addition to the formal record. I just wish to record some of the thoughts which were in my mind as the time approached when a decision must be made whether or not to use the bomb. I knew that Japan would succumb within a matter of months even if the bomb were not used. But I also knew that an invasion of Japan was already being mounted, that it involved several hundred thousands of estimated casualties, and that once rolling, it could not be stopped in its tracks. I also felt sure that use of the bomb, far less terrible in my mind than the fire raids on Tokyo, if it

* Feis, Herbert, *Japan Subdued* (Princeton, Princeton University Press, 1961), is an admirable review of this troubled time.

brought a quick end to the war, would save more Japanese lives than it snuffed out.

But there was another aspect to this heavy subject. By that time I knew that civilization faced an utterly new era, and I felt that it might as well face it squarely. I knew that nerve gases, delivered in a dozen different ways, could be as terrible as an A-bomb. And I had no illusions about the potential power of biological warfare. When science became really applied to warfare, which occurred only during World War II, it presented humanity with two alternatives. Either it could refrain, formally or informally, from use of weapons of mass destruction—not only the bomb but also gases and bacteria and viruses—or it could thrust itself back into the dark ages. Over twenty years have passed, and the world has understood and has thus far refrained. If for no other reason I would justify the use of the bomb at Hiroshima and Nagasaki because it was the only way in which the dilemma could be presented with adequate impact on world consciousness. Can the stalemate continue until the world becomes more sane? I think it can. And organized effort can expedite that purpose.

The subject of organization should not be left without some treatment of the form it took after the war ended. A complete review would require a book by itself, or many books, but again my object is just to fill in a few gaps, and to add some comments, on parts where I was myself involved. This will not take long, for about three years after the war ended, the stresses—and I have to add also the frustrations—finally caught up with me; I proceeded to fold and go out of circulation, and by the time I recovered my balance the whole relation between government and the scientists and engineers had taken new forms in new hands. I only hope I backed out of the scene gracefully.

There are two aspects of postwar organization which have since proved to be of great importance. First is the mechanism by which government has supported research in industry and the universities; second is the form of relationship within the Department of Defense between military men and civilians, and between the armed services, on the development of weapons. After the war ended, we might well have gotten ourselves into a serious tangle on this whole matter of government subsidy of research. When large amounts of money flow, from taxes, into an effort which the public, and to a considerable extent its representatives, cannot understand, there is real danger present. It can take the form of support of the inconsequential, of bureaucratic control of universities, of waste, and of downright scandal. As we look back I believe we can take pride in the fact that we escaped all these dangers to a truly remarkable extent over the years. And the fact that the momentum of the application of science as the war ended was not lost has had much to do with our present national strength, in an industrial and military sense, and also in our standing among nations in the pursuit of science.

The whole program started when President Roosevelt toward the end of the war called on O.S.R.D. for a report and recommendation on postwar science. It was soon possible to gather together committees on various aspects of the problem, for the men who could contribute were already working together. It did not take five years to come to conclusions, as it sometimes does on such matters; it took only a few months, for there was an extraordinary consensus of opinion. The result was entitled *Science the Endless Frontier*. It called for heavy federal support of the scientific effort in the postwar scene.

Not that there were not present those who feared such a program would be disastrous. Frank Jewett, as good a friend as a man could have, certainly thought I had gone berserk when I endorsed the recommendations of the various committees, joined them together, and sent them to the President. He was sure that we were inviting federal control of the colleges and universities, and of industry for that matter, that this was an entering wedge for some form of socialistic state, that the independence which has made this country vigorous was endangered. And there were some, I feel sure, who thought this was some sort of a grandstand play by which a chap named Bush was trying to perpetuate into the peace the authority he exercised during the war. These latter were very far off the mark; I was as anxious to get out of government as were nearly all of those who manned the war laboratories.

That some form of organization was inevitable to carry on the effort in atomic energy was clear. I do not need to review the juggling which went on before this settled into its final form. The Atomic Energy Commission has written its own history. This omits some of the clash of personalities and the strange relations with the

Congress. But I had personally no great part in the affair. My principal comment is that it finally evolved into a well-managed, sensible, vigorous effort, and that the present and future use of atomic energy for peaceful ends has certainly justified all of the effort and expense which has gone into it.

It was inevitable also that the armed services would themselves continue to support research in civilian hands along the lines of their special interests. The prompt establishment of the Office of Naval Research for the purpose is a notable example, which I was glad to help on its way.

But there would still remain a serious lack if the program included no more than this, for basic research, fundamental research, would hardly be supported adequately by those with special interests. This gap was filled by the formation of the National Science Foundation.

To persuade the Congress of these pragmatically inclined United States to establish a strong organization to support fundamental research would seem to be one of the minor miracles. We in this country have supported well those pioneers who have created new gadgetry for our use or our amusement. But we have not had during our formative years the respect for scientific endeavors, for scholarship generally, to the extent it had been present in Europe. There were some on Capitol Hill who felt that the real need of the postwar effort would be support of inventors and gadgeteers, and to whom science meant just that. When talking matters over with some of these, it was well to avoid the word fundamental and to use basic instead. For it was easy to make clear that the work of scientists for two generations, work that had been regarded by many as interesting but hardly of real impact on a practical existence, had been basic to the production of a bomb that had ended a war.

So Congress passed the bill and President Truman promptly vetoed it. There is a point here which warrants discussion. Truman wanted the director of the Foundation to be appointed by him and to be responsible to him, with the Board in an advisory position only. For an organization which was to work closely with universities, this was hardly the way to inspire confidence. When the bill was passed the second time, I managed to convince Truman he should not veto it again. But I did so on the basis that he was being given protection, a buffer against those coming to seek favors. So we finally have a

Foundation with its director responsible to a Board, and it works. For one thing, a director operates with more assurance when his program has been approved by a widely representative board that he has convinced of the program's soundness, rather than when he reports only to a more or less inaccessible President with little time for his affairs. There is a vast difference between an authoritative board and one that is advisory only. Board members that have distinct responsibility take their duties very seriously.

We have somewhat this same general setup in the Atomic Energy Commission, except that that Commission is small, and itself operates. It works smoothly today because the chairman, Dr. Glenn T. Seaborg,[65] is a man of deep wisdom, but the Commission had its troubles in the early days. A larger board, that just selected and supervised a director, would have saved some headaches.

We had excellent organization in the old National Advisory Committee for Aeronautics, a board made up of military officers and other government officials, plus citizens appointed by the President, which selected a director and reviewed his operations. When this was transformed into the National Aeronautics and Space Administration, it took the form which President Truman preferred. I think this was a mistake and said so at the time, for I have great sympathy for the director who stands alone before the public and the Congress. But the men who organized it did not agree with me.

The second problem to be solved, if all was to go well in the postwar world, was that of coordination of research and development within the military services. This was just one part of the broad problem of unification, but an important part. We had certainly learned during the war the need for unitary command of armed forces in the field. It was equally clear that unification was needed at home; it no longer made sense to have two, and then three, services reporting only to the President and otherwise going their own independent ways.

There was much opposition to unification. Some of it was based on the fear that there would emerge one man in uniform with absolute control over all our armed forces, who would be tempted to defy the civilian authority. I never felt there was great danger that a chairman of the Joint Chiefs of Staff would ever become, in this country, a menace. But there was also opposition because services preferred to play their own games in the contest for appropriations. Nevertheless unification occurred and operated, confusedly at first, but gradually better until we now have a system which makes sense. Its central theme is, or should be, that the civilian authority is supreme but leaves military judgment to military men, while making sure that those who judge are the most competent officers to be found. And with the corollary that the Joint Chiefs recommend the men to take command in the field, support them on the home front, but do not try to manage a campaign from a distance.

It has worked out well in the end, but there was a mistake at the beginning. Forrestal[66] should never have become Secretary of Defense. He had vigorously opposed unification when Secretary of the Navy. To undertake then to operate the combined services placed him in an impossible spot, as I feel later events proved.

Into this I was injected, and I probably should have had sense enough to stay out. I became chairman of a Joint Research and Development Board, a voluntary association at first, and later established by law. It was supposed primarily to coordinate programs between the services and to plan future research. I took the point of view, as presiding officer, that my job was not to plan but to see that the officer members got together and did so. But this was not easy going. If the Navy and the Air Force each had a program aimed at putting a satellite in space, as they did—and a tough program at that, for there was then no guidance system in existence which would work for the purpose—my job was to see that it became a joint and fully coordinated program. But it was not my job to try to dictate it. Rather I should provide the means and the forum whereby the officers directly involved could accomplish it. And, if they didn't, then as the representative of the Secretary I should insist on officer members that would. However, with Forrestal in the position where he hesitated to rule either for or against the Navy, I couldn't get very far.

It must not be concluded from this that we did nothing whatever. Quite informally, as a result of simple good will, a lot was accomplished in moving toward a more effective relation between services. This was helped by the presence of a number of highly able officers, many of them old friends of mine, and men of combat experience. It was probably too early to expect more than this.

What, after all, is an organization? It is merely the formalization of a set of human relations among men with a common objective. The form of organization is important. Far more important are the men themselves, and their insistence on working together effectively for a common end. The United States progressed rapidly on new weapons and new medicine during World War II. Part of this advance was due to the fact that there was a body of science standing ready to be applied. Part of it was due to the fact that there was organization which worked. But the central reason was that military officers, scientists, engineers worked together effectively in partnership. This country faces many severe problems in addition to that of avoiding all-out war. They can be solved only if professional men insist on working together with intelligence, devotion, and mutual understanding. When we look beyond our own concerns to those of the world as a whole, to the continuance of the stalemate and ultimately to the elimination of the need for it, we see that organization in these terms, by men of good will, is the vital matter. The question before us is whether men in power can become reasonable before they become exterminated. To this end we must be sure we do our part to make the stalemate last, and to establish understanding with all those willing to understand.

III

Of Stumbling Blocks

THE more complex a society, the more chance there is that it will get fouled up. If it gets to be complex enough, just one small detail can throw it all out of gear. It is like a television set with a thousand electrical connections arranged to present Mr. Cronkite or Mr. Brinkley for our edification; one wire becomes unhooked and the whole thing goes poof. Something of the sort happened in 1929; the blooper was a million innocents loaded with lots of money to gamble on margin in the stock exchange. It could happen again, for some other reason. In a complex society, too, there is a wonderful chance to set up roadblocks. We have all seen them—the stumbling blocks of smugness, greed, conceit, inertia, empire-building. Some are placed by the stuffed shirt who takes pride in putting others, especially younger others, in their places. We remember the big toad in the small puddle who thinks he is still the chief toad when the puddle has become an ocean. Beside him belongs the dull-brained traditionalist who was doing something in a set way forty years ago and sees no reason to change now. One could multiply examples, but to do so is hardly necessary. Every reader can supply his own.

When he knows that stumbling blocks may get in the way of a joint effort in which he is engaged, a man who is light on his feet tries to anticipate them, to figure out where they may arise and why, and how best to evade them. This means that he tries to figure out strategies—even stratagems, if necessary—to get them out of the way so that the work can go ahead. Planning strategies means taking into account the personal quirks of some individual who almost always is the source of the blockade, and devising ways to annul his blockade by disarming him, by avoiding him by an end run, or, if necessary and possible, by knocking him on the head, figuratively of course. The justification is the importance of the work and the depth of one's commitment to it.

These hazards occur whenever men must work together in organizations—in business, campaigns, research, what have you. They are found in military undertakings, especially when collaboration between civilians and the military is vital, as in time of major war. In this chapter I shall discuss primarily a particular sort of stumbling block. It appears when an officer in high command has a fixed conviction after technical progress has made it obsolete. It also occurs when a relatively unintelligent individual suddenly boosted to control of important military procedures, as is bound to happen occasionally under the stress of war, throws his weight around for effect. Unfortunately it also occurs because highly capable officers become so severely burdened that they just cannot find the time to keep up to date. It occurs in especially difficult form when some section of the vast military organization has developed special skills and the vested interests that go with them, and has walled itself off from interference not only by civilians but also by other military branches or even by the high command. However this sort of thing occurs, it is a lame leg in the march of progress and we need to overcome it or avoid it. Sometimes in civilian-military joint efforts, barriers are salutary, for they are no more than a justifiable defense mechanism against interference by cocky amateurs or conceited authority, as will be discussed in the next chapter. But we are most concerned here with obstacles to progress. A bit of review will be in order, for the same human traits operate today as always.

The United States came very close—too close—to being defeated in each world war by the submarine. This is a fact that we should not forget. And we should be equally mindful of the reason why we ran so near to the line of disaster. It was the fact that we needed, and did not have in the beginning, an effective collaboration for the development, not merely of defense, but also of retaliation and counterattack against the submarine. And because we did not, we lost precious time and nearly lost the war.

We were fortunate enough in the second war to get such a joint effort working in time—that is, to bring it about that the groups concerned sank their differences and ultimately entered upon genuine collaboration. In the first war, I saw the beginning of this process in a small way; it was weak, and the collaboration, even toward the end of the war, was nothing to be proud of. I made my first acquaintance with submarines during that war, in a program on antisubmarine devices, specifically on short-range submarine detectors. My efforts did not cause the Kaiser any embarrassment, but they did teach me a bit about how to develop new weapons in time of war, or rather how not to do it.

The story of the submarine at that time was of course very different from what it was in the second world war. Submarines could not then descend to great depths, so they preferred to operate in rather shallow waters. Their usual procedure, when they could hear the propeller of a destroyer or of one of the wooden antisubmarine boats chasing after them, was to sit on the bottom, turn off all their equipment so that they would make no noise, and simply wait until the pursuer went away. These were perfectly good tactics. The U-boats could not sit down in water that was much more than a hundred or a hundred and fifty feet deep, but waiting it out at that depth completely baffled the attack. There was thus a need for something which could locate enemy U-boats when they were sitting on the bottom, and locate them accurately enough for surface craft to drop depth charges—ashcans—to clear them out. Several Navy groups and others went to work on the problem.

My own scheme was a magnetic bridge affair. I put an alternating current magnet on a wooden subchaser, with a pickup coil, and a rig for balancing out the voltage normally developed. It used the principle that has been used in many ways since, in mine detectors, even burglar alarms. One simply creates a field and so balances a receptor that, when the field is disturbed in any way, there is a signal, usually a tone heard in earphones. My crude apparatus gave a nice signal, suitable for dropping a depth charge. As one approached a submerged submarine, the signal tone gradually increased; when one was just over the target the tone abruptly dropped to zero; then it picked up again and died away as one left the area. I remember a test on a tame submarine sitting on the bottom to which was tied a small boat with two sailors in it. I was below in the research vessel—a wooden subchaser—at the instrument, and on my signal a smoke bomb was to be dropped over the stern to test the accuracy of the apparatus. After sitting a long time with earphones on my head, I finally got a signal. It dropped to zero. I yelled "Fire," and went on deck. All I could see was a cloud of smoke with an oar sticking out of it and beating away at the smoke bomb, smack under the bow of the small boat. Of course a proper understanding with the gunners who fired that smoke bomb had helped accuracy a bit.

The arrangement for submarine studies at that time was this: The National Research Council had a committee on antisubmarine warfare, of which Robert A. Millikan [1] was chairman. This civilian group was working with a committee of naval officers. Thus there was a partial system for collaboration. But the trouble was that the civilian committee had no money. All it could do was recommend. Although military officers became avid for a new development once it had thoroughly proved itself in the field, they were not at that early date likely to get enthusiastic about a weapon in embryo. I will come back to this later. I presented my idea to Millikan, and somehow I did not seem to hit it off with him. Hence I never appeared before his committee or the Navy committee.

At that time, though, I was consulting for a little company called AMRAD, wholly owned by J. P. Morgan,[2] which I shall mention again later in this book. It was much easier to convince the AMRAD people than the constituted authorities that they should support my submarine detection idea. I may have implied that the device would have use, after the war, in tracing ore bodies or the like. At any rate, I spent Mr. Morgan's money throughout the war, and this gave me a unique measure of independence. I do not know whether Mr. Morgan knew how his money was being spent, but I am sure he would have approved if he had. Since I was not in uniform and took no government money, I was a maverick; in my youth I had been taught that the most independent being in existence was a hog on ice, and I emulated a hog on ice. When the war ended, I went into uniform as a Naval Reserve officer, to show there was no alien feeling, and had many years of pleasant association in the Reserves.

When I look back on it, I think what a crude idea it was that I was pursuing. Soon after I started I found that my generator had lots of harmonics in its output which would give unwanted noises, and I had to get rid of them—filter them out. I knew a bit about the theory of filters, but almost nothing about the practice.

Colonel R. D. Mershon [3] was the Army officer attached for liaison with the Naval committee. He helped me more than the entire Navy. He introduced me to Michael Pupin,[4] who was supposed—I think erroneously—to have invented filters, and I visited Pupin in his New York office. The visit was not helpful. He made me sit at a distance from his table so that I would not see the profound memoranda, if any, thereon. I came away with the conviction that one of three things was true: he thought there was no use trying to help me, or he did not care to assist in the antisubmarine campaign, or he did not understand how a filter worked. I believe it was the last; certainly the explanation of filter operation he gave me did not make sense. But the Bell Laboratories presented me with a nice filter that worked, and my harmonic problems were solved. They could make good filters because Campbell [5] had worked out the theory correctly and completely, as Pupin had not. Campbell was a giant in my eyes. Many years later, I had opportunity to pay just tribute to him when his collected papers were published.

All seemed to be going well. I detected many tame submarines; a test to find out whether the instrument would stand depth explosions was successful. That test nearly wrecked the subchaser, but the device still operated. Then came a shock. The Navy insisted that the gadget was of no use on a wooden ship; it must be put on an iron ship, a destroyer, for example. I was pretty sure it could not be done. The magnet and coil would have to be draped over the side, for the alternating magnetic field could not penetrate the steel plates of the hull, and the reflections from the varying water surfaces would raise hob. But I was young and foolish, I did not get a real chance to argue my case, the decision to use a destroyer had been made by some senior officers who knew even less physics than I did, and so I went to work and wasted six months trying to adapt the equipment to an iron ship. Then a decision came down from the heights that, after all, they wanted it on wooden subchasers.

So at AMRAD we put the device into production, making one hundred sets. We even got three of these to the North Sea where they were installed on British subchasers. They were technically successful and detected British submarines. Then the war ended, and no set had ever found a single German submarine. But I had learned quite a bit about how not to fight a war. That experience forced into my mind pretty solidly the complete lack of proper liaison between the military and the civilian in the development of weapons in time of war, and what that lack meant. If there had been any such thing as our later system in operation at that time, the problems would soon have been thrashed out, and a competent committee, made up of military men and civilians, would have decided that the detectors ought to be rushed into wooden ships and that the conversion for iron ships was absurd.

One other point on the lack of liaison and what it meant: Toward the end of the first war there were several other groups, about whom I knew nothing, working on this same problem. One group was trying, by trailing a wire which had some nonpolarizing electrodes on it, to pick up stray currents from the submarine resulting from the electrolytic effects produced, for instance, between a bronze propeller and a steel hull. I did not even know they were working on it until one day down at New London I went aboard a craft and saw the gear. I asked them what they were doing and they told me. I asked what troubles they were having and they said, "The stabilizing of the galvanometer on shipboard sufficiently so that we can get proper sensitivity." I said, "Why don't you use a pivot instrument? Weston Instruments has just gotten out a very sensitive microampere meter of this form." They had never heard of it. They got one and put it on deck, and it promptly proceeded to work all right.

The point is that their scheme was a lot simpler than the gadget I was developing. It was not as good in some ways, because my device signalled by the cessation of the tone when the seeking ship was immediately over the submarine and theirs gave the signal later, but their device was simple, was cheap to build, and could have been put into use in quantities in a hurry. But it also never did arrive. I think if I had known about that system and known that there were nonpolarizing electrodes that would work that way, as I did not, I probably would have seen how the two ideas could have been combined. The reason they were not was that there was no centralizing group able to bring together parallel efforts and compare them.

Another incident in point is that Ernest Fox Nichols [6] was also working on magnetic submarine detection, using a dipping needle affair. One day he came aboard a yacht on which I had my stuff installed. I was engaged in adjusting the rig by detecting a locomotive that went by on a track, a hundred yards or so away. When he found I could do that rather positively, he told me his instrument could never equal mine when operated on shipboard, and I believe he abandoned his program. Here, again, was a case of complete absence of correlation between similar efforts.

Twenty-odd years later, as we faced an even greater war, we were in some senses better prepared than we had been before. The peril confronting us was far more complex, it is true, but we did have the considerable advantage that our organizations, military and civilian, were more versatile and adaptable, and especially that civilian science and technology were focused in an organization that was not limited merely to making recommendations but on the contrary had ample funds of its own with which to initiate projects and to press forward with work. There still remained, however, the crucial matter of evolving or establishing that true collaboration through which the several agencies, military and civilian, American and British, might fully and genuinely support one another in the great endeavor. Again, the story of submarine warfare well illustrates the problem and what was done about it.

We nearly lost out again in the second war because of the submarine; the margin was critically close. One reason was that on entering the war we were altogether too cocksure on the subject and were slow in getting innovations into effect. The system of bouncing echoes off a submarine, called ASDIC by the British and SONAR by the Americans, had been developed during the peace, and it was thought to provide an answer. Certainly it was thought—that is, thought by the Navy with no real consultation with anyone else—that when a submarine once came within the range of a destroyer equipped with sonar, it could not get away, and would be followed until depth-charged to destruction. I never heard of adequate peacetime tests; perhaps there were some. But, as experience later showed, there were lots of tricks that could be worked by a smart skipper on a submarine, and the depth charges of the time, the ashcans, unchanged from the first war, were absurdly crude in operation, while the speed, allowable depth of submergence, and maneuverability of the submarine were much improved. However, when N.D.R.C. was formed the appropriate admiral made it crystal clear to me that the Navy did not want any help from anyone on anything concerning submarines. Incidentally, this was about the only case of overconfidence we met during the war.

This matter of ashcans will bear emphasis. Only after several years of the second war did the top brass finally discover that such depth charges were not much good; they sank so slowly that a deep submarine had plenty of time to get away. Destroyer skippers said that sometimes they even boiled back up to the surface in the wake of a fast moving destroyer. Yet the Navy entered the war with the ashcan as their only method of destroying a submerged submarine. They had not built explosive charges that would sink rapidly; they had not even made tests to find out how fast an ashcan would really sink. Such a test could have been made in an afternoon by two sailors with a piece of knotted string and a stopwatch. But for two years or more, crews of young men struggled with rough seas, watched merchant ships they were guarding go down, torpedoed; indeed, lost their own lives, while supplied with our absurdly inadequate weapons.

Anyone who reads the accounts of antisubmarine warfare in the first years of World War II is bound to be impressed by the frequency with which depth charge attacks were made with no result. It is easy enough now to see why. The U-boats could submerge to several hundred feet, and do so about as rapidly as an ashcan would sink. A fuze on the ashcan had to be set to explode it at a chosen depth. It was later established, too, that the ashcan had to explode within some thirty feet or less from the submarine hull if it was to cause fatal damage. The chances of a successful attack were small indeed. All this should have been known, and acted on, before the war started, not after Britain was on the verge of being starved out. It could have been acted upon in time even after the war started in Europe, had it not been for self-imposed ignorance or stubbornness in high places.

How can an able organization such as the United States Navy (and it is and was able) get into such a frame of mind? Stubbornness was also shown plenty of times later, by delay on hunter-killer groups, by torpedoes in the Pacific which wouldn't go off. I think there are two reasons. First, the organization was tight, and all decisions were made by a top brass old-fashioned technically. Second, these decisions, once made, were not reviewed, were not even commented upon, by anyone whatever. This is serious business in a modern world. Take the matter of battleships. After the sinking of *Prince of Wales* and *Repulse* near Singapore in the fall of 1941, it was evident that the days of the battleship were numbered. This disaster occurred right after Pearl Harbor. Two of the finest, most modern British battleships left Singapore without proper provision for air cover, depending on their own antiaircraft guns for protection. They were attacked by Japanese land-based aircraft and swiftly sunk with great loss of life. Yet we had plans to build a whole fleet of magnificent new battleships, which would never see real service, and it was only after Midway that the situation was fully grasped throughout the Navy. Midway was the turning point of the Pacific war. It was a battle between aircraft carriers almost exclusively. We won, with forces that were far inferior, because of the magnificent judgment and energy of the top naval command, the devotion and sacrifice of our pilots, and, above all, the skill of our intelligence officers. During the whole battle a fleet of Japanese battleships stood off and never entered the fight, and after it was over they just went back to Japan.

But weren't the flaws in such decisions as that to complete battleships after their days were clearly numbered revealed on review in the Joint Chiefs of Staff? Not at all; the other chiefs would never claim competence on a Navy matter. How about the appropriations committees of Congress? They were hardly set up for a technical and strategic review, although they could have insisted on one. But there were plenty of keen young naval officers who knew their way around. Certainly they did; they knew it too well to try to argue with the brass. How about the President? F.D.R. knew a lot about the Navy. He did not force his opinions on military men, and hardly had time to review such a matter thoroughly. Churchill would have made his own judgment, whether his grasp was or was not complete, but interference by lay civilians in technical and strategic military matters is not the way to do it. What was needed, what is still needed, is complete searching examination by a competent professional group, the members of which have no personal stake in the outcome.

I am here entering upon a very controversial subject, namely, the extent to which the civilian authority should enter into military decisions. I know exactly where I stand on it. It breaks down into two distinct parts. First is the question of command in the field, and we have finally gotten this into good order. In any theater of war there should be a single chief commander with full authority over all forces in the theater, land, sea, and air. His mission should be given to him by the Joint Chiefs of Staff after approval by the President. This approval is given by the President on the advice of his Cabinet. This is essential, for missions have political and economic repercussions. We should not have much doubt on this point as matters now stand. The Joint Chiefs of Staff should have no power to alter such a mission, once assigned, without securing renewed approval. Nor should they interfere in the slightest degree with their commander in the field as he carries out the mission. They should watch him—closely—and if he falters they should promptly recommend his relief and replacement. They should operate the home front to ensure that he is furnished men, supplies, and weapons. They should, in so doing, balance the needs of the several theaters in accordance with the approved policy as missions are approved or altered. But they should not interfere with the judgment of the field commander. All this is now well accepted. It is interesting, in this connection, that during World War II the President of the United States, under the Constitution the Commander in Chief of the Armed Forces, did not interfere with the judgment of military officers on military matters, while the Prime Minister of Great Britain, not formally or legally Commander in Chief, did interfere. He interfered by suggestion,* it is true, but his were pointed suggestions. He also inspired his country to a supreme effort when the cause seemed almost hopeless; let us not forget that as we individually pass judgment.

The second part of the problem concerns the planning, development, and procurement of systems of weapons, and this goes on both in peace and in war. It involves bombers versus missiles, large carriers versus small carriers, transportation and bases, and emerging systems still in embryo. Here the final decision should not be made by a

* He showed one such to Eisenhower before he sent it. Ike said to him, "If as an American commander I received such a message from the President of the United States, he would expect my resignation to be on his desk tomorrow morning—and I would make sure that it would be there." Eisenhower, Dwight D., *Mandate for Change, 1953–1956; the White House Years.* Garden City, Doubleday, 1963, p. 31.

single individual with unlimited authority. Yet it should be made within the military structure and should not be imposed from above by the civilian authority. What the civilian authority, the President, should do is to insist that what is done be done well. This involves, of course, deep study and planning, the opportunity for dissenting opinions to be fully expressed, the bringing to bear of diverse disciplines, financial, economic, scientific, engineering, management.

But it should involve one more thing. The final program, of course, must be approved by the President and the Congress. It should also have approval, after study, by a highly competent professional group, not made up of those who constructed the plan, nor those who will carry it into effect, but by a predominantly military group with some outstanding civilian membership and full civilian professional advice, and above all, a group that is genuinely independent. Can such a group be assembled? It certainly can. There are plenty of vigorous, alert retired officers who would be proud to serve in such a capacity. They may be a bit out of date technically, but they would not long remain so if surrounded by keen young officers and civilian technical advisers. There are eminent civilians, with sufficient military experience to understand, who have public respect, and would be willing to serve as well. In addition to its great direct value in providing impartial objective judgment of a proposed military development, such a reviewing group would make another highly desirable contribution. This would appear in the delicate relations between the military services and the universities. The military is bound to call upon the universities for help, for many of the best scientists and engineers are there located. It would certainly do no good, to the country generally, to try to attract them away. Moreover, the universities are not, and should not be, concerned with whether or not a program results in commercial production. But a university and its faculty certainly need assurance that a projected program makes sense. They should not be called upon to render this judgment themselves, but they should be assured that judgment has been made by a fully competent body. The university should itself then judge whether such a program fits into its affairs appropriately, whether its presence will enhance the university's primary functions of educating men and advancing knowledge. It should also judge whether the pursuit of the proposed objective will or will not benefit the general esprit de corps of its entire organization. But it should be asked to carry on only things that have been fully studied and endorsed after such study, and to carry on development only, not production or the supervision of production.

The question is sure to be raised whether such a reviewing group will not take over some of the functions of the Joint Chiefs of Staff. The answer is yes. And the only way for the Joint Chiefs of Staff to protect themselves is to see to it that the review boards are formed, and are adequate and fully supported. But will not the presence of a reviewing group cause delays? A properly manned board will prevent them, by substituting sound professional judgment for bickering, horse trading, and stubborn blindness. When we get this sort of thing we shall have genuine, intelligent, central military planning, fully coordinating our three armed services. We shall have one war plan instead of three more or less interrelated ones. But we will not get such a system until we have a President and a Secretary of Defense who both understand and insist on it. As it is, the subject doesn't even appear in the political platforms. Yet it is central to our defense.

To return to our review, it took patience, skill, good fellowship, and time for O.S.R.D. to bring about cordial relations with the Navy on all such matters as antisubmarine devices, and that such relations finally eventuated was due to such men as Jack Tate, Tim Shea, Keith Glennan,[7] all of whom contributed mightily. It was also due to the movement into positions of authority of able young naval officers, who knew how to work with civilians, and also knew how to work around reactionary brass.

The U-boat was finally brought under control in World War II by a dozen or more new devices and methods. It is interesting that, in the same period, the Germans produced only one really new advance on the submarine itself, namely the schnorkel, and they produced that too late to be of much help to them. Dictatorships have to use, for the safety of the dictator, rigid lines of authority, and rigid schemes do not produce the best innovations. The same thing is illustrated in the atomic energy field, where the Germans got nowhere, and the Japanese did not even start.

Nor can a dictator be flexibly minded; he may suddenly find his flexibility somewhat overstrained. The best atmosphere for an independent agency is in a democracy. Even there it cannot be made to work, in peace or in war, unless the chief executive understands and supports it, unless he insists on it.

One incident illustrates nicely the advantage of an independent civilian activity on weapons, and it also illustrates the value of independent military command in the field. The German submarines constantly made radio reports to home base by radio, presumably because the German High Command did not dare to leave them on their own. By direction finding on their radio transmission, we—and the British—could tell to a remarkable degree where the submarines were. We also could to some extent learn what they said. This fact helped to route convoys away from them, and it helped to find them when hunter-killer groups came into use, and long-range aircraft were used as submarine killers. But a way in which to direct the hunters accurately to the position of the hunted was much needed, and this soon appeared. As it did there nearly appeared a crack in the fine British-American scientific collaboration.

The British got there first. They developed a system by which a ship or an aircraft by precisely timing the arrival of radio pulses from a number of shore stations could determine its own location, with truly remarkable precision. This system was of such great importance in their bombing program, as well as in antisubmarine activities, and was so vulnerable to enemy jamming that extraordinary measures were taken to keep it secret. The British told me about it, and also Conant, but asked us to let it go no further. We agreed, as this request was thoroughly in accord with policy under which sensitive information of possible great value to the enemy was not transmitted beyond those who needed it in performance of their assigned tasks. Then the Radiation Laboratory at the Massachusetts Institute of Technology came up with essentially the same idea in the form of LORAN, now used for marine investigation all over the world. Fortunately, the British believed us when we told them that what had occurred was independent invention, and fortunately the secret was well kept and jamming was not a serious problem.

As was typically the case, the American Navy and Air Force were only mildly interested in this gadget at first. The Navy made some tests—and attributed to the new method all the errors they found in position finding, which were mostly those of the old system used for comparison. For a while there was no excitement. Then, suddenly, everybody wanted the new equipment at once, the Navy for antisubmarine work, and the Air Force for its ferrying of aircraft across the Atlantic from Brazil to Africa. There were in existence only a few sets made by the Radiation Laboratory as a part of its research; to make more would take some months. In passing, it was important that the word "Development" appeared in the title of O.S.R.D. and that the Appropriations Committee of Congress, under the chairmanship of Congressman Cannon, saw the point and supported the idea that the laboratory could proceed beyond research and build hardware to demonstrate its accomplishments. So those few sets had been made by the Radiation Laboratory, but they were not nearly enough. There was a meeting in the Chiefs of Staff Building to sort out the matter, and I presided. The argument became warm, and officers ignored the chair and went after one another directly. So I tapped the table and said, "Gentlemen, you seem to overlook a point as you argue; I 'own' these sets." The discussion then became more orderly, and an agreement was reached. This seems like a small matter, but it was not. It illustrated forcibly that orders could be given to O.S.R.D. only by the President of the United States, and he never gave any. Collaboration usually occurs only between equals in authority. This does not mean that O.S.R.D. was accepted into the top military councils where strategy was determined. It was not, as is recited elsewhere in this book. But the agreement that was reached on this matter moved us a long way toward mutual respect, out of which only can arise genuine concert of effort in a common cause.

There were a lot of antisubmarine weapons that finally came into use, among them MAD, magnetic airborne detection of a submerged submarine, which made my World War I attempt at magnetic detection look amateurish indeed. The greatest of all was centimeter radar. Developed at M.I.T., this was a form of radar using very short wavelengths and hence giving great detail. It rendered an aircraft a powerful enemy of the U-boat. It was no minor accomplishment; the Germans never produced it, nor could they counter its use. There were also forward-thrown depth charges, Mousetrap and Hedgehog. Sonar was developed to give distance as well as direction, and to distinguish a moving submarine from stationary decoys the hunted submarine could plant. The most striking new weapons were the antisubmarine rocket and the Mark X mine, so called. The former was a terrifying weapon. I saw it tested early in Britain, and that test scared me plenty. An airplane would come in at treetop height, zoom up a bit, dive a bit, and as it pulled out with a great roar, release six rockets at a target. At sea these solid-headed missiles would take a long shallow underwater trajectory and would go clear through a submarine they encountered on the way, leaving great holes, often in inaccessible places, through which the sea poured in. The Mark X mine was a self-propelled target-seeking torpedo. Dropped in the swirl where a submarine had just submerged, it would listen to the sound of the submarine's propellers and steer itself to run into the submarine with fatal results. I want to tell three stories about these weapons, each with a purpose.

The first has to do with invention. I have noted that, of powerful antisubmarine weapons, one was British, one was American, and one was produced independently on both sides of the water. But an important point is this: In no case did I know who the individual inventor was, nor did I care. Moreover, the men in the laboratories did not care either. For one thing, once a problem became clear, the invention, if a useful one, was bound to appear, if not made by one man, then by another. For another thing, in general no one was looking for personal credit. Oh, there were a few with that motivation, on both sides of the water. But the general attitude in laboratories everywhere was, "The hell with the credit, get on with the job." That attitude was nearly universal, and it was genuine.

There was a patent department in O.S.R.D., operated by Captain Robert A. Lavender [8] of the Navy. There had to be. For example, consider the group at the Radiation Laboratories at M.I.T. They came from universities and institutions all over the country. Certainly it would be absurd for M.I.T. to own patents on the inventions which emerged. It would also have been absurd to have them the private property of individuals, if the equities in the matter could be sorted out; no one wanted this to occur, and to try it would have spoiled morale. Yet patent applications had to be filed, for otherwise, under the law, individuals who were by no means the original inventors could have obtained valid patents, and those would have been obstructions to cause untold confusion and inequity in the postwar world. So hundreds of patent applications were filed by Captain Lavender, assigned to me as Director of O.S.R.D., and by me dedicated to the public. Thanks to the good sense of the officers in Lavender's office, the whole thing was done with a minimum of annoyance to the laboratories, who accepted the system, once it was explained to them, as a necessary evil. I suppose that in the process I personally destroyed more property in the form of patents than any other man living, and this needs a word of explanation. A patent is property, created by the government under the statutes which became law under an explicit authorization in the Constitution. The purpose of issuing a patent is not to reward an inventor, but to enable the investment of venture capital, without which many inventions would die on the vine. The patent also enables small companies to carve out a corner for themselves in a large field, and to grow and prosper in the midst of large, powerful potential competitors. Our patent system has had some rough sledding at times, largely because the public, general attorneys, and especially judges fail to understand it, but it has nevertheless been responsible for much of our prosperity, and especially for the advent of small successful industrial units. I will touch on this again in another chapter.

So to destroy a patent, which is what is done when it is dedicated to the public, would seem to be a sin. It is paradoxical that I, who am a great believer in the system, should have been called upon to commit this particular sin. But I could not help it, if the war effort were to be uninterrupted by undue attention to a subject of no significance to that effort. In addition, the inventions on which I destroyed patents had no need for venture capital in their development. O.S.R.D. had supplied that.

The object of the second story is not a serious one. Skippers of tame submarines used in the research on countermeasures were disgusted with their assignments—they naturally wanted active duty, combat duty—and especially disapproved of the civilians who got them to do foolish things. One of them had to make his ship the target for early tests of the Mark X mine, and he expressed his discontent accordingly. But he duly submerged out in Long Island Sound, and the "mine" was dropped in the place where he had disappeared. It listened, took off, and rammed him, on the broadside. It did not carry a warhead, of course, but weighed a few hundred pounds, traveled at twelve knots, and packed quite a punch. So it made a dent, and quite a racket inside. But it bounced off, itself unhurt. And after a moment it said to itself, "Why, there's that damned submarine," and butted it again. Several times it socked the sub before it ran down, and each time it put in a dent. So the sub had to be pulled out to see if any serious structural damage had been done. And the story goes that the skipper, his ship out of service, got himself transferred to an active theater. Be that as it may, one officer of the Navy became convinced on one weapon.

I tell the third story to try to help dispel the idea, that gets around somehow, that senior British naval officers are stuffy. If there were any stuffy ones I did not meet them. I met a few American ones, but then I saw more American than British brass, so the proportions may be equal, and low in both cases. This incident occurred on the Clyde, at a station where training was underway on antisubmarine hunter-killer groups. The British were well ahead of us on this system.

The British felt, with reason, that they knew a lot more about hunting submarines than we did, and the admiral in charge of the station on the Clyde was in no mood to discuss the subject with any American, including me. He received me politely, but coldly. It was just as the tide was turning against the submarine, and I spoke with enthusiasm of the fine job the British were doing in the Bay of Biscay with the antisubmarine rocket. No thaw. So I spoke of the Mark X mine, having got news of the first use of it only a day or two before, said I hoped it might fit into hunter-killer operations, said that one could hardly judge the value of a new weapon from a short use, that one had to take reports of submarine kills with a grain of salt, but that I had a report that in missions out of Newfoundland, using the Mark X, there had been two kills in the past week. "What's that, will you repeat that, sir?" And I did. He called in his executive officer and said to me, "Now, sir, would you mind saying that all over again?" When I had, he said to his exec., "Why in hell haven't I been told about this?"

Late that afternoon there were a dozen of us in the room, discussing uses, whether the mine should be on the first plane to attack or the second, whether its speed could be increased, and so on. The coolness had completely vanished. The small craft on which I was living was to leave at seven-thirty that night to proceed to another station, and I had promised to be aboard. So at about seven-thirty I said to the admiral, "You know, sir, I promised the skipper of *Sister Anne* to be aboard for dinner." He replied, "If you will give me another half hour on this problem, I will see that you are aboard." So I left somewhat after eight o'clock. A car was at the door and rushed me to the wharf where a PT boat waited. As I went over the gangplank it was drawn up behind me, we went down the Clyde at about fifty knots, we overtook *Sister Anne*, and I stepped aboard for dinner.

Before I go ahead, let me say a bit about *Sister Anne*. She had been a yacht before the war. Her owner had lent her to the Royal Navy, which assigned her to Lord Louis Mountbatten. Bennett Archambault,[9] head of the London Office of O.S.R.D., went aboard her with me, along with Charles Wright, Director of Research of the Royal Navy.[10] We moved from one station to another on the Clyde and lived aboard. It was very convenient. When I joined her, I walked down the gangplank, and the skipper of the craft was waiting for me at the foot. He and his executive officer, they being the only two officers aboard, came to salute. I saluted the British ensign, then turned and returned their salute. Whereupon the skipper said, "Captain Teacher, Royal Naval Reserve." And I said, "Why, I know of only one Teacher in the British Isles. Before the war he used to make a beverage called Teacher's Highland Cream. Could you by any chance be related to him?" And he said, "I, sir, am that Teacher."

My quarters aboard had a bathroom with a pink bathtub in it. I do not know whether this was the former owner's taste in colors, but at any rate that is what I had. Archambault and Wright kidded me continuously about my pink bathtub for the rest of the war.

After my session on the Clyde, I visited the British antisubmarine installation at Londonderry, flying there with Admiral Holland.[11] On the airplane, Admiral Holland was reading C. S. Forester's book *The Ship*.[12] I asked him how it was, and he said he thought it was Forester's best. When I was down at the airport something like a week later, ready to take off for the United States, here came the admiral's aide with a copy of the book. It is one of my treasured possessions. It also gives, I think, the finest picture which exists of a naval ship in action. On another occasion I had a grand session with Admiral Holland at White's where we had dinner together. I remember going from there to Claridge's rather late at night through the blackout. At White's there was another man who was going to Claridge's, and he volunteered to guide me. I could not figure out how he knew where he was going. It was absolutely dark as far as I could see; I could not recognize a thing. But he went without any hesitancy and we arrived safely. I found out later that he was the man who introduced the Russian Ballet into the United States, the impresario Sol Hurok.[13] I didn't know that ballet people learn to navigate in the dark.

The British have been kind to me in various ways since the war. But I always remember that session on the Clyde as one that caused me to hope, heartily, that if we ever get into another jam, we will have the British as allies. We approach life in vastly different ways, but on fighting a war we think alike. It pains me to see British naval power recede. I believe it would be a pleasanter world, and a safer one, if we shared world responsibility on an equal basis. Besides, the British smoke pipes, and pipe smokers can get together far better than these deluded chaps who smoke cigarettes.

Our long delay in really getting down to the job of fighting the submarine was due partly to Navy-Air Force relations, which were none too good at times, but principally to the conservatism of the Navy, if we can call it that, which was evident in Admiral King's[14] insistence that the only way to carry on the war against the submarine was by use of the convoy system. Convoying had worked fairly well in the first world war, and King resisted any departure from it, even after the British had clearly shown the power of the hunter-killer group. Thus, when submarines began operating off our coast there were months of confusion because Navy and Air did not get together. To bring them to entire cooperation was a matter for Admiral King and General Marshall. The latter, in my opinion, was one of the most reasonable men that ever lived.

The historian of the Navy in the second world war, Rear Admiral Samuel Eliot Morison,[15] sums it up thus: "This writer cannot avoid the conclusion that the United States Navy was woefully unprepared, materially and mentally, for the U-boat blitz on the Atlantic Coast that began in January 1942. He further believes that, apart from the want of air power which was due to pre-war agreements with the Army, this unpreparedness was largely the Navy's own fault . . . In the end the Navy met the challenge, applied its energy and intelligence, came through magnificently and won; but this does not alter the fact that it had no plans ready for a reasonable protection to shipping when the submarines struck, and was unable to improvise them for several months." * Later Admiral Morison observes, "All through the latter half of 1942 and the first half of 1943, when the German submarine threat was at its peak, this struggle for control and organization of the air aspects of antisubmarine warfare was going on. In an acute situation, where a prompt solution and close teamwork were imperative, neither the Joint Chiefs of Staff nor any other authority were able to find the one or impose the other." †

It is well to examine this situation briefly, for it illustrates the danger of leaving important tactical decisions to senior officers who do not have the time—or, occasionally, the inclination—to study deeply the capabilities and limitations of new weapons and methods.

The tide turned abruptly in 1943. It could have changed much earlier. In forty-four months of war up to May 1943, the Allies sank 192 U-boats; in three months—May, June, July, 1943—they sank 100. Even more striking are the figures of the ratio of ships sunk to U-boats destroyed. During the height of the wolf-pack attacks, the ratio was an appalling 40 : 1. U-boats tore through the convoys at night, firing torpedoes right and left, and usually escaped unscathed. After the new methods were in full use, the ratio dropped to less than 1 : 1.

As the depredations of the U-boats mounted early in 1943 there was no doubt in my mind that we were headed for catastrophe. It was clear enough that, if U-boat success continued to climb, England could be starved out, the United States could mount no overseas attack on the Nazi power, Russia certainly could not resist alone. We would be forced into a situation where only the advent of an A-bomb could alter the trend toward world conquest by the dictatorships of Germany and Japan, and as far as we then knew, the development of that weapon might go either way.

Yet I was in a tough quandary. My associates were working hard with alert naval officers on a friendly basis. These in turn were urging their points of view internally in the Navy. I had close relations with a few highly competent admirals and could discuss the problem with them. I could write to King and state the problem forcibly. But should I go directly to F.D.R. and urge a change, which would have been to attack directly the competence of the Chief of Naval Opera-

* Morison, Samuel Eliot, *History of U.S. Naval Operations in World War II*, Vol. I, *The Battle of the Atlantic*. Boston, Atlantic-Little, Brown, 1947, pp. 200–201.

† *Ibid.*, p. 244.

tions? There was great doubt as to the success of such a move. But there was no doubt whatever that it would have spoiled the gradually improving relations with the services generally. Why not tackle King personally and directly? I knew that Mr. Stimson and General Marshall were pressing him vigorously on one rather limited aspect of the problem and getting nowhere. And I knew he had little concept of what I would be talking about.

Fortunately Secretary Stimson had no such inhibitions about approaching F.D.R. I knew, of course, about his contest with King on the subject of unitary air command in the antisubmarine effort, but interestingly he did not talk to me about his relations with the Navy. Mr. Stimson and I were concerned along somewhat different lines as we discussed the submarine problem. He was much stirred by the fact that Navy and Army Air Force were at odds; in contrast with the smoothly operating British Coastal Command, there was far from a unitary coordinated approach to the attack on U-boats as they raised havoc along our coasts. In fact the concept of attack itself was pushed into the background by King's antediluvian conviction that convoying was the only way to combat the submarine. There never was a clearer example of the tendency to fight a war with the weapons and tactics of the preceding war. My own unrest was a different one. There were a number of new and very powerful weapons and they just were not being used. There were plenty of examples. On one occasion a group of O.S.R.D. men had succeeded, after a struggle, in getting a new device on a destroyer, together with sailors trained to use it. When the destroyer came back from its tour of duty the device had not even been taken out of its shipping wrappings. The morale of the team working on antisubmarine devices did not collapse, but only because they knew some of us were trying to do something about it.

Ed Bowles,[16] whom I had recommended for the appointment, was attached to Mr. Stimson's office as consultant, primarily in connection with radar, and kept him posted on all the technical matters involved, for he was thoroughly in touch with the men in O.S.R.D. who were struggling with the problem. No doubt Mr. Stimson felt that it would be improper to discuss an interservice struggle with a civilian independent. And he was undoubtedly right; it would have done lots of harm if I had been regarded as an ally of the Army in a struggle with the Navy. But he knew my problem as I knew his.

The Stimson account of that struggle * and Morison's comments on it† give all that is necessary from a historical aspect. But the struggle involved only one part of the whole problem. The real problem was not just to get command relations cleared up. It was to get off the defensive and on the attack, and to put into real use the new devices which were fully capable of reversing the trends in U-boat warfare. The solution, when it came, involved the creation of the Tenth Fleet in the charge of a highly competent officer, Rear Admiral Francis S. Low.[17] This saved face for King, for the Tenth Fleet reported directly to him. And this did no harm whatever; King did not interfere with Low as the whole program was altered, probably for the simple reason that King had no grasp whatever of the technical revolution which had occurred. The Tenth Fleet was one element in the abrupt change; the real change came about because new weapons were at last used. There was instituted at once a situation in which naval officers and O.S.R.D. engineers planned together vigorously, with no artificial restraints. The new devices moved onto the antisubmarine fleet. And, before long, Doenitz [18] and his U-boats were on the defensive, and we were very definitely on the attack.

This drastic revolution came about for many reasons. The system of radio direction-finding, and Doenitz's absurd insistence that U-boats report frequently, enabled us to tell where they were located. Good airborne radar, especially the centimeter radar developed by the Radiation Laboratory, enabled a plane to search widely for a U-boat on the surface. Magnetic search means extended this finding ability against U-boats when submerged. The antisubmarine rocket and the target-seeking torpedo, which I have mentioned above, were terrible weapons against a surfaced U-boat. Forward-thrown, rapidly sinking, contact-fired depth charges were far more deadly than the old ashcans. Sonobuoys, which could be dropped to listen for submarine noises and broadcast what they heard, could enable a destroyer to pick up the submarine on its sonar and then keep in contact.

But what made the great change occur was fundamentally the fact

* Stimson, Henry L., and Bundy, McGeorge, *On Active Service in Peace and War*. New York, Harper & Brothers, 1948.

† Morison, *op. cit.*, p. 310.

that we went on the offensive. Hunting groups, equipped with modern weapons, were the most striking example of this. Battles and campaigns are not won by staying on the defensive; it is astounding how long it took us to find this out. The success of the hunting groups, consisting typically of a baby flattop and a pair of destroyers, came about because the submarine of that day could not remain submerged for much more than twenty-four hours; it had to come up to charge its batteries and get fresh air for its crew.

There was blindness in the German naval command as well as in ours, and we recovered first. The carrier could make wide sweeps, at first by day and later also by night, in an area where the presence of a U-boat was suspected. After contact by radar, its planes could keep track of the sub by sonobuoys until the destroyers arrived. Then began a hold-down. If the U-boat could not break away it was almost surely doomed. In one historic case the U-505 was actually captured. It came to the surface. The crew opened sea cocks and abandoned ship. But a boatload of nervy sailors went aboard, shut the cocks in spite of the danger of booby traps, and got the U-boat under control. Their feat involved more than just the capture of an enemy ship on the high seas, historic though that event was; it also meant the capture of the U-boat's code books. Locating enemy craft then became even more effective. Dan Gallery [19] tells the story in *Eight Bells and All's Well*, one of the best yarns of the war.

In general I think Admiral King had a terrible blind spot for new things—and about as rugged a case of stubbornness as has been cultivated by a human being. The most striking instance I witnessed was one time when he was talking to some young officers as they were looking over the designs of a new cruiser. The whole craft bristled with radar, and Admiral King said, "There's too much radar on this ship. We've got to be able to fight a ship with or without radar." This was just after we had lost three cruisers off Guadalcanal at night because the Japanese used their radar properly and we did not. It was also after Pearl Harbor, where radar had picked up the Japanese fleet and was ignored, and more radar was still in its packing cases on the dock.

There is one more phase of antisubmarine warfare which should be described, although it applied in other fields as well. This is operational research analysis. It was introduced by British mathematicians working with Coastal Command, was adopted by us, and is still in use. It was a great success. By studying results, using statistical analysis, theory of probability, and the like, this group much increased the number of U-boat kills about the British Isles, with no increase in personnel or planes.

A few words are needed to show how an increase such as this could occur. There are not many in the world who know how to use statistics or probability theory. Businessmen, politicians, advertising men should all be encouraged to study these things assiduously, in my opinion, although not many have enough grasp of mathematics to do so; these three groups all commit gross errors, again in my opinion, by relying on something called common sense, where it does not apply. Any such bald statement as this needs some support, and I will give an example which has often been used for the purpose. Suppose you are at a dinner of say thirty men and you offer to bet even money that there are at least two men in the room with the same birthday. You will promptly be taken up, for common sense says, with 365 possible birthdays and only thirty people, you have become reckless. Actually, you will win, almost every time, and the fact that the odds are heavily in your favor is readily demonstrated by probability reasoning of a simple kind. Military men are, of course, also not immune to the disease of using statistics improperly.

Let me give another example. I once reported * a study by Purcell,[20] using a computer and probability theory, which showed what one might expect in the way of batting slumps among ball players, due simply to chance, not in any way connected with any change of methods or skills on their part. It showed just about as frequent and prolonged slumps as we, who follow the game, see occurring. Will managers study this? Not a chance. They will use their common sense and bench the man who slumps.

To get back to the submarine war, the group at Coastal Command began to get real results. In this country, there was immediate response for which Phil Morse,[21] Jack Tate, and Warren Weaver[22] were primarily responsible. They went to England to observe Coastal Command's operations and results, and on the basis of that study at once went ahead with advances at home. Such matters as the probability of sighting a submarine, as it depends on the search path

* In *Science Is Not Enough*. New York, William Morrow & Co., 1967, p. 112.

taken, the position of the sun, the state of the sea, were rigorously studied, not just guessed at.

The success of Coastal Command involved not only the establishment of groups to put operations analysis to work elsewhere, but also immediately tangible accomplishments. One part of the method was to break problems into their various parts and assess each part. Such a problem was, for example, the rate at which a depth charge would sink. Morse's people established for one thing that putting a suitably shaped container around the ashcan would make it sink twice as fast. It was also established that contact fuzes were far better than depth-controlled fuzes. The resulting factor of gain in probable kill of the target submarine was substantial.

In later days the development by von Neumann [23] of the theory of games, better called theory of contests, extended the scope of such studies. No military man today would ignore this sort of reasoning if he expected to use the men and weapons under his command effectively, on land, on sea, or in the air. Of course when mathematics became thus effectively applied to warfare the various groups exchanged ideas freely. But the British, and Professor Blackett [24] in particular, deserve credit for introducing the idea and making it operate with spectacular success.

The system of exchange between scientists and engineers on the two sides of the Atlantic offered no real stumbling blocks after it once got started. Scientists in particular are so used to full and prompt publication of their results that interchange came naturally. Getting the system set up in the first place offered some difficulty, however, as is recited elsewhere. But there was a constant and voluminous flow of data and results across the ocean. O.S.R.D. had a London office, and the British a Washington office. The man who succeeded Hovde as head of the London office, as I have said, was Bennett Archambault. He was an engineer of a sort, but he had had his later training in Wall Street. I can best indicate how he operated by an example. One day I picked up a list of members of a British committee set up to resolve some differences between two weapons development groups. To my surprise I saw the name Archambault on it, and inquiry showed that it was indeed my representative in London. It was an indication of the healthy state of the relationship between allies that he had been invited to join in such an effort. But it is also an indication of the way in which men in O.S.R.D. were accustomed to go ahead, and not to take the time to tell the director. That suited me, and I am sure it suited everyone else. As long as I could keep abreast of things well enough to keep the organization on good terms with the Congress, the President, and the military services, I thought I was doing my job, even although I missed out on digging into problems that would have fascinated me.

Why do we now study submarine warfare, as we have done in this discussion of a small portion of its history? It is not that it is more important than any other phase of warfare. But it does illustrate what I have in mind in this chapter. Beginning in confusion during World War II, with many roadblocks in the path toward maintaining our sea communications, we finally worked out organization and relationships which resulted in real progress. We look at roadblocks and their avoidance because we have to look ahead.

And this brings me to another phase of my subject. There are still roadblocks in the path along which progress needs to be made. But there are blockages, too, for the existence of which we should consider ourselves fortunate. They are present in the deep convictions of the human race. Some will deny their existence or their power. But they are present, and we need to evaluate them as we consider the future. The entry of advanced techniques has completely altered the nature of warfare; it will never be the same again. But the seamy history of mankind has been one of recurrent clashes of arms, small and very frequent at first, then between groups of professional soldiers, usually mercenaries, then, as Napoleon introduced the concept, struggles involving the manpower of whole nations, then culminating in two world wars involving in one way or another all of civilization. It would be too much to hope that the whole evolution should end now. And, as we look forward, there are four possibilities.

First, the cessation of all war. As our recent history shows, it is much too early to believe that this is now possible. It will come some day, if the human race is to continue with its great experiment, and the world will turn to the equally difficult problems of the control of its population, the exhaustion of some of its resources, the alteration of its climate, the protection of air and water against pollution, and perhaps to wise use of leisure and affluence, which latter may well be as challenging as the control of war as we now face it.

Second, there may be an overwhelming atomic war, which would not necessarily eliminate the race, but which would certainly set it back to begin the long slow climb all over again.

Third, there may be no atomic war, but great clashes between large groups of nations without it. This is by no means impossible. We have had small wars in this restricted fashion and will no doubt have more. A great one without atomic weapons could indeed occur. We should remember in this connection that poison gases were used in the first world war, but not in the second, although far more powerful gases had been developed. The Germans in particular, with their new nerve gases, were in a position to devastate whole populations, and they refrained. This action may have been just from fear on their part of retaliation. But the point is that they refrained. Also, during the war, there was research on both sides on biological warfare, which indicated that even more terrible than the atomic bomb would be spreading among an enemy population a disease of a new type chosen because no means of combating it existed. Yet there is no evidence that either belligerent seriously considered its possible use. There is something rather fundamental here. There is a contention, perhaps just a myth, which Robert Oppenheimer [25] often cited, that if one puts two scorpions in a box, they will fight, but, if they get into a position where each could sting the other, they will withdraw. Two great groups have now faced each other for more than twenty years, first with stocks of atomic bombs and bombers to deliver them, then with guided missiles for more effective delivery, and now perhaps with a contest on ways of countering such missiles. There has been plenty of ill will, and there have been incidents and the clash of interests. Yet the stalemate has continued, and it is even possible, should great war come, that both would still refrain from the use of weapons of mass destruction.

Fourth, it is far more probable that the stalemate will continue among the great power groups that have atomic capability, but that secondary wars, using "conventional" weapons, will go on. That is what we have now, and what we will probably have for some time to come. The test will arrive when China achieves full atomic and missile capability. The stalemate may survive even that crisis. After all, the ruling group of no nation is likely to initiate the type of war in which it would inevitably, and personally, be wiped out.

So we can hardly disregard various means of carrying on war just because there are guided missiles in the world and atomic warheads for them. And submarine warfare is a good example for study, for it presents a situation in which work involving both military and civilian men, in this country, started off very badly indeed and, as we learned better, developed into excellent collaboration. But we certainly should keep in mind throughout that while we must prepare for war, our underlying object is to prevent it, or, if it occurs, to limit it. And where and how it can be limited is also a subject on which military-civilian collaboration is essential. Therefore, as we work toward better internal organization and relationships in this country, let us not forget that the problems have two sides.

Mutual undertaking is essential in any highly technical military effort, for the simple reason that the great majority of scientific and technical skills will inescapably be found in civilian ranks. It occurs most readily in new fields, for example in radar in the last war. It occurs with most difficulty, with more stumbling blocks, in fields where military men have long practiced, where there have been built up vested interests and complacency. Strategies to overcome these barriers then have to be devised.

An example of this latter kind of stumbling block appeared during the last war with the marine torpedo. An entrenched group in charge of this weapon not merely wished no civilian aid; it tolerated no interference by the rest of the Navy. So we shipped torpedoes to the Pacific, our submarines carried them on arduous and dangerous voyages and fired them at enemy ships, and the torpedoes would not go off. The torpedoes' speed had been increased, and the firing mechanism, having thus less time in which to function, became crushed before it operated. It was also improperly oriented—crosswise instead of end on. There had apparently been no tests to reveal the defect. Some of the mechanisms were altered at Hawaii by the Pacific Fleet itself, and there were protests that this action was highly irregular. There is another story (I cannot vouch for this one) that magnetic torpedoes had been designed to explode under a ship by reason of the alteration of the vertical component of the earth's field, and that when used in low latitudes, they ran unexploded under the enemy ship because at low latitudes there was no vertical component to speak of.

All this was bad enough, but there was something worse. The Japanese torpedoes were far better than ours; they were faster, had longer range, and packed a heavier wallop. The details are to be found in the official history by Samuel Eliot Morison, and I shall refer to them later in another connection. Here I note Morison's statement that the torpedoes with which our submarines were armed as the war began suffered "grave defects in the depth-control mechanism and the exploder, which did not come to light until the war was well along." * The Japanese, however, apparently had not yet allowed their torpedo experts to establish a monopoly free from interference or criticism. Fortunately, torpedoes were secondary in importance to air power. But it was not pleasant to realize that we were outperformed primarily because our military organization for torpedo development was isolated and hence defective.

We still do not have the full story, I fear, though Morison gives us a great deal. The loss of a cruiser at Kula Gulf and Kolombangara, and the crippling of several others, occurred apparently because, among other things, we moved in to a range which might have been reasonable had we been opposing our own torpedoes but was not so against those of the Japanese.

They were armed with their Model 93, "long lance" torpedo. It was invented by 1933 and refined in the years between then and the war. Two feet in diameter, 29½ feet long, weighing over three tons, oxygen-fueled, packing over half a ton of high explosives, it could travel nearly eleven miles at 49 knots, or twice as far at 36. These figures compare rather staggeringly with those for our standard destroyer torpedo of the time, the Mark 15: 21 inches in diameter, carrying originally 789 pounds of explosive, traveling three miles at 45 knots or 25 miles at 26.5 knots. The Japanese were also using some of the new and more powerful explosives, while we still stuck to TNT.

Morison rightly asks: ". . . why was everyone in the South Pacific, from Admiral Halsey down, ignorant of this 'long lance' and its performance? Japanese destroyers had employed it since the beginning of the war. One is said to have been picked up on Cape Esperance in

* Morison, Samuel Eliot, *History of U.S. Naval Operations in World War II,* Vol. IV, *Coral Sea, Midway and Submarine Actions.* Boston, Atlantic-Little, Brown, 1949, p. 191.

January or February 1943 and taken apart, and the data sent to Pacific Fleet Intelligence but nothing except rumor appears to have reached the Fleet . . .

"Perhaps it is inherent in American thinking to assume that our own gadgets and machinery, from plumbing to atomic bombs, must be the world's best. A dangerous way of thinking, indeed. As a Roman poet wrote at the beginning of the Christian era, 'It is right to be taught even by an enemy.' " *

Suppose the data on that Cape Esperance find had been recognized for their full worth. What could have been done so that we should be taught by our enemy? Let us place ourselves in the position of the Navy command for a moment. Could it send an officer to examine the technical competence of the torpedo wizards at Newport? For the conventional naval officer, this would have been like sending a banker into an art school to see if the students knew how to paint. Of course, there were highly competent young officers with sufficient background to do the job, but the high command probably didn't know this. How about turning to the engineers of companies producing torpedoes for the Navy? Contractors tend to keep on good terms with those who place their orders. There were plenty of excellent independent engineers in the country who could, in a few months, have become masters of the entire techniques involved, and vigorous enough to innovate. How could they be gotten together? O.S.R.D. could have done it, if asked and given full backing. It was not asked. Nor could it barge in uninvited. This roadblock, and the one on tanks, could have been broken only if the high command had recognized the need for breaking them and had had the determination to do so. Neither of these conditions was present as we fought the last great war. Fortunately, the whole technique of warfare was changing so fast that there were few such ingrown inertias and vested interests.

Let me discuss one other situation in which military thinking has become a bit hardened and in which progress was not at all what it should have been. The tank appeared in the first world war. It was capable, had it been fully exploited, of breaking the bloody and

* Morison, Samuel Eliot, *History of U.S. Naval Operations in World War II*, Vol. VI, *Breaking the Bismarks Barrier*. Boston, Atlantic-Little, Brown, 1950, p. 196.

absurd stalemate of the Western Front. But it was manufactured in driblets and used in ways that did not develop its full possibilities.

In the second world war the tank became of great importance, as central in influence on land warfare as was air cover. Its uses became understood and the employment of it often determined the outcome. Throughout the war the Nazis built tanks that were more powerful than ours, but this was partially offset by the fact that ours were more reliable. The early Nazi blitz relied on tanks and on tanks and aircraft in combination. Later, the desert battles and Patton's [26] wide end run in France put chapters of an entirely new nature into military history. The tank also became greatly developed, but mostly along conventional lines—with thicker armor, greater speed, longer endurance, more powerful guns. There was some non-conventional progress, but not nearly as much as there should have been. Stabilization of guns, so that they could fire accurately when moving, was not brought to its full potential. The possibilities of night operation were not really exploited. Night tank raids in the desert held real possibilities, where the tank commander could see and the enemy could not. Infrared for such a purpose was developed in the sniperscope and used against the Japanese, so that the sentry could see, and shoot, an infiltrating enemy in inky darkness. It was developed for the tank by O.S.R.D., but never did see service. The shaped charge, first examined during the War Between the States a hundred years ago, was built and used in antitank shells and notably in the bazooka. But it was not perfected to the point of which it was capable, at which point thickness of armor would no longer make sense. Guided missiles against tanks, such as a rocket which was guided through a wire it unrolled as it flew, came in a little toward the end of World War II, but had little influence. The bazooka, the recoilless gun, entered to have important results. Antitank rockets, carried by aircraft, really did have an important role. But it was a long time, too long a time, before they were used. O.S.R.D. developed rockets for all sorts of purposes, and especially for aircraft use against gun positions and tanks. I thought personally that any tank caught in the open by a properly armed aircraft was doomed. But Hap Arnold [27] couldn't see it. Why use a rocket when you can use a bomb? The reason of course is that you can aim a rocket, not just drop it. I well remember a test I witnessed out in a California desert of a large air-to-ground rocket, in fact much bigger than needed. There was no question that it could hit a tank with excellent accuracy, and no question in my mind that this would be the end of the tank. Incidentally, on that occasion transportation somehow got confused so that my party was left for some hours without water or shelter, and I wondered how anyone could exist inside a tank under desert conditions even in the absence of rockets. The trouble, of course, was that Arnold didn't know much about tank fighting or was just more interested in other things. So we didn't get antitank rockets until long after we should have. In fact, I understand that, at Teheran, Stalin asked F.D.R. why we didn't use airborne rockets against tanks, for he had found them useful.

Innovations are very likely to appear outside the organization which could find them useful, and there was no system for insuring that they would not be simply brushed aside. Use of infrared appeared outside the tank area; so did gun stabilization, which of course had long been used on shipboard; DUKWs and proximity fuzes outside Ordnance, rockets outside the Air Force. Toward the end of the war O.S.R.D. developed a better capability for bridging some of these gaps.

The tank story is another case of entrenched vested interests; Ordnance knew all about tanks and allowed no one to enter their sacred precincts. All through the war we lagged behind the Nazis in tank development; there is some indication that we even lagged behind the Russians. And none of the three introduced innovations from outside, such as infrared for night fighting. Some of my chaps tried to break into this fortress and were fully repulsed, not only by Ordnance itself, but also by its chief contractor. I will tell only one story in this connection, although many could be told. I went to a demonstration at Aberdeen arranged by Ordnance for a group of congressmen. Tanks were put through their paces and described. But it was a relatively ancient German tank that was demonstrated and a new American one. I made a remark which showed I had caught on, but I didn't spoil their party. After all, it made little difference to me what the congressmen thought. But when I tried to discuss tank design in a group of officers it was evident that only those who had built tanks for years were to be allowed to think about them. My gloom, however, was relieved by one incident. An American tank moved into the parking area and a girl crawled out. My wild speculations were dampened when I found she was just a member of the test crew.

The whole tank affair illustrates the danger of allowing closed circles of specialists within the military organization. The tank specialists, military and civilian, including engineers of prime contractors, knew the answers and allowed no novices to penetrate their preserves.

Only once did I witness the breaching of the protective boundaries of the tank mystic circle by an officer of the top echelons. There were some tanks of a new type, and complaints that they were full of defects in design somehow reached the ear of Mr. Stimson. He sent Major General Stephen G. Henry [28] to find out about them. I was with Henry, as we were headed for some tests on the Pacific Coast, but was merely an interested observer as far as the tank problem was concerned. When we arrived at a station in the Middle West where the new tanks were assembled, we were met by the commanding officer, who presented Henry with a detailed plan for his visit. Henry simply said, "I will inform you of my wishes." Since he was operating on direct orders from the Secretary, through General Marshall, there was no more attempt to show what the officers present wanted to show. I need recite only one more incident. It occurred the first morning. Some fifty officers gathered in a small building adjacent to the tank park where about a hundred tanks were assembled with their crews. Henry said quietly, "I am now going out into the tank park. No officer will come within a hundred yards of that park while I am there." I looked out later, and there was Henry, sitting on a tank and surrounded by a dozen sergeants, and an interesting discussion was evidently in progress. There is no doubt that the Secretary received a full report on those particular tanks. But there was no follow-up which might have brought about the entrance into the problem of a group of highly competent and independent engineers, who might—who knows?—have developed a better tank than those of the enemy.

This tank story and that of the Dukw, which I shall soon tell, and at first that of the antisubmarine program, illustrate the dangers of isolated groups with vested interests. In order for great progress to be made on methods and weapons of war there has to be a system of close joint effort of military and civilian men, especially engineers. The civilians must have independence and the opportunity to explore the bizarre; it is not enough that they be the engineers of contractors to the armed forces. Above all, there must be mutual respect and reliance. This must be present, whenever we have to fight again. My point, at the moment, is just that such a system is hardest to develop in the areas where the military men are themselves most competent. One can hardly expect military groups, who have handled all aspects of semiconventional weapons for years, to receive engineers from industry or scientists from the universities with open arms, and to give them freedom to innovate. But it is often in these hard-core areas that new thinking is most needed. It seems to me that it is squarely up to the Chiefs of Staff to see that it occurs.

Lest it be thought that I am here criticizing military officers in general, let me recite a story concerning one such officer who has certainly been a controversial figure. Field Marshal Montgomery was a great tank fighter. Did he demand, and get, radically altered forms of tanks, or ways of destroying them? He did not. Was this because he had a closed mind on the subject? I believe it was just that he did not have time to get fully acquainted with the possibilities and because he was not offered real innovations, not because of any reactionary attitude.

Soon after the war Al Gruenther [29] called me over to SHAPE * for some discussions. The subject, this time, concerned the tactics for a field army in the presence of an enemy having tactical atomic bombs. Some training exercises were planned in this connection, and Montgomery was in charge. I was his guest at the Château de Courance. One evening we had dinner at the château, six or eight of us at a small table in the middle of the great empty hall. I was the only American. Several of the others were British generals who had fought in Africa. As the dinner ended, Montgomery turned to me and said, "Doctor, give us a text which we can discuss." I thought for a while and then said, "I will give you a text, and a rough one: The heavy and medium tanks, as used in the war, are now obsolete." Montgomery banged the table and said, "Excellent. We will discuss that." We

* Supreme Headquarters Allied Powers, Europe.

did, with no holds barred. There was no reactionary point of view at that table.

One more incident occurred at a lunch. I forget the exact topic. But one officer—thank God he was an American—met one of my statements with a rebuttal I had heard before to the effect that no damned civilian could possibly understand a military problem. So I waded into him, with my usual aversion to remaining on the defensive, and said that, unfortunately, there were still some officers in existence who were so dense that they did not realize that the art of war was being revolutionized all about them. I was a bit rough, and after lunch I started to apologize to the field marshal for engaging in altercation at his table. General Montgomery said in effect, "On the contrary, we owe you a vote of thanks. That chap has been an obstacle to good discussion for some time." As a matter of fact, his remark was a bit more pithy than I have related.

Very few reactionary officers indeed reach high command in the field; if they are conventional in approach, close-minded, they simply do not fit as war is now fought, and that fact is soon found out if their supreme commander is truly able. But, in the field, they just have to depend on the officers at the home base to bring forward innovations, for they are altogether too busy, too harassed, to think about much beyond the tough daily problems. And the officers at home are likely to be far less vigorous and far less willing to take chances or to back novel ideas. Fortunately, men sometimes come back from the field and leaven the mix. I think at once of Admiral Lee, Admiral Purnell, General McAuliffe, General Devers,[30] General Waldron,[31] and many others. But the places where reactionary thinking becomes concentrated are the bureaus or divisions, manned by officers of technical or engineering background, with no hope of field command.

And even here there is a sound reason for conservatism which I hasten to express. The bureaus, the divisions—the technical or engineering centers—have their own tough problems, to get the weapons manufactured in ample quantity and shipped to the right place at the right time; to anticipate needs for food, clothing, medical supplies; to control shipping; to train men and conduct affairs with Congress, labor, and the public. And, among them, even in the midst of almost unbearable burdens, are some men with keen eyes on the evolution of weapons—some, not many. Of course, I write of matters as I witnessed them. It may be much better today, and I hope it is.

The story of the Dukw brings out what I am here trying to express. The full story is a large one; I will just touch on it at the points where it illustrates what I have in mind.

The Dukw was an amphibious army truck. The idea is surely an obvious one. Take a standard truck, make its body watertight, put a propeller on it, and it can swim. It will then be able to cross rivers and lug men and material ashore across beaches. Obvious indeed. But the Dukw, when built and tested, had a surprising capability, not fully anticipated. It could go through surf, vicious surf, which no small boat could survive. It could do so because, just as it lost its propeller propulsion, it gained wheel traction on the beach and came straight in without the pause that would throw a landing craft over in the surf. In fact, while we thought early that it might be able to negotiate surf five or six feet high, it actually on test went through surf measured at fifteen feet. So it was a vital factor in landings in the Pacific, in Africa, in Sicily, on the Normandy beaches. General Bradley says, "Had it not been for the ninety preloaded DUKWs that waded ashore on D day, we might have been hard put for ammunition." * There were plenty of similar comments.

But Army trucks are developed and built by Army Ordnance. They knew all about the subject, and they did not want any bright boys playing in their back yard. When N.D.R.C. proposed the project of the Dukw, the Army representative on N.D.R.C. voted against it. General Somervell,[32] head of the Service of Supply, said to me forcibly that the Army did not want it and would not use it if they got it. But the Dukw was built and used in large numbers.

The power of the Dukw in surf is well illustrated by an incident on the back shore of Cape Cod, early in the game. A Liberty ship was to be unloaded by the Dukw to show what it could do, or perhaps what it could not do. A winter northeaster came up. A Coast Guard craft with seven men aboard went ashore on the Peaked Hill bars, in the dead of night. No help could reach them, either from the sea or from the shore. The Dukw was in a garage in Provincetown. Palmer C. Putnam,[33] who had developed the Dukw and was

* Bradley, Omar N., *A Soldier's Story*. New York, H. Holt & Co., 1951, p. 278.

in charge of the group on the project for N.D.R.C., got his men together and the Dukw out to the back beach, where the wind howled and the sand cut faces. The Dukw went through the surf, out to sea, picked the seven men and their dog off the boat, and brought them ashore. In the morning their boat was gone.

Now Putnam was a go-getter, in fact so much so that I had occasional demands from the more staid individuals in O.S.R.D. that he be tamed or suppressed, demands which of course never budged me. So he remembered on that wild night to alert his photographer and thus to get pictures of the rescue. I took these to Mr. Stimson and told him the story. Mr. Stimson took them to a Cabinet meeting, waited until Mr. Knox [34] was close by, and then told the President that this was probably the first case in history in which a naval vessel had been rescued by an Army truck. This eased off some of the opposition.

Putnam had some of the characteristics of the best type of promoter in industry. He was well liked by men with lots of drive, and often disliked by those with less. General Devers, for example, tested Putnam's Dukw at a training camp, probably without authorization to do so. Mountbatten took Putnam thoroughly into confidence on landing problems in his area. So did commanders in the Pacific. At times he nearly got me into trouble, but it was well worth the risk. His full story would be an epic; I hope he will write it.

One very cold day he demonstrated the Dukw to a group of officers on a beach in Virginia. Ten officers and I stood in the truck as it rolled down the beach; a vicious surf was pounding in. "Gentlemen," said Put, "I am sorry we have no surf this morning. It was excellent yesterday but has subsided so that I can give you only a weak demonstration. Driver, take her out to sea." So we went to sea. The water flew and the craft plunged. After some time with this sort of thing a rather wet group of officers got ashore and headed for a drink. Put smiled and said he had hoped for better surf.

At first there were all sorts of obstacles to the use of the Dukw. For example, we had a few in North Africa while getting ready for the invasion of Sicily, and we needed to send over some O.S.R.D. personnel to train the drivers. This was blocked by a general who knew all the answers. This particular roadblock, however, was readily avoided; the British had a few Dukws, so we sent the trainers to them, and they loaned them to the Americans. Opposition disappeared when performance became spectacular. Dukws were able to carry 105 mm. artillery right across the beaches to their firing positions. Equipped with 144 five-inch rockets, Dukws could plaster the beaches before troops landed. This led to the rocket ships, with far more rockets, which saved casualties on many a landing. At Normandy the Dukw carried eighteen million tons ashore, across stormy beaches, in the ninety days during which the enemy denied us the use of ports.

The Dukw was one of a few war developments that found a real peacetime use. After the war the Coast Guard adopted it for beach rescue work, where it is far better than a lifeboat. Today we have bigger and presumably better Dukws.

The same group of vigorous, sometimes wild individuals in Put's organization developed amphibious jeeps, snow buggies, and other strange vehicles. What a slow crowd we sometimes are in this country! It is only now as I write that snow vehicles are being widely used for sport. And sport fishermen still do not really use amphibious vehicles to get where the fish are.

After discussing subjects which contained a bit of ingrained resistance, it is a relief to turn to one where there was none whatever: the proximity fuze.

Now why the difference—blockage on the Dukw, close collaboration on the fuze? Army and Navy both had sections that knew all about fuzes, knew well what could not be done. It was the Navy, not the Army, that collaborated; sea warfare had evolved into new forms to a far greater extent than land warfare during the first half of the century. The real point, though, was something else. To make a Dukw one had to work closely with Army units. One could make a new fuze without bothering anyone. Once the fundamental problems had been solved in the laboratories and the equally baffling problems of bringing the device into full production had been worked out by laboratories and industry together, shells with the new fuze could just be loaded into the guns as they stood, and fired, and the results were immediately spectacular.

What is a proximity fuze? It is a fuze that will fire the shell when it is in proximity to the target, ideally in just the right position to secure maximum effect. Some scheme to let the fuze know when the shell carrying it is at the right point is a central factor. Many were investigated early in the war—photoelectric schemes, and acoustic, and electromagnetic, and radio. The fuze we are concerned with is the radio proximity device, the most effective—and the most difficult to achieve. On this the British were at work as early as May 1940.* Discussions with the Navy were going on in this country before the N.D.R.C. was established by executive order June 27, 1940, and they led to the placement of proximity fuze research among the earliest projects undertaken by the new agency when it came into being. In a war where air power was of such great import, the idea of the proximity fuze was especially vital to antiaircraft fire. With an ordinary time fuze there is little chance indeed of so setting it that the shell will burst at the right point, and the chance of actually hitting the plane with an unexploded shell is small indeed. It is quite different if the fuze fires the shell at just the right point before it arrives at the position of the airplane. A deadly cone of shrapnel fans out and, even if the aim was a bit wrong, smashes into the fragile structure.

A similar point shows up on land use. High-angle fire against enemy troops in the open, or even in trenches, can be very deadly. The old way used to be to post an observer where he could see the bursts. He would then phone to correct the timed fuze settings to get the bursts at the right height above ground to spray the area with shrapnel. This took precise setting of fuzes, and timing mechanisms were erratic; moreover, it could not be done at all in fog, or where the enemy was over a hill. With the proximity fuze the whole affair was automatic. If a body of enemy troops could be caught in the open, the effect was devastating.

It took a group with courage and ingenuity to tackle anything of the sort. The basic idea was simple enough; anyone who had heard a radio squeal as one passed his hand near it would think of it at once if he puzzled about fuze problems in shells. Simple though the basic idea was, making it work would be something quite different. Consider what it meant to try to build a proximity fuze. One was to build

* Work on the fuze in the United States is reported in Baxter, James Phinney III, *Scientists Against Time* (Cambridge, The M.I.T. Press, 1968), pp. 221ff.; and Boyce, Joseph C., ed., *New Weapons for Air Warfare* (Boston, Atlantic-Little, Brown, 1947), pp. 102ff.; and the history of the original British work is told in Brennan, James W., "The Proximity Fuze: Whose Brainchild?" *United States Naval Institute Proceedings*, Vol. 94, No. 9, September, 1958, pp. 73ff.

a radio set about the size of a baking-powder can, with its thermionic tubes, condensers, wiring, batteries, all finely adjusted. Then one would fire this from a cannon, whereupon it would press down on its support with a force of nearly a ton. Then, after this rough experience, one would hope it would still be in operation and in fine adjustment, glass tubes, delicate filaments, and all.

To bring this hope to reality would be a long and difficult task. The N.D.R.C. had hardly got under way when I had a visit at the office from Merle Tuve,[35] Larry Hafstad,[36] and Dick Roberts [37]—men I knew well, among the ablest young scientists in the country. They had a program to propose, a plan for bringing the idea to working form. They by no means ignored the enormous technical difficulties involved; but they had ideas on how to approach them. We talked over the various problems and agreed that though the chances of success looked small, they were not negligible in the light of the importance of a successful outcome. Informal financial support was worked out, to become formalized as the operations of the new agency settled down. Tuve rallied forces and went to work, being briefed on the secret details of the British studies by Professor John Cockcroft,[38] who had come to this country as a member of the British scientific mission headed by Sir Henry Tizard. The upshot was that the start made in the United Kingdom, where the theory had been carried all the way, went on to practical accomplishment in the United States—a notable example of cooperation in crucial times.

Very soon models were made by hand and tried in gentle ways. But then the really tough part came: The models had to be fired in guns, and recovered to see how and why they failed. This was not easy; when a shell is fired from a gun one does not usually get a chance to examine it afterward. Oh, one can examine a bullet fired from a pistol into a box of cotton, but not a five-inch shell. But finally came the day of partial success. A target was hung between tall poles. Shells were fired at it; once in a while one would go off at just the right point.

Next came the most rugged part of the job. It is one thing to make, by hand, a device which, with tender care, can be made to work. It is a far different matter to make thousands, millions, by production methods, all alike, all safe to use, with a tolerable percentage of duds. This is not science; it is engineering of an advanced nature. The men engaged in the effort were a group of young scientists; they became engineers.

The support of the Navy was here magnificent. As the effort moved toward production, they placed the contracts as the little group advanced. Plants all over the country made parts; most of them did not know what for. Others assembled, under the tightest sort of security measures. The Russians found out a great deal about the atomic bomb during the war, all, in fact, they needed to know. I do not think they even knew this fuze development was going on. Certainly the Germans were in complete ignorance, as will become clear. Young Navy officers became part of the team, and it was about the finest team ever assembled. When full production started, as I have mentioned, and I told the Navy it was time for them to take over, they urged me to keep the project in N.D.R.C., with the original group controlling the technical aspects with the dozens of Navy contractors, an arrangement I had previously carefully avoided. This was done. Liaison was excellent; there was never a disagreement, not even a misunderstanding, during the whole program.

The first great application of the fuze was in antiaircraft fire in the Pacific. There the fuzes increased the effectiveness of the five-inch antiaircraft batteries by a factor—variously estimated, but probably about seven. This was the same as having seven times as many guns on a ship. Especially during kamikaze attacks, the fuze forestalled many a catastrophe.

Next it put the final period on the V-1 attacks on London. Batteries of ninety-millimeter radar-controlled guns with proximity fuzes, on the coast of Britain, ended this threat. Hitler's slow flying bombs, which flew in a nice straight line, were sitting ducks for such a battery, whether or not there were clouds. I watched one test somewhere on the Carolina coast. A robot target plane, barely visible, flew across out to sea. The battery began to bark. After quite an interval the first shell arrived and the robot disintegrated and started to fall toward the water, and after a bit the battery ceased to fire. But the last shells to arrive followed the robot all the way to the water. These radar-controlled batteries were uncanny. They not only picked up and followed the target, they computed where it was going to be when the shells arrived and adjusted accordingly. No gunner needed to touch anything during the process. Hitler's flying bombs had no chance whatever against this combination.

But the very success made trouble. The Joint Chiefs of Staff ruled that the proximity fuzes were to be used only over water. Both the Navy and the Air Force feared that a dud would get into enemy hands, would be copied, and would be used against us. This was serious business. Many of us knew what could be done by an enemy armed with proximity fuzes in land war in France, what it could mean in American casualties.

We set up a committee consisting of some of the most respected engineers in the country, men thoroughly competent to judge the question we asked. And we asked just one question. If the enemy were presented with an intact fuze of our best model, how long would it take either the Germans or the Japanese to reproduce it and put it into production? The answer came back, "A minimum of two years." Still the Joint Chiefs would not budge.

There is a point here which is worth dwelling on. I have written that a military organization must be tightly formed and controlled in order to fight well. But this carries a great disadvantage when it comes to a question such as this one. Only officers of relatively junior grade have the technical background, the time, the interest, fully to understand a radically new departure in weapons and methods. The top brass does not. It does not even have time to listen and learn. Yet the top brass makes the decisions, and junior officers cannot protest. Fortunately, there are senior officers who appear once in a while who know how to break through this impasse.

In this case, General McNarney [39] told me he could take care of Air Force objections, if I could take care of the Navy. So I saw Admiral King. As I have suggested earlier, he was a tough customer. It was well known that he scared his junior officers so thoroughly that often he didn't get adequate information through them. Characteristically, our discussion opened as follows: King scowled and said, "I have agreed to meet with you, but this is a military question, and it must be decided on a military basis, to which you can hardly contribute." So I told him. "It is a combined military and technical question, and on the latter you are a babe in arms and not entitled to an opinion." It was a good start, and the discussion went on from there —and went well. When I finally convinced him, we went together to a Joint Chiefs' meeting. There he asked the questions to bring out my arguments. The Chiefs voted to release the fuze for use over land. Twenty-four hours later I was bound for France.

I went over at once because I thought there might easily be road-blocks, or at least ignorance, there as well. There were, but not many.

With me went General Waldron, Chief of Requirements of the Army, and Colonel Cox, secretary of the Joint New Weapons Committee. We visited, at Versailles, General Bedell Smith,[40] Chief of Staff to Eisenhower. Just as we went in I said to Waldron, "You take the lead and I will follow up on technical matters." "Not by a damned sight," said he. "On this expedition I am just your aide." It was fortunate we arranged it that way.

When we went in, Smith, whom I had known well in Washington, said, "What the devil are you doing over here? Don't we have enough civilians in the theater without your joining?" So I told him I had come over to a dense bed of ignorance to try to prevent the destruction of one of the best weapons of the war.

We got along swimmingly; there was no ignorance or lack of mental alertness on Smith's part. But Waldron could not have talked quite so directly as I did. Smith soon agreed the fuze was hot stuff and asked me what I wanted done. I proceeded to reel off a well-rehearsed list of orders that should be issued. We had some four million proximity-fuzed shells in France or on the way there, so the problem was to get them to the right places and in the hands of men who knew how to use them. Because of the tight security, ordnance officers with the divisions did not even know these fuzes existed. When I finished Smith said, "O.K., I will do all that. Now will you get the hell out of here and let me get to work."

The following week I visited all along the front, telling the story to chief ordnance officers of divisions, Cox backing me up where my knowledge of artillery practice was lame. There is a point here. My rank in the hierarchy of war was never defined, but it certainly was not minor. Yet I made contact with ordnance officers only, not with division commanders, certainly not with corps or army commanders. This was business, not a social call. Senior officers had far too much on their minds to be intruded upon; they would get the story through channels. Yet when I was in the south General Devers bounced into the room for thirty seconds, told me, "I would join you but I have a fight on," and left. And some years later General Bradley reproached me for not giving him the story firsthand.

The fuze went into use just at the time of the Battle of the Bulge. It caught German divisions in the open, feeling secure against timed fire, for they had chosen a period of bad weather when planes could not fly. The proximity-fuzed shells decimated them. I leave it to military analysts to judge the fuze's value. Patton said that it saved Liége, and that with its advent the tactics of land warfare required full revision.

Yet Bradley, in *A Soldier's Story*, devotes only a dozen words to the fuze; Eisenhower in *Crusade in Europe*, one sentence; Arnold in *Global Mission*, nothing. Now I certainly do not recite this in criticism. I worked closely at times with all three men and admired them greatly. I mention it for a far different reason. I have often been asked whether, during the war, I sat in on sessions of top-level strategic planning. The answer is no, except on use of the atomic bomb. The next question is, "Why not?" The answer is that under the conditions as they were at that time I could not have contributed much if I had. Technical matters such as the evolution of weapons were left to lower echelons. It had always been so, much more so in the Army than in the Navy. But why didn't I force my way in? After all, I had the President's nod. It would have done more harm than good; good relations were far more important than sitting in at top-level sessions. It would have been impossible at that time to change the aged system, to divert attention away from the path that was fully accepted. That is why I did not intrude in France. I hope I was right. But I record this here for a very different reason.

The system by which the advent of new weapons was left to lower echelons in the last great war was inevitable. It is not nearly good enough today. What is needed is more than just some system by which senior commanding officers can be kept informed of developments. In staff conferences where current strategy and tactics are being discussed there should be present an individual who is master of recent technical developments that are just entering into use or are not far in the future. This principle holds at every level, even at the top where grand strategy is formulated. It did not occur in World War II, as far as I was concerned, except in connection with the atomic bomb. It did occur in the case of Palmer Putnam, thanks to his drive and his genuine contributions to the war in the Pacific, but he should tell his own story. It did occur to some extent in the case of Ed Bowles, who joined Mr. Stimson's staff of aides to advise on radar matters. There is an interesting point here. Such men could enter into planning while I could not. If I had attempted to do so, the effect on O.S.R.D.-Service relations might well have been disastrous. Today the situation could be far different. I hope it is.

This is serious business, so let me turn to a lighter incident, one which, however, relates to military-civilian relations as they sometimes occurred. A group of us were waiting at a British airport, bound for Paris. France was closed in, England was going to be. We did not know whether we could get away or not. A general in the group, let me call him General Blank, remarked so he could be heard all over the room, "Who is the damned civilian?" Now I was in uniform, officer's uniform without insignia, which all men from O.S.R.D. wore in the field. It was an excellent system—no rank; the youngest member wore the same as I did; he was not barred by rank from useful contacts. But the uniform labeled me as a civilian just the same. Now I did not mind his question. In fact, the more forcibly I was called a damned civilian, the more I was assured that my tribe of innovators was making a dent here and there. But the point is that both Waldron and Cox minded very much indeed. Why this is significant leads me to write a bit about each of them.

Waldron was a fighting general. In bringing artillery over the mountains to subdue Buna he had taken a piece of shrapnel in his shoulder. After some months in the hospital he had somehow gotten himself restored to active duty, with special orders that enabled him to salute with his left hand, much to the surprise of an occasional sergeant. In Paris I had to help him get into his shirt. But he knew what was happening to hoary methods and weapons for making war. And he resented, deeply, anything that seemed to build a gulf between those he regarded as partners in an enterprise.

Cox came out of the National Guard, as I have recounted earlier, out of a unique part of it. Sometime Colonel of the First Regiment of Virginia, he wanted badly to get into the fight and was sidetracked to the Joint New Weapons Committee, but he never complained. In fact, I believe he got quite a bit of quiet fun out of its struggles. When he went with me to France he promptly fixed matters so that he would stay there. This he arranged by taking on the job of indoctrinating ordnance officers in Italy. Of course I did not stand in his way. But I did not see him again until the war ended.

But to return to General Blank. When I was about to leave Bedell Smith's office after the discussion of the proximity-fuzed shell which I have outlined above, he called me back and wanted me to tell him whether there was real danger the Germans would introduce an atomic bomb before the war ended in Europe. I told him I would prefer to answer that question after a week, but we talked for quite a while. His aide came in once or twice to tell him he had an officer waiting. When I finally left, Smith told me the visitor was General Blank, who was to be transferred to Italy. So when I got to the anteroom I greeted Blank pleasantly and told him I trusted he would enjoy his new assignment, thus leaving him wondering still more intensely who in hell the damned civilian could be. I recited this to Waldron and Cox, and it seemed to cheer them up.

The reason I asked Bedell Smith for a week's pause before I answered his question about a possible atomic bomb was that his inquiry came at an important juncture in our search for evidence on that subject. I spent that evening with Sam Goudsmit [41] and some of his crew, who were soon to go to Strasbourg, which was just then being captured. Goudsmit was scientific director of the Alsos Mission, set up by General Groves, O.S.R.D., and the Army for the explicit purpose of finding out about the German program. It was very ably led by Colonel Boris T. Pash,[42] and well supported by Colonel C. P. Nicholas [43] of the War Department Special Intelligence, and by the Manhattan District. The story of its operation is well told by Goudsmit in *Alsos*, a book of real interest to all those who are annoyed by spy thrillers and would prefer the performance of a sane, well-managed intelligence operation, which got results. Incidentally, Alsos was, in the words of James Phinney Baxter III,[44] historian of O.S.R.D., "One of the finest examples of the cooperation of the scientists and the armed services." Irving * adds good detail to the story, as does Pash.

Early in the war we believed the Germans were well ahead of us

* Irving, David John Cawdell, *The German Atomic Bomb: The History of Nuclear Research in Nazi Germany*. New York, Simon and Schuster, 1968; Pash, Boris T., *The Alsos Mission*. New York, Award House, 1969.

in the race toward the atomic bomb, and we were appalled, for winning that race would have delivered the world over to Hitler, at least for a while. By the time of my talk with General Smith in November 1944, we had all concluded that we probably held the lead, but we were not dead sure of it. While I was talking to ordnance officers about the fuze, Goudsmit and his crew were headed for Strasbourg, to interrogate German scientists, to pore through files. They found out plenty. When they were through, there was no doubt left; we were ahead of the Germans. In fact, we were so far ahead that their effort, by comparison, was pitiful. So I could meet Smith again and this time with assurance. He outlined for me the timetable of the campaign, including incidentally the expectation of a massive German counterattack. He said the plan could be speeded up, but at the cost of increased casualties. Then he asked me whether, in the light of a German atomic bomb threat, haste was essential. I could tell him flatly that he could take a couple more years, if necessary, and there would be no German atomic bomb.

Now why was German science and technology so deeply ineffective in this program? It was not because the Germans did not try, much as some of them later tried to assert. There had been nothing really wrong with German science and technology; when I was just starting in as an engineer, they had led the world. There were two reasons for their failure. First, Hitler's insanity had eliminated their Jewish scientists. The Jews have produced far more than their share of brilliant scientists, in Germany and here, have in fact produced a host of leaders in every profession. So German science was insanely crippled, but it was still formidable. The second reason is the subject of this chapter. The Nazis had no genuine collaboration between scientists and military men. They had a caricature of an organization. They took the old Prussian caste system, with arrogant generals strutting in their bemedaled uniforms, and they made a caricature of that also. Sound scientists, when used at all, were bossed by second-rate ones who contrived to become members of the SS or the Gestapo. We have to add, also, that many German scientists, by no means all, were carried away by the prevalent conceit, the Aryan myth, and were in no condition to collaborate with anyone whatever.

Goudsmit presents all this, and concludes that a truly democratic country has enormous advantage, in the modern world of science and technology, in a contest with a totalitarian state. I came to the same conclusion, just after the war ended, and said so.*

In coming to this conclusion I did not overlook the fact that Germany produced the V-1 and V-2 weapons, and that these might have had serious consequences if they had not come too late and had not been, as far as the V-1 was concerned, countered. In fact, as I later recount, I expressed the opinion to Eisenhower that, if the Germans maintained their production and delivery programs, and directed the missiles onto Plymouth and Bristol, they might well have stopped the invasion of Normandy. I base my conclusion on a matter of balance. Germany came nowhere near to making an A-bomb. It produced no proximity fuze, no advanced radar, no effective ground-to-air rockets, no Dukw, no loran, or a dozen other items which would have been of real use to them.

Do we need to modify this point of view today? We now confront Russia. It is a totalitarian state, with a democratic form which is a pose only, ruled by a self-perpetuating clique. Yet it produced a Sputnik before we did. We do need to modify.

Russia has a caste system, but that system does not put the military man on a pedestal. That spot is occupied by the party member and the practitioner of a new form of palace politics. Their scientists are not pushed aside; on the contrary. Moreover, they learned much from us, and their brilliant successes in limited areas have come about because they have extended a notable measure of freedom to scientists and technologists where results of possible military value seem attainable, and where the freedom would not threaten the police state control. Their internal organization is by no means crude where such objects are in view. Their effort has been devoted to rendering Russia strong in a military sense, in a world where modern methods of warfare control. We live in a far different era from that in which Hitler launched his program of military conquest, in which Japan attacked Pearl Harbor.

There will be no direct, all-out military struggle between Russia and the United States, unless it is brought on by accident or the worst sort of ignorance, for it would be joint suicide, and both we and Russia know it. I have something to say about the ignorance elsewhere in this volume.

* *Modern Arms and Free Men.* New York, Simon & Schuster, 1949.

The Russian plan of conquering the world by subversion is also far from a sure formula. The Communist state expanded enormously at the end of the war. This was inevitable. The scheme of Stalin, to crouch as Europe exhausted itself and then pounce, was wrecked when Hitler's insanity caused him to attack Russia. But still the exhaustion came, and but for the Marshall Plan would have been complete, and so might still have enabled Stalin to take over by military force all of Western Europe. China became Communist also. A possible great empire, in a wholly modern form, was on its way to world domination.

But today the scene changes. Russia bit off more than she could chew. The Balkans cannot be held forever under a tight yoke even if military might, as in Hungary and Czechoslovakia, can maintain Russian despotism for a while. China will not suffer domination by Moscow. The potential great Communist empire creaks at the joints and will ultimately break apart.

Russia had a clever idea of how to entice others to do its fighting. Minorities, hungry groups, would be taught the Communist myths, would rise and take over. Then Moscow, with its police state control of the system, would just move these new areas into its monolithic empire. The idea worked well in Eastern Europe, in the Baltic States; it was a formula for a new form of world conquest. But it did not work out in Greece or Korea, or Indonesia, or Malaysia. We will come to Vietnam presently. I write from the standpoint of science and its applications, not from a far broader approach. But I write also from a study of how technology and organization fit together in the modern scene, and there is much to be considered even from this limited standpoint. The thoughts of would-be conquerors have long revolved about just such considerations.

Genghis Khan conquered most of his known world, with his wild hordes of skilled horsemen. Moreover, he held a vast empire together as long as he lived. The seal of the Khan produced instant obedience in a distant satrapy, so distant that the emissary of the Khan spent weary months on his journey. No one has told us how the Khan could do it. But a large part of it came about because of a technical innovation: the invention of cavalry.

At the height of the Roman Empire a legion, a thousand men, armed only with sword and shield, tramping down a Roman road in the midst of crowds of barbarians, nevertheless dominated by reason of the fear of Rome, by reason of fear of its organization, its system of communication, and its transportation by sea and land, technical matters all.

Now we have instant radio communication, rapid transportation by air, tanks and machine guns, bombers, helicopters, rockets: Certainly no subjected people could possibly rise again, against a conqueror thus armed; certainly the time has come for complete world conquest. That was the dream of Hitler, of Stalin, of others too. It just is not so.

As I write there is a war in Vietnam. It started as a fight by proxy, with Russia and China backing one side and the United States, with some, but not much, support from the rest of the free world, backing the other. It is still a proxy fight for Russia, but not for us. Both sides have pulled their punches to avoid direct confrontation. This is clear enough in our restraint in bombing the north. It is not so clear, but should be, from the other viewpoint, for there is a great deal Russia could do and has not done.

There are lessons to be learned from it, even some in technical areas. There we are with complete air dominance, with undisputed sea control, with lavish supplies of all sorts, faced by a third-rate power, supplied over great distance. It should be over at once. It is not. The power of modern weapons has its limits.

But there are far more important lessons for us than this.

At the time of the second world war, the collaboration between scientists and military men was straightforward, and it finally worked well. As the war started the development of weapons had lagged far behind what was possible. Scientists, applied scientists, engineers, were all ready to go, and a hundred opportunities faced them for the attainment of very practical results. There was no doubt of their capability after the first successes. In spite of a roadblock here and there, intensive collaboration was almost inevitable.

In World War II military planning and military strategy were determined by military men alone. Where military and political considerations were joined, the strategy was determined on a broader basis to include political considerations. I do not write here about the border between the two, or the incidents when that border has been crossed. But no scientist, no engineer, sat in on the planning at the top, until the atomic bomb arrived. In the field, to some extent, yes; at the top, no.

I speak here of the top. There were of course many instances where men from O.S.R.D. entered into discussion on planning, sometimes quite informally, with military men at lower levels. Ed Bowles, working, as I have said, as consultant to Mr. Stimson on radar, was certainly not accepted as a planner, but he certainly saw to it that the technical progress on radar became known in many spots where it did a lot of good. Karl Compton and Ed Moreland [45] went to the Pacific and spread the gospel there. A number of men, Hartley Rowe for one, were accepted as advisers on antisubmarine affairs when the Tenth Fleet was established, and soon found that Admiral Low, who headed that strange innovation, supplied plenty of steam of his own. Perhaps the most notable example was Palmer Putnam. As originator of the Dukw he went to the Pacific to help with its use in amphibious landings. He hit it off to an extraordinary extent with Mountbatten of Burma and as a result was drawn into planning in several theaters. For one thing, he lectured in London to groups of senior British officers who were planning the Overlord landings on the Normandy coast. Incidentally, this shows that while Mountbatten may have been a bit gullible in the case of one Geoffrey Pyke, as the next chapter will recount, he also recognized sound engineering talent and tactical common sense when he met it. But all these incidents were at secondary levels. At the level of the Joint Chiefs of Staff, no man who saw the broad picture of the imminent revolution in techniques of warfare sat in as plans were made. I was handily available, for I headed one of their supporting committees, but they did not invite me in on planning, and I did not butt in. I'm quite sure this was not due to timidity on my part. I just hope I was right in my feeling that aggressiveness would have done more harm than good, as matters then stood.

This situation as a whole was undoubtedly as it should have been. Military men view with great seriousness their task in the planning of military strategy; they have devoted a lifetime to preparing to do it well. An invasion into this area by men of political background is resisted with vigor. The lessons of history are strongly in the direction that, while Clemenceau's remark that war is too serious a matter to leave to the generals has validity, one should go on and state that warfare has two aspects, political and military, and that mixing the two indiscriminately is perilous. The mission of the military should be determined and stated by the full system of the political state; the top military leaders should be thus selected, and then they should be given full support to plan and carry out their mission. This is sound doctrine. To have altered it by injecting scientific thinking at the very top level, by introducing into the planning counsels some man of primarily scientific or engineering background, would have done more harm than good. It would have spoiled a fine and developing partnership which was essential for the progress that was made.

We can learn much from Vietnam. One thing we can learn is that our military planning has not been good enough—and this is quite apart from the question of whether our political planning also has been faulty. In particular, it appears we did not plan well in regard to the power and limitations of modern weapons, their evolution, their applicability under novel conditions of warfare.

Is the answer to place a civilian scientist at the top table, where the military plans are made and the campaigns controlled? I do not think so.

I do think there should be in that position a military officer who has made a lifetime study of the evolution of weaponry, who has made himself a master of this subject. He should of course be also a master of the military art in all its traditional phases. Such men exist. Many more would exist today if we had been fully determined to develop and encourage them. There is an old doctrine among the military that he who is fully skilled in weaponry is not qualified for top command. It is a superstition only and has no validity in the modern world. At the planning table there should be at least one man who understands weapons, those of the present and of the probable future, in all their aspects, as completely as the conductor of a symphony orchestra understands music, in its traditions and its current trends. That man should be a military officer. But he should be there, and it is up to the military men of this country to see that he is.

IV

Of Tyros

THIS chapter has to do with tyros, amateurs, and professionals, and it is necessary to define the terms as I use them here.

The Romans called a new recruit a tyro. I extend the definition, for there is no word in English which covers what I have in mind. The tyro is the freewheeler in an organization, who gums up the works because of his arrogant ignorance, often because he filches authority which does not belong to him. He operates because his boss doesn't know what he is doing, or knows and doesn't care. He can throw any organization, civilian or military, into confusion. His breed should be exterminated for the good of society. I prescribe no special brand of Flit for that purpose.

The tyro has a contempt for channels of authority and ducks around them. Military men, during the great war, especially distrusted and disliked his pattern of operation, and they still do. Everyone who has ever worked in a complex pyramidal organization recognizes that there occasionally appears somewhere on the ladder of authority a dumb cluck who has to be circumvented if there is to be any progress whatever. But the process of shearing such a one of his formal control is then usually a group process, and this is not what I mean. The tyro I have in mind is not formally in the chain of command at all; he just butts into it at all sorts of levels and clashes the train of gears. I will discuss one such tyro later.

The amateur is a far different breed of cat. He is operating in a field in which he is, at first, in ignorance, for it is far removed from his formal training and experience. But he can learn, and often does. In fact, he can become a professional. And a professional is a master of his craft.

Let us look at a few brief examples—some hypothetical, others actual—of the man who, of necessity, deals with matters in which he is perforce an amateur. Consider the president of a modern industrial company. He came up, let us say, through the legal profession. Now he deals with production, marketing, finance, and a dozen other fields. He certainly cannot be a master of all. Or the president of a university, called on to deal with fund raising, speechmaking, building, faculty recruitment, relations with government, trends in scholarly disciplines, resurgence of youth. It is remarkable that we have any fully capable presidents of commercial companies, or of colleges and universities. We do have. But in the academic field we do not have nearly enough, and, in view of the turmoil in the colleges as I write, we are likely to have difficulty finding any whatever.

Certainly the leader must be a master of some one phase of operations in order to hold the respect of his fellows. As we subdivide fields of learning and render all of them more precise and more intricate, no man can be a master of all, but a man who sets his mind to it can become a valuable amateur in fields outside his mastership. This is what makes the whole system work—when it does.

There are two human characteristics which account for the fact that we have not just bogged down. The first of these is the fact that a man of intelligence—and only such will fill the need—can become well acquainted with a subject in a surprisingly short time if he puts his mind to it. This is not to master it, but to arrive at a point where he speaks the language and can judge whether a proposal before him has been thought through and is sound. The other is that, if a man is a good judge of men, he can go far on that skill alone.

One is led to believe in these characteristics strongly as he witnesses incidents of real success. My best example is that of Harry S Truman. With experience almost entirely in the area of practical politics, he was suddenly called upon both to manage the most intricate organization on earth and to deal with the tangled foreign relations of a great power in a chaotic world. And he became, I think nearly all will agree today, a great President.

By contrast, the tyro is a nuisance, and can be a menace. Sometimes he not only seizes power by cunning and intrigue; he seizes it at the very top. The worst example we have is Hitler. Much has been written about his dictation to his military leaders. But he was conceited in every other way. Long after such things had proved useless, he had thousands of men on the coast of France building tricky explosive gadgets to be used against invasion. The V-1 and V-2 missiles caught his eye, because they involved revenge against England, and they were built while more important weapons were not. The tyro—the man who has actually no authority of his own but who appropriates that of others—can do great damage. At the top he can even distort the whole course of civilization, as we have good reason to remember.

As I have noted, a common destructive type of tyro is the chap who assumes and flaunts his boss's authority, either without the boss's knowledge, or simply because the boss does not realize the havoc being caused—the morale disintegrated, the hard-working lieutenants disgusted. He can disrupt any organization, civilian or military.

Here is a case in point. The most serious end run by a tyro during the war was by a British citizen named Geoffrey Pyke.[1] I had never heard of him before he appeared on the American scene during the war; one of my British colleagues told me he was a child psychologist, but BBC, shortly after his death, spoke of him as a physicist and ranked him with Einstein. I doubt if Einstein would have welcomed the comparison. But Pyke was ingenious, and he certainly was persistent. He had two main great thoughts.

One of his proposals was for an ice island, to be called "Habakkuk," to float in the Atlantic as a way station for transatlantic planes and a home base for planes hunting submarines. There were many wild ideas of this sort about, but this one took an unusual course.

The story as quoted from a journalist, Wilfrid Eggleston,[2] in his account of the Canadian war effort, goes as follows: "An inventor with a goatee beard produced a hundred page report on a scheme called Habakkuk, and presented it to Lord Mountbatten, Chief of the Combined Operations. Mountbatten sat up in bed until noon reading it." * I doubt if Mountbatten ever stayed in bed that long at any time during the war, but he certainly bought the scheme, hook, line,

* Eggleston, Wilfrid, *Scientists at War*. London, Oxford University Press, 1950.

and sinker. Shortly thereafter Churchill also became convinced and, in characteristic language, sent a memo to the Chiefs of Staff Committee for urgent action. I cannot discover that there was ever a favorable report on the scheme from competent engineers anywhere. But Churchill in his memo passed on some bright thoughts of his own as to how an island could be made by cutting a piece from an ice floe in the Arctic and fitting it with propulsion equipment, antiaircraft defense, and so on.

I had heard that such a scheme was alive, and some of the details as it began to take form. It was, as Eggleston remarks, "audacious and unorthodox." When I discussed it with engineers in O.S.R.D., as I was bound to do if I wanted to avoid surprise, they evidently thought I had some obscure reason for testing their hardheadedness. The plan was to involve a block of ice, reinforced with wood splints or straw, 2000 feet long, 300 feet wide, and 200 feet thick. This latter dimension was a minimum. The block really should be much thicker, for an elevation of thirty feet or so above the surface would hardly prevent storm waves from washing over it. As the plan developed, it included diesel engines to propel the ice block, a refrigerating plant aboard to keep the ice frozen in the warm waters of the Atlantic, workshops, living quarters, etc. Submarines could knock chips off the thing with torpedoes, but presumably they could be frozen back on.

I knew I was bound to hear about this confection officially, although not in the manner in which notification actually occurred, and I knew this notification could cause trouble. So I talked to some of my friends in the Navy. They had long considered an island for aircraft, but not an island made of ice. Their plan involved a steel structure with long cylindrical flotation members so that its platform would be well above the waves, but they had sidetracked the plan on the ground that even such a structure would be too vulnerable. I told them they had better get it out of the files and polish it up as they might need it.

The next thing I heard about Habakkuk was when Mountbatten and Pyke walked into my office. They had evidently just come from the White House. There was no presentation of a proposal, no request that O.S.R.D. should study one and advise on it. Rather, Pyke told me the plan was approved and just what O.S.R.D. was now to do about it. Mountbatten looked embarrassed but not nearly enough so. I listened. Then I told Pyke, no doubt with some emphasis, that I took orders from the President of the United States and from no one else, and that ended the interview. I never had any word on the subject through the very active channels for interchange which we had with British science.

As I expected, it was not long before the President brought it up. He did so in the casual way in which he usually asked me about all sorts of things, and wanted to know what I thought of the idea of an ice island. I told him, "I think it is the bunk. If we want an island, the Navy has a far better idea for one." He never mentioned it again. He may have consulted the Navy, but I doubt it.

These two short interviews probably spared this country the waste of a million man-hours of work by scientists, engineers, and technicians who had much more realistic things to do. The Canadians went ahead with the program. The National Research Council at Ottawa took it on. "The order stemmed from the highest authority, its temper was mandatory, and as a result a tremendous flurry of activity got under way in Canada," as Eggleston tells it. Much of the effort of some of Canada's most able scientists and engineers was thus used. The scheme was, of course, ultimately judged to be impractical. One could build a good aircraft carrier for the cost of a Habakkuk and it would not melt.

Pyke charged in with another grand idea. This one had a side dividend or two, even though Pyke's plan itself miscarried. It came down through channels, although rather unusual ones. The sequence apparently was: Pyke to Mountbatten to Churchill to Marshall to General Moses to me. If Roosevelt was in the chain of transmission, he did not mention it to me.

The plan,* which originated in April 1942, was to build a large number of snow vehicles and have them ready to be dropped by parachute into Norway in November. There they were to attack, among other things, power plants where heavy water was being made, it being then thought the Nazis needed it badly in their atomic energy program. The vehicles were to be small and light enough to be carried by a British Lancaster bomber, rugged enough to stand the drop, fast enough so that the Nazi ski troops could not catch them in the

* Milton Silverman tells the whole story in "Project Weasel," *The Saturday Evening Post*, Vol. 218, No. 3, February 9, 1946.

snow. Pyke had a grand scheme for propelling them by a pair of Archimedes' screws.

When General Moses turned to O.S.R.D. for help, Hartley Rowe's section, and notably Palmer Putnam and his group,[3] went to work. My first contact with the plan, and my first contact with Pyke, was at a meeting where General Moses and General Hoag[4] represented General Marshall; Pyke and Colonel Wedderburn represented the British; and Rowe, Putnam, and I represented O.S.R.D. I was naturally skeptical about it, questioning supply, weapons, communication problems, and the like. I also wondered whether surprise could be attained, for otherwise the heavy water plant would be well protected, and snow vehicles would make good targets. I was not consulted by General Marshall before my people were called in, which did not trouble me, for it happened all the time, and Army and O.S.R.D. relations were in this case excellent. But I assumed, naturally, that the plan had been given rigorous study in Britain before it was mounted full scale, and given high priority, so I agreed to go ahead.

Things had to happen fast, and they did. Putnam carried the ball throughout, and he certainly made it move. While the basic design was in process, a rugged search for a proper test site—with the right kind of snow and plenty of it—took men to Alaska, Banff, Chile. Snow conditions on the Columbia River ice fields north of Banff finally turned out to be right, and a road was built to give access to them while the new design was being rushed into prototype stage. Army engineers, incidentally, built the entire road in twenty days, which shows the pace of the whole affair. Pyke's cherished Archimedean screw would not work, as was readily apparent to any competent engineer, and was discarded over his protests. Putnam designed a caterpillar type, with very wide rubber treads which gave great promise, and it was rushed into production, before its design was fully worked out, at a Studebaker plant. The details of this mad rush against time would fill a book. The net result was that a fleet of vehicles was built, capable of negotiating hard or soft snow, mud, and sand, with reasonable speed and life, and all this was done in a small fraction of the time normally required for such a job. The tough, versatile machine was called the Weasel. A special regiment had been trained to use it, and a thousand of the versatile vehicles were delivered to the regiment by the end of 1942.

Then the whole program was turned down flat, and it was decided to rely on fifth-column sabotage to knock out the power plants. Apparently Air Marshal Harris[5] whose planes were supposed to be used, heard of the Weasel for the first time only when its major problems had been solved, and he said "no" with emphasis. My recollection of the affair, as I heard about it at the time from Marshall's aides, is this: Churchill presented the plan to Marshall when the latter was visiting him at Chequers, and told him, "My people have looked into the problem. They conclude the best way to do it is by dropping snow vehicles from Lancasters." Marshall naturally assumed the whole plan had been carefully reviewed and endorsed. So did I. It was only after much hectic effort, and interference with other plans, that I found that General Harris, in charge of the Lancasters, had never heard of it. One is reminded of the remark of the switchman who observed two trains headed for each other on a single track. "That's a hell of a way to run a railroad."

But the Weasel was not a total loss. In fact, before the end of the war it had accomplished fine things in one theater, for it carried men and supplies to the front in Italy through mud and snow, and carried wounded back. Of course this was in no way commensurate with the confusion and delay it had caused, for it was given priority over the B-17 for ninety days and caused confusion in W.P.B., the Engineer Corps, and O.S.R.D. This on a project which was never evaluated before it was launched, and which was merely an instance of Churchill's unbridled enthusiasm.

While the Weasel project was under way, I found that brother Pyke in his role of tyro was roaming all over my organization, apparently disregarding security boundaries, even interfering with contractors. I issued an order to those in O.S.R.D. who were most involved—one of the very few issued throughout the war—directing every man who had a contact with Pyke to put in my file promptly a full memorandum on the conversation. It is fortunate that I did.

I received some remarkable notes of conversations as a result. One recited that Pyke had boasted that he carried some of Churchill's secret files, also that the P.M. had urged F.D.R. to fire a chap named Bush.

This didn't bother me much. I didn't believe it, of course. And it resembled another incident when a chap named Ingersoll,[6] in a journal called *PM* (not named for the Prime Minister, however), charged Conant and me in issue after issue with suppressing the synthesis of quinine for the benefit of quinine producers. When I did not reply, some of my young associates urged me to tell the real facts. But I assured them that, if one were attacked by a sheet like *PM*, one should just relax and enjoy it. For no one would believe it, and no one would copy it. The real benefit of absurd attacks is often overlooked. So I didn't mind having Pyke take cracks at me, although I did wonder later whether this was one reason why the P.M. did not seem to welcome me with open arms.

But I did take one precaution. I visited Secretary Stimson and gave him my collection of Pyke memoranda. I didn't do any more than this, although when Putnam brought me the message that Pyke was intent on getting me fired, and I asked his advice, he said that, in my place, he would get Pyke deported in twenty-four hours, which I thought was excellent advice.

Then I heard that Pyke was leaving us. Just about that time I also heard that Pyke had gotten General Marshall out of bed at 2 A.M. with a message on Habakkuk. I never found out whether this arrogant phone call caused his abrupt departure; it probably did. That morning I happened to visit General Walter Bedell Smith, then Secretary of the Joint Chiefs of Staff. I told him I had heard that a prominent Britisher was leaving us and that we both should go out and see him off. He looked at me bewildered for a moment and then said, "Yes, I'll put on my heavy boots." Thus ended the saga of Pyke in America, the saga of one kind of tyro—the dangerous kind.

Now let's look at amateurs for a change. A marvelous group of them are found among men testifying before congressional committees. I do not have in mind the flamboyant hearing, with glaring lights and television, but the run-of-the-mill hearings on all sorts of subjects. In my experience some of the citizens who appear and testify in these latter hearings are not notably adroit, and the reason, I think, is that they act as though they were going to speak at the local Rotary. This is a bit strange. If a ball team is going to play in an alien ball park, the first thing it does is learn the local ground rules. If a prominent business executive finds his diameter expanding and decides to play golf, he at least gets the pro to caution him not to wabble his head. If he stages a cookout for his neighbors, he gets his wife to tell him how to gauge when a steak is done. But the same man just walks into a hearing room with no preparation whatever, testifies, and often looks foolish.

The reason, it seems to me, is that the practice of politics is not generally regarded as a profession, despite the fact that it is not only an honorable profession, but is the one profession essential to the operation of democratic government. Like every other profession, it has its exhibitionists and scoundrels. But, I fully believe, it has as large a fraction of devoted and hard-working practitioners as any other professional group. And it certainly has its own set of accepted practices and its own code of behavior.

I speak here as a man who was once a rank amateur at testifying. The first time I appeared before a committee I was scared, I did a poor job, and I did not get the full appropriation I was looking for. I made a number of mistakes; the worst one was that I got mad. It is all right to get mad and all wrong to show it; I showed it. Some member of the committee made a remark to the effect that I did not make sense with my figures, and I came right back to say it was simple arithmetic which most people understood readily. Turmoil followed. Why? Because the chairman did not want near insults left in the record. So we mixed it up for a while, then there were apologies, then we all agreed to cancel the record and start over again. But the damage was done.

At another hearing I got badly caught because I had not done my homework thoroughly. We asked for funds to complete a building addition. Representative Taber[7] asked me why we were putting a brick addition on a wooden building. I did not know we were, and hence looked foolish. I did have the sense to assure him that there would be no more brick unless I knew about it. After that, my colleagues and I, when we faced what might be a serious inquiry, had practice sessions in advance.

In this fashion in those early hearings, before the war, when I was appearing as chairman of the National Advisory Committee for Aeronautics, I learned something of the manner and method of appearing before committees of the Congress. I began to do better, and then I did not mind appearing. And, finally, I got to where I thoroughly enjoyed it. There was just enough hazard to give it zest. And I found that, when my inquisitors discovered that I genuinely admired those among them who were thorough masters of their strange art, they were inclined to reciprocate.

For they do have a difficult art. One time I was before a committee on a tough technical matter, a Navy problem of some sort. I was none too anxious to testify, for my Navy friends would not be happy to see me rambling about in their backyard, but one has no control over where a committee will head in, if the chairman consents. Toward the end of the hearing a congressman said to me, "Doctor, how do you expect us to understand as complex a technical matter as this sufficiently to pass judgment on it?" I replied, "I don't," which alerted any that were taking a mental vacation. So he said, "What do you expect us to do?" I thought a minute and said, "I expect you to follow your usual practice. You will listen to a number of men; you will decide which ones make sense and know their stuff, and you will go along with them. Moreover," I added, "you are all good judges of men, or you would not be elected." This is, after all, about the way in which the system operates, and, taken generally, the system works.

One thing a witness can be sure of, in routine hearings, is that the committee members are thoroughly bored. Hence they welcome a bit of humor. But the welcome is a conditional one. To be really effective the humor must appear to be accidental. The witness who undertakes to be an intentional humorist is frowned upon, for very good reason.

One time I was seeking appropriations for wartime medical research. With me was Dr. A. Newton Richards, the recognized dean of the medical fraternity. It was toward the end of the war, and Richards was feeling tired and a bit old. One of the congressmen asked him, "Doctor, will all these researches you are carrying on tend to lengthen the span of human existence?" "God forbid," said Richards, smack into the record. The committees were always gentle with him, for his integrity and caliber were impressive, but they enjoyed that one. When the record came down from the Hill for correction, I sent it first to Richards, and when it came back, I found he had crossed out that question and answer. So I restored them. I thought the passage ought to be in the permanent record of Congress.

Committees are not always gentle. One day I was preceded on the stand by Irving Langmuir.[8] He was doing his best to explain something, but he was doing it as though he were talking to a class of undergraduates in physics. The committee did not like it; but they did not know Langmuir, so they were polite. They did know me, and I expected their annoyance would be saved to descend on me when I took the stand. Fortunately, their first question took me off the hook, for it was related to the social sciences. I told them that I was not a social scientist and that I really ought to ask them the question, for they were at least able practitioners in one branch of the social sciences or they would not be there to ask questions. There was no more stress. And it is true: Our unique system of government could not operate if it were not for the politically able. Let me go a step further. It could not operate if it were not for the fact that there are sufficient numbers of men who have a good grasp of the problems we face as a nation, and have also political skill and the ambition to exercise it. It is altogether too seldom that we pay tribute to such men. But I do believe we come far nearer to a proper evaluation than was the case a generation ago. I even believe that there are now scientists among us who realize that to become a successful politician requires as much intellectual prowess, although of a far different kind, as it takes to become a successful scientist. But I do not think Langmuir thought so. And the committee, being made up of men who understand men, knew this.

To digress for a moment, I understand that, in France the general opinion among the populace is that their representatives are a collection of crooks. I believe there have been times in this country when the same opinion prevailed. I am sure it is not the general opinion today, and that is a good thing. But I do find, when I am among friends and say something nice about Congress, as I have just above, I see a raised eyebrow here and there. I suspect this implies that I have lived a sheltered life, in an ivory tower, or that I am adept at wishful thinking, and hence blind to the grosser characteristics of my fellowman. On the contrary, I see Congress or congressmen doing foolish or damnable things almost every time I pick up the paper. Recent examples? Probably the most conspicuous is the shilly-shallying over postal reform, strung out until finally the postal workers launched their illegal strike and came near to tying up the whole country. And earlier than that we heard the House joke and refuse to kill rats, just at a time when rats in tenements were no joke at all. Or the Senate passes an amendment which would destroy collaboration with a number of European countries on gold, just when we badly need all the help they can give us to protect the dollar against speculators. And the Senate passes, with only one no vote, a resolution on ethical standards for senators which does not mean a thing, and which they know does not mean a thing. I could multiply incidents; there is one just about every week. It is democracy in action, with all its crudities; still democracy is better than any other system of government ever tried. And I still maintain that almost all congressmen are honest, trying to do a good job.

Sometimes in a committee room an inquisitor is mean, for the sake of the papers back home, or just because he feels mean. One time, before the war, I believe, Nelson Rockefeller [9] and I appeared before Senator McKellar,[10] chairman of the Appropriations Committee of the Senate. Rockefeller went on first and was harassed and insulted. McKellar would ask him a question and then talk to the clerk. Or he would ask a question and when Rockefeller was answering would interrupt by asking another. Rockefeller had a rough time. Then I went on and had no trouble whatever. When McKellar tried one of his nice tricks I would just light my pipe and wait. If he asked two questions at once, I would just say, "Which one?" After a bit of this other committee members told him to get on with the job. In the car later Rockefeller said, "Why, Van, did I have a rough time and you did not?" "Well, Nelson," said I, "your name is Rockefeller." "I know," he said, "but there was more to it than that. What was it?" "I'll give you my diagnosis for what it is worth. You went on the stand and looked worried, so he raised hell. I acted as though I did not give a damn, so he left me alone." "Was that it?" "That was it." "Boy," said Rockefeller, "I will certainly proceed to learn that lesson." I think most of my fellow citizens will agree that he learned that lesson well, and many more, in the political arena. I had been an amateur and so had he, but not for long.

A congressional hearing can be a farce for one of many reasons. I tell the following story lest some of my readers think I have altogether too much respect for our system of committee inquiries. During the war, as I have noted, McKellar was chairman of the Appropriations Committee of the Senate. This sort of thing makes many a citizen squirm when he views our congressional seniority system. Let us say that the Appropriations Committee had an important function in the prosecution of the war, and that McKellar was not the most able man in the Senate. At any rate, one day he called about six government agencies at the same time to a hearing before his committee. Work stopped all over town; we trekked up the Hill and cooled our heels in an anteroom. Irvin Stewart and I were finally called into the great presence. Only McKellar and the clerk were there. The first question the senator asked showed that he did not know what agency he had before him. So I did not enlighten him. He would ask a question which had nothing to do with my agency. I would give him a long answer which had nothing to do with my agency, and not much to do with his question. We kept up this sort of shadowboxing for ten minutes and then he adjourned the meeting. I went back to my office wondering whether a democracy could fight a war. No transcript of the hearing ever appeared.

Having reviewed this absurdity, let me recite a case when the democratic process most certainly did work during the war. The Manhattan District spent a considerable sum in developing the atomic bomb. This money was appropriated as a part of the War Department funds, but in a form which did not reveal its nature. Each year Secretary Stimson and I went to Capitol Hill and presented the matter to a small group of congressional leaders from each house. The full story was told. I traced the status of the program, the time schedule, the problems giving trouble, the calculated power of the bomb, and the estimated enemy progress, and then answered questions. Then, when the item came up in Appropriations Committee, one of these men would vouch for its justification. There was never a hitch. There was never the slightest leak of secret information. There was never an instance of a member of Congress prying into the affair. I think of this once in a while when I am discouraged about the way we make our laws and conduct our national business.

My last meeting with the Appropriations Committee was somewhat informal. When the war ended, Irvin Stewart and I secured funds to carry the remnants of O.S.R.D. while we closed out contracts, and we told the Committee we thought we could do this in one year. But, at the end of the year, the job was not quite finished, so I went back. When I appeared there were remarks around the table: "Look who's here." "We thought we had seen the last of you." "I thought you did not want more money." I said, "That's a hell of a way to receive an old friend. I just came up to pay a social call and see how you are getting along. I do not want any money. I want sixty thousand dollars, but that is not money."

They sat me down and asked a few questions to have something in the record, and followed this with questions on weapons which had just emerged from wraps. Then Congressman Cannon said, "Doctor, you held out on us." "Why, Mr. Chairman, I never held out anything from this committee." "But you did not tell us about atomic energy." So I told them, "You remember, every time I appeared before you, I put a red-covered book on the table and told you that it contained an account of everything my agency was doing." "I remember all right," said Cannon, "and I remember stating that it was a very dangerous book, and telling the clerk to guard it carefully. But the atomic energy matter was not in it." "Oh, yes, it was, Mr. Chairman." "But," said Cannon, "we would not have understood it if we had read it." "No, you would not have understood it," said I, "but it was there."

As a matter of fact, at the end of each session, the clerk gave me back the book. I took it to my office and put it in the safe, and not one of the Committee ever opened it. But this was far more than a gesture. It was a symbol, and an important one. By its use I had said, in effect, to the Committee, "You are entitled to know anything you wish to know about the program of O.S.R.D.," and they had replied, "We wish to know nothing." It was this sort of thing which produced between us a spirit of confidence, and it was a mutual confidence. They trusted O.S.R.D. and backed it. I acquired a confidence I had not had before in the workings of the democratic process in time of war.

There is a myth to the effect that the witness before a congressional committee is helpless, a victim to be pitied. This is not the truth; he is far from helpless. If he has studied the committee before appearing (and he deserves to be helpless if he has not), he will know just who is likely to give him trouble, and also where the individual is vulnerable. Not that he would take advantage of that vulnerability, not at all. But he might edge up to it a little if hard pressed. Moreover, unless there is a real reason for frying a witness in public, the committee itself is intolerant of a member who departs from congressional courtesy, which is real, belief to the contrary notwithstanding.

For example, once I was before a committee on which there was a new member who did not know me, and he was inclined to go after my scalp, probably merely because he was bored. He would ask what was supposed to be an embarrassing question. I would look very innocent and answer something else. After a while he pounded the table and said, "Doctor, you evaded my question." "Well, you know, Mr. Congressman," I said, "I have had a lot of practice in doing just that." The committee laughed at him, and he laid off.

Now I freely admit that, in a hearing that is front-page stuff, a witness can have a rough experience. But I have never seen an instance in which a witness was actually prevented from stating his position. I have seen committees get very vigorous—in cases where I would have liked to join them in scalping a witness. But I have never seen a committee be cruel to a man they did not think would be able to defend himself. No man should enter the political arena if he has a thin skin. Any man who appears before a congressional committee, on his own initiative, is certainly entering that arena. My point is that before he does so he should study the system into which he is entering and attempt to understand it. If he does not study it, then he remains an utter amateur by choice.

So much for the drama of committees and for the serious business which underlies that drama. I want to return now to another aspect of industrial efforts, where the effect of the tyro can be tough. A problem in research organizations and in business generally is the presence of the tyro inventor. He is likely to be short on fundamental physics and long on persistence. He is often convinced that all the rest of the world is out of step, and that there is a cloud of ignorance and reactionary thinking about. In other words, he can be an unholy nuisance. But there is another side to the matter. Bright ideas do sometimes come from the amateur inventor, and the professional ranks should not be closed against him. Wisdom distinguishes the amateur from the tyro, in invention as well as in everything else.

This matter is of great importance in the relations between men in uniform and the civilians during a war. In spite of the old cliché that military men always plan to fight a war with the weapons of the previous one, senior officers of the last great conflict in general welcomed the efforts of civilian scientists and engineers, worked with them, came at last to understand them. If this had not been true we would, indeed, have fought largely with only the weapons of World War

I. But still there were officers in the top ranks who regarded civilian efforts as presumptuous and a nuisance, and who sometimes set up rather effective roadblocks, as has been discussed earlier. The presence of the tyro inventor in our midst did not help in this regard. Fortunately we had an effective system for taking care of him. And, also fortunately, we had a system where the sound individual with a good idea, the amateur, could get a reasonable hearing.

Let me give an example or two of how wild inventors can be. To do so I am going to digress to describe a joyful occasion, the final dinner of O.S.R.D. When the war ended the men of O.S.R.D. were fully as exuberant as the rest of the population. The organization promptly disintegrated, as nearly everyone headed back to his old haunts. But we had a dinner in celebration, and it was quite an affair. All the top brass of O.S.R.D. were present, the members of N.D.R.C., C.M.R., the Office of Field Service, the Council. Also present were the Joint Chiefs of Staff, the Secretary of War, the Secretary of the Navy, officers who had worked closely with O.S.R.D. in various ways. Forrestal said he would come but had to leave by nine o'clock. He was still there at midnight.

As each man entered he was given a set of "orders" marked, not simply "Secret," but "Unmentionable." It assigned various tasks to be sure the proceedings did not bog down. Various civilians were furnished with issued United States patents—perfectly genuine patents on weapons—and they rose and queried the appropriate military officer as to why these weapons were not being used. One, I remember, was a plow that could be converted into a gun by the throw of a lever. Another patent described a boom projecting out in front of a naval vessel. On the end of the boom was a switch. When the boom hit something the switch reversed the engine, and thus the vessel avoided a collision. There were a number of other good ones—all inventions by amateurs.

The highlight of the evening, in passing, however, was a speech by Admiral Nimitz.[11] Although in uniform, he was sure he was safe in that company, so he proceeded to build up in his speech to a nomination for President of the United States. Both Conant and Eisenhower were there; both had been mentioned in the press as Presidential possibilities, and I was thoroughly enjoying watching them squirm. Then Nimitz got to his climax and nominated me, and I proceeded figuratively to crawl under the table. The next day Eisenhower went into Walter Reed for one of his regular checkups. I met Nimitz that day, and he told me jokingly he was afraid Ike could not take it. Ike could, all right; I am sure he enjoyed it as much as I did. And, when it was all over, O.S.R.D. disappeared from the scene, leaving only a small piece of the business office to close out a few contracts. We demobilize, in this country, all too rapidly when a war is over. But this rapid demobilization of O.S.R.D. was inevitable.

We had from the beginning, as I have noted, a National Inventors Council, independent of O.S.R.D., but in close contact with it. It did yeoman service. It received suggestions from the public, over 100,000 of them, and every one was reviewed by competent scientists or engineers, and, if of possible utility, by military men. Out of all this came some useful suggestions, but mostly on relatively minor matters. This was to be expected, for improvements in the more complex areas could be made only by men who understood thoroughly what was currently being used, most of it secret, and who also understood conditions in the field.

Suggestions could also go directly to O.S.R.D. Any engineer or scientist with an idea on a weapon, or for that matter on a subject of military medicine, was bound to have a friend, or many friends, in the far-flung O.S.R.D. organization. Such a friend could readily direct him to the right spot. Since O.S.R.D. was a pyramidal unitary organization, it was not necessary to cross any boundaries between semi-independent departments or committees in giving such directions. At the proper spot the man with the idea would be heard, and told nothing, by a man deep into the subject. If his idea did not seem absurd, after the section considered it, he might well find himself soon working on it, with a contractor or even with the section itself.

Most of the amateurs with whom we thus dealt were honest, patriotic citizens trying their best to help. I hope that, on the whole, we made them feel we appreciated their efforts, and that we abhorred brush-offs. I remember one chap who followed me all over Washington for several days. When I went into an office he would camp outside and wait until I came out, and then follow again. I became a bit concerned lest he have some private reason for wanting to stick a knife in me or the like. But an intelligence organization—not mine; I did not have any—soon told me that he just had an idea so important in his own mind that it must come directly to the top boss if he could find him. So I listened to him, and the big idea was absurd. But he quit following me.

On another occasion my secretary, Sam Callaway,[12] told me that he wanted me to talk with an old man who was in the outer office. Now Sam was the finest secretary, bar none, that a harassed executive ever had. So when Sam told me to see someone, I did. It turned out that the old man had driven his Model T down from somewhere in the Blue Ridge Mountains to present an idea—I think it was on a way to trap submarines. I listened as he described it in detail, and did it well, with a lot of good sense. Then I told him that we had to be very careful with ideas of that sort, to keep them out of enemy hands. Why, if we were hard at work on that very idea, I could not tell even him about it. Otherwise enemy agents with fake ideas could learn a lot. He agreed heartily, his eyes shone, and I suppose he headed home happy. The idea itself was one we had received dozens of times.

What he did not know was that I was no expert on submarine warfare, or any other weapons system, and that, for a full review, I would turn to those who were. When I needed to be reasonably up to date on antisubmarine devices, or guided missiles, or antiaircraft fire control, or radar countermeasures, for some conference at a top echelon, I would cram for the session as though I were about to take an examination before a mixed group of professors.

Of course, when the war began, we were nearly all amateurs in regard to new weapons. But after a while, a year or more, those directly on the job became professionals. The Radiation Laboratory at M.I.T. became a hard-working collection of true professionals on the subject and accomplished major advances in the whole field of radar. At one time early in the war I suggested to Lee DuBridge, director of the Laboratory, that he had an oversupply of physicists in the team he led, and not enough engineers. He did not agree, and of course I did not interfere. But late in the war he said to me, "You see we did not need the engineers." I told him, "Hell, any self-respecting physicist can become an engineer in a year or two if he puts his mind to it." Scientific personnel became so scarce they even took in biologists and made radar experts out of them. Thus, in all sorts of fields, civilian scientists and engineers, rank amateurs, gradually became professional men, thoroughly competent in the development of weapons, from incendiaries to guided missiles.

The men who operated O.S.R.D. came from all over the country, and when the war was over they went back. They came mostly from the colleges and universities, but also from industry. Most of them went back to their old haunts. But many a young man among them had tasted real responsibility for the first time, and some of them liked it. This was true also of young men in the services. So, after the war, many of them struck out on new paths. Some went into academic administration, where they were much needed, and of these quite a few became college presidents. Some founded their own small businesses, often around an instrument or material for which they sensed a market, and some of these little businesses grew. This was much aided by federal subsidy of research in the postwar period. But most of it occurred because young men had the foresight to recognize the opportunity and the courage to grasp it.

War brought grief to us in many ways—far less to us than to Europe—but there were compensations. I have heard it maintained, and I have no reason to doubt it, that the medical advances during and just after the war, in antibiotics, steroids, etc., saved more lives within a few years than were lost in battle. One may be troubled because much of this improvement occurred in areas where the population explosion threatens to cancel all the effort directed to raising the standard of living. But there is more to it than this. People are in better physical condition, thanks to such gains as those in the conquest of malaria, and only healthy populations can make a sound advance.

The war also jolted us out of our complacency. There is something in the thought that we can withstand adversity but that it is more doubtful whether we can stand affluence. Certainly it is true that, when all seems to be going smoothly, many young men are far too prone to think more about security than of pioneering. Unfortunately, also, there are far more youngsters who think they want responsibility than there are who actually do want it, or who can handle it if they get it. The war shook things up. And out of it came pioneers, in industry and also in scholarly endeavor.

I could cite dozens of cases of this sort. In fact I wish I could write a whole book, in tribute to my war comrades. I will pick just one or two. I choose George Kistiakowsky,[13] not just because he is an old friend and I admire him greatly. He has been called a scientific politician, and hasn't minded it, just as I have been so called, and haven't minded it either. Also, he is a chemist, and the chemists have not been rendered their just due. But I think the real reason I choose him, as an example of the comrades with whom I worked during the war, is that I think he has had a lot of fun living, more perhaps even than I have. He had a remarkable career before World War II. A Russian, he joined the anti-Communist White Army when the Russian Revolution broke out, fought two years, was wounded, and finally escaped to Turkey. Later he got to this country. And after a while he became a professor of chemistry at Harvard University. He might have stopped there, for a professorship offers a pleasant life. He already had enough career for an ordinary mortal, but not for Kistiakowsky.

He was an amateur on all matters of military technology when the war started. But he became a recognized professional expert on all aspects of modern explosives, and he was an important member of the team that built the atomic bomb. Wherever he appeared the pace of development stepped up, for his example was contagious.

One incident will illustrate his attitude. Fairly late in the war President Roosevelt got the idea—I never found out from where—of sending a scientific mission to Russia to exchange information on weapons. I was appalled, and as a matter of fact ultimately sidetracked this into a mission on military medicine, which Dr. A. Baird Hastings[14] carried out magnificently, and with benefit to the Russian effort, if not to us. But it looked ominous for a time, and one evening Conant and I were discussing it in a garden in Virginia. Kistiakowsky was sitting in the shadow under a tree listening to us and saying nothing. But suddenly he spoke up, "Look, Jim, if you send a mission to Russia, I want to head it." To which Conant replied, "Why, George, if you went to Russia, they would probably shoot you." There was quite a pause, then, "Oh, I don't think so. At least I'd like to try it."

When the first atomic bomb went off at Alamogordo, Kistiakowsky, who had much to do in the final arrangements, was supposed to be inside a bunker at the six-mile point. Apparently he was not; he was outside for a better view. The story goes that when the explosion occurred he was scaled along the ground in the mud formed by a shower that had fallen just before. At any rate, when I saw him soon after he was plastered with mud.

After the war Kistiakowsky did not settle down to a quiet life. For one thing, he served as the scientific adviser to the President of the United States; in other words, he was the most influential man in the country on the relations between scientists and the government.

To return to my argument, it is a far cry indeed from men like Kistiakowsky to the tyro who, attached as an assistant to a man with wide authority, does not know the difference between staff and line, or else knows the difference but disregards it. Such a tyro is adept at making trouble. He is more likely to be found in civilian than in military circles, for young officers are well instructed in lines of authority and the functions of the staff, and if they disregard such instructions, they do so at the peril of losing their chances for high command. I have noted one civilian tyro who annoyed the military. But rampant tyros do occasionally appear within the military structure themselves.

Probably the best example of the havoc which can be caused by a staff officer of this breed occurred during the first world war. The formation of the great German General Staff was regarded as an important step forward in military organization. But at the Battle of the Marne a staff officer, of the rank of lieutenant colonel, was issuing orders in the name of the General Staff to the commanding officers in the field. These were not just interpretations or explanations of orders the field commanders already had in their hands; they were alterations which produced new strategy. Exactly what occurred has been a matter of controversy ever since. But there is no doubt about the result. It was confusion, conflicting efforts, failed opportunities. In the interval General Mangin [15] had time to move troops from Paris up to the front in taxicabs—a spectacular and well remembered exploit, but not the crucial matter. That was the shifting about of German armies in the confusion so that the German lines were penetrated, the Marne was crossed, and Paris was saved.

This lesson has not been forgotten. The military principle is that every officer, from the young platoon commander to the head of an army group in the field, knows exactly the scope of his authority within his own area, and over his own troops and equipment, where he operates under orders which define his mission. These orders may be changed by higher command. But his operation within those orders is never interfered with. If he cannot perform he is relieved, but no one tells him how he must do his job. A staff officer can transmit orders from his boss to clarify a mission, being exceedingly careful that this is all he is doing and that everyone involved knows it. A staff can also take a general order from its chief and break it down into a set of detailed orders; in fact, that is often its primary function. The really good staff man can even, in an emergency, alter orders on his own, if he knows his boss's plan well enough, but he will do this with care and check up promptly. But the staff officer who steps over the border can be a menace. There is no job which calls for more good common sense than that of the staff officer, or of the deputy to the chief in any organization. I am no expert on military organization, but it does not take long to learn this much.

Another principle is that tasks or missions should not in general overlap, or that, when some duplication is inevitable, there must be constant staff action to avoid gaps or conflicts. Corporations can get into trouble on new products in two ways: They can fail to produce them on time and let their competitors get ahead, or they can get them out too fast and have them fail in the field. Either is usually the result of lack of proper coordination. We are all familiar with this sort of thing. Research says, "Unless we have six more months on this, it will not work properly"; Production: "If you do not have some of the gadgetry cut off, we cannot make it for an allowed cost"; Marketing: "We cannot sell this thing the way you have it." Here is where staff work is essential, not to make the decision, but to see that the boss has full information and that he does make it. In fact, as full information and opinions are gathered in explicit form, controversies will often settle themselves. The tyro aide who does not exhibit reasonable modesty, or who butts in merely to imply delegated authority, can produce a Battle of the Marne in any organization.

During World War II we came far along the road toward proper effective organization of armed forces in the field, but we were by no means perfect. Despite the arguments that occurred afterward, the handling of command, for example during the Battle of the Bulge, was correct. This is not to say that it was perfect; it was correct. Divisions were transferred from Bradley's command to that of Montgomery, and transferred back somewhat later than was reasonable. The point, though, is that Eisenhower, with Bedell Smith at his side, kept it entirely clear at all times just who commanded what, and just what each army group commander was supposed to do. Moreover, he did not interfere improperly with the way they handled their forces, nor did he allow anyone else to do so. And this while he faced that most difficult of tasks, the directing of allied forces in the field.

This, and all such matters, should be treated by military analysts, and I write as a layman. But analysts and participants have written much on the subject, presumably for people like me to read, and having done so, they should not object to my expressing an opinion. So let us turn to another military affair where we nearly had a disaster. At the time of the Battle for Leyte Gulf we had two commanders in the field, General MacArthur [16] as Supreme Allied Commander, Southwest Pacific, based in the Far East, and Admiral Nimitz as Commander, Pacific Fleet and Pacific Ocean Areas, based in Hawaii. Responsible to Nimitz were Halsey [17] with our Third Fleet and, through him, Mitscher [18] with our Fast Carrier Force. Under MacArthur's command, in addition to the Sixth Army under Lieutenant General Walter Krueger,[19] was the Seventh Fleet, commanded by Vice Admiral Kinkaid [20] and comprising two attack forces and a group of escort carriers. The actions of the two field commanders, MacArthur and Nimitz, were coordinated only by the Joint Chiefs of Staff in Washington. It is this situation which the Navy's historian characterizes as unfortunate * and of which another historian remarks, "Perhaps the most important single conclusion to be derived from the study of these events is the vital need for unity of a command in conjoint operations of this kind in place of the concept of control by co-operation such as existed between MacArthur and Halsey at this time." †

We here need pay attention to only one incident in the four-part struggle known as the Battle for Leyte Gulf. This occurred when

* Morison, Samuel Eliot, *The Two-Ocean War.* Boston, Atlantic-Little, Brown, 1963, p. 432.

† Churchill, Winston S., *The Second World War,* Vol. VI, *Triumph and Tragedy.* Boston, Houghton Mifflin, 1953, p. 186.

the main Japanese force came through the San Bernardino Strait and started down toward our transports at Leyte. Halsey, with the modern battleships of the Third Fleet, which incidentally never saw real action, and with the fast carriers of Mitscher's force, was headed north after some Japanese carriers, which as it turned out were there as a decoy. Kinkaid, whose Seventh Fleet had just won at Surigao Strait a battle magnificently handled by Rear Admiral Jesse B. Oldendorf,[21] apparently did not even know that Halsey had gone north. So our troops and transports at Leyte were covered only by the six escort carriers under Rear Admiral C. A. Sprague[22] with three destroyers and four destroyer escorts as screen, plus such air support as the rest of the escort carrier group could supply from substantially distant stations. Sprague's escort carriers put up a fight against heavy odds which was magnificent, and also hopeless. If the Japanese had kept on, this covering force would have been annihilated, and our troops and transports would have been left wide open. But the Japanese turned back, probably because they could not conceive that we would leave the San Bernardino passage unguarded for long, and passed through the Straits before Halsey returned. The battle as a whole was a great victory, which was the beginning of the end of Japanese naval power. But this incident illustrates very forcibly the need for unitary command in every war theater. Where was the action of our fleets coordinated? Not in the field; there MacArthur and Nimitz were independent. Not at the Joint Chiefs level; the Army and Navy Chiefs were equals, and King could certainly not transmit orders to a fleet through MacArthur. Apparently only the President, as Commander in Chief, could act to be certain the fleet actions were properly interlinked. And this is patently absurd.

Now this illustration is presented not to emphasize the necessity of unitary theater command, for that lesson has been thoroughly learned, but for another reason. Here was a situation where staff work would not have helped, would probably have made things much worse. There were, fortunately, no emissaries of the President about, exercising his authority. The point is that no amount of staff work can make a poor organization into a good one. And when staff members try to do this, they can produce complete confusion. Nor is the illustration presented to criticize Halsey. He may not have been our most brilliant admiral, but there was none who excelled him in producing loyalty and devotion to duty in a fleet of ships. The confusion, and the approach to disaster, occurred because the organization was wrong, and fleet actions were hence improperly coordinated.

We face a tough problem on the whole subject of sound military planning. That there is something wrong today is clearly in the minds of the American people. Some of this rests on incidents, fortunately rare, which occurred during World War II, where most of our planning was excellent. But we did make an assault on Tarawa, taking over 70 per cent casualties in the first wave on the beaches, because an obtuse admiral was thinking in terms of a previous generation. And, as I have noted, another admiral was allowed to let our great battleship fleet go off on a wild hunt and miss a battle. Then, in Korea, we advanced to the Yalu and took a beating, because we did not correctly estimate Chinese intentions and capabilities. The story is not yet told, and some day will be, but there are many, and I am one, who think we got ourselves in a mess in Vietnam largely because Kennedy and Johnson received military advice which was far off the mark in regard to capabilities and timetables. And this was after the French in Vietnam earlier had been thoroughly defeated, and had given us plenty to think about in regard to guerrilla and jungle fighting.

One thing should be emphasized at the outset. Military planning that is strictly military should be carried on by military men. Of course, when political matters are involved, the military planning should be merely a base for broader decision.

In this country there is no doubt that the military services must be and are subordinate to the civilian power, resting ultimately in the President as the Commander in Chief. This is accepted genuinely by military men. We need only to look about us in the world to realize that this is a relationship which must be maintained. Whatever we may think about the details of the confrontation between Truman and MacArthur, there must be no defiance of the civilian authority if we are to avoid disaster. And along with the acquiescence of the professional military men in this thesis there should be a converse principle: Civilians in authority should not interfere during war or peace with the judgment of the military men on military subjects. Genuine acquiescence should proceed in both directions.

But if military planning and judgment is sometimes faulty, as it is, what do we then do about it? The first thing to do is to seek for causes. One cause seems to me to be clear. When an army fights on land, or a fleet on the sea, it is essential that there be immediate unquestioning obedience to orders. Anything else invites chaos. This is indoctrinated, and should be, into military men from the earliest days of their training. But, unfortunately, this indoctrination almost inevitably leads to silence by a junior in the presence of a senior officer as plans are being made, and in these days of rapid technical change this can be disastrous. For decisions are made by relatively old men, and young men are far more likely to be abreast of altering techniques. Grasp of technological complexities and the courage to speak out at the risk of careers appear in the same individual only rarely. Here I think is the nub of the problem. If the atmosphere of a modern research laboratory were present throughout it, the military structure could fight only feebly. If this atmosphere were completely absent, there would be great danger of not being able to think.

I know that some of our finest military men can resolve this quandary, for I have seen them do it. If the general's hat is on sideways, the field is open to discussion and no holds barred. If it is on straight, it is time for orders and action and no fooling. The high in rank may not actually shift their hats, but their young associates recognize the prevailing attitude nonetheless. Certainly I hope, in these times when tactics are being modified every day, that this duality is becoming an important part of the indoctrination.

But it is at the top echelons where the situation is of major importance, and here the responsibility rests squarely on the shoulders of the Commander in Chief, who passes on the recommendations for appointment of the senior military officers. Does he inquire, in the case of a major appointment, whether the suggested officer is skilled in maintaining this duality? If he doesn't, and if he does not have means for finding out, if he does not insist, he, or his successor, will receive unsound advice and we will be in trouble. But if he does create the sound atmosphere in the top levels, so that the best military minds, of any age or rank, are brought to bear on the major military problems, it will not be long before this occurs as well at every level in the military structure, and we will have a healthy fighting organization.

We had little trouble with tyros during the second great war. Roosevelt had Harry Hopkins as his principal aide, and because of this fact he knew pretty well what was going on everywhere, and there was little opportunity for tyros at the White House level. Harry was anything but cocky, and he was generally liked, by civilians and military alike, except of course by hard-baked conservatives to whom his history of liberalism was anathema. Everyone knew he had enormous power, for F.D.R. trusted him and listened to him. But whenever Harry entered into a situation which was snarled up, it soon became evident that his main object was to get it cleared without F.D.R.'s getting into it, or even hearing about it. Some, I know, will disagree with me on this. But, as for the field of new weapons and military medicine, a field where there was plenty going on which was exciting, Harry never entered or inquired into it, except once, as I have told, when his entry saved me grief. This was not because there were no arguments between civilians in O.S.R.D. and military men. It was because we did not need help to settle them.

Secretary Stimson was, as would be expected, wise in this same regard. His chief assistant was Harvey Bundy.[23] No man could have been more helpful or more careful not to be regarded as personally ambitious. There were other aides, and there were occasional difficulties. But a good part of the fine relations between Stimson and Marshall was due to the restraint which Stimson's aides exercised.

All this is recited for the light it may throw on the problems we now face. I think few will argue that our governmental organization works perfectly. Probably we should not expect it should; it is enormously complex, interfered with politically, with its top echelon continuously changing. We have not developed our Civil Service, as the British have, to the point where the whole system keeps right on running even when the whole Cabinet is absent. Some confusion is inevitable in a democracy; we merely hope we are better managed than Russia or China, and we believe we are. But three points stand out which are here illustrated. We need to be more careful to have unity of control in a theater of operations; consider for example any one of our embassies, or the way we deal with farmers, or how we are now approaching urban renewal. Second, I suspect we still have plenty of tyros gamboling all over the premises, although, being now inactive, I do not see them gambol. Third, we need to do a better job at military planning.

I would like to end this chapter on a different note. Some pages back I cited a chemist as an example of an able man's genius and adaptability. I turn now to one more, a physicist. This is Robert Oppenheimer, and the reason I select him among a thousand will be clear to all his friends. He did a grand job for us, and after the war we treated him shamefully. His was a profoundly complex character, a character, in fact, which could be analyzed only by one who knew him well, and who was equally profound. Certainly many of his fellow citizens failed to understand him. So my comment will be brief. I simply record a poem, which he translated from the Sanskrit, and which he recited to me two nights before Alamogordo:

> In battle, in the forest, at the precipice in the mountains
> On the dark great sea, in the midst of javelins and arrows,
> In sleep, in confusion, in the depths of shame,
> The good deeds a man has done before defend him.

V

Of Inventions and Inventors

"INVENTORS are queer birds who live in attics, defy conventions, and have sudden bright thoughts. The patent system was set up to reward them for inventing. When one of them makes a useful invention, promoters crowd around, most of them intent on taking the invention away from him. A scientist is above such scurvy affairs; when he makes an invention he will dedicate it to the public, and that is all he needs to do. Big companies buy inventions and then suppress them. The patent laws create a monopoly and the antitrust laws then have nothing to do with it. When a court destroys a patent it confers a benefit on the public."

The myths above are some which, unfortunately, surround the whole subject of inventions. In our country we can no longer pioneer by cutting down trees and shooting catamounts. But we need to pioneer by creating new things, new small companies, new ways of competing in international trade. And doing so is not just a matter of making new gadgets to render existence more complex; it includes the creation of new medicines, for example, such as those which have saved my life a few times so that I could write this book, and probably yours so that you could read it.

Industrial progress is essential if the nation is to be strong, and industrial progress in no small measure depends upon the creative ingenuity of individuals who can generate new ideas. The whole history of the United States has been marked by that fact, from the days of Benjamin Franklin or Eli Whitney to our own.

But the nature of inventors and inventions is not well understood. Difficulty is often encountered in bringing an invention into production and use. Industries can get bogged down and stagnate, and so, to ensure that things move, the rise of small independent industrial companies is essential. Our patent system and antitrust laws help in meeting these difficulties. They could do still better if the problem were fully understood, by business, by the courts, by the inventors themselves, perhaps even by you who read.

An invention has some of the characteristics of a poem. Standing alone, by itself, it has no value; that is, no value of a financial sort. This does not mean that inventions—or poems—have no value. It is said that a poet may derive real joy out of making a poem, even if it is never published, even if he does not recite it to his friends, even if it is not a very good poem. No doubt one has to be a poet to understand this. In the same way an inventor can derive real satisfaction out of making an invention, even if he never expects to make a nickel out of it, even if he knows it is a bit foolish, provided he feels it involves ingenuity and insight. An inventor invents because he cannot help it, and also because he gets quiet fun out of doing so. Sometimes he even makes money at it, but not by himself. One has to be an inventor to understand this.

One evening in Dayton I dined alone with Orville Wright.[1] We had been on the way to a dinner which was cancelled. During dinner, and during a long evening, we discussed inventions we had made which had never amounted to anything. He took me up to the attic and showed me models of various weird gadgets. I had plenty of similar efforts to tell him about, and we enjoyed ourselves thoroughly. Neither of us would have thus spilled things except to a fellow practitioner, one who had enjoyed the elation of creation and who knew that such elation is, to a true devotee, independent of practical results. So it is also, I understand, with poets.

And, as with poems, so with inventions; some are made on the spur of the moment. Here follows the story of one such. My wife's Uncle Walter, who lived up in the country, had a rather unusual sense of humor and a very pleasant one. One time when I was on a camping trip, my friend and I dropped in to see Walter. He told us that he had an old gas engine that had not run for years. We dragged it out and looked it over. It had a cylinder that was probably eight inches in diameter and a comparable stroke and a couple of heavy flywheels some three and a half feet in diameter. It was of the old-time engines that they used for sawing wood. We tried to start it and had no luck at all. We had a dry battery for it, but that did not please it any.

It was quite a while before we tumbled to the fact that that engine had been built in the days of highly volatile gas and it did not like our heavier gas. It had no carburetor or anything like that. You just squirted a little something into the cylinder to get it going. So we went downtown and got a bottle of ether, and gave the old-timer a shot. It not only fired; it pretty much jumped off the ground. After that it went very well.

It fired hit or miss. It had a little fly-ball governor, and when it got up to a certain speed, the governor would cut off both the gasoline and the spark. Then the engine would just sit there and go chug-a-chug-a-chug-a for a while. Then it would go bang from another explosion and then it would go chug-a-chug-a-chug-a-chug-a.

This gave us a nice idea. The engine had a vertical exhaust pipe three inches in diameter and perhaps six feet tall. While it was sitting there chugging we got the bright idea of dropping an apple down the exhaust pipe. When the engine next fired, that apple went clear out of sight, whereupon we stood around and covered our heads and waited for it to come back. It came down smack on the ground and that was that.

So we tipped the exhaust pipe over, Walt helping us, took a crowbar to train it, and began to shoot apples at the cows down in the field. We would shoot an apple, it would go over a cow's back, and we would take the crowbar and tap the exhaust pipe down a little and take another shot. We finally hit a cow on the rump, and the cow went galloping across the field. This was mild sport for anyone who enjoys shooting cows, and in its way here was a spontaneous rudimentary invention. It was no more impractical than other types of dodads that have been invented and patented by wild-eyed dreamers. Insofar as it was a new and useful combination of elements, it was a patentable invention. Note "new and useful combination"; a bare idea is not enough, as the following story suggests.

One time when I was a young professor at M.I.T. and was also a consulting engineer, I was called in by a chap who ran a considerable business, who told me that he had made a very great invention and wanted me to develop it for him. He had invented a watch which would run without winding; it was to have a little radio receiver in it which would pick up energy from all the broadcasting stations, and this energy would wind the watch. All that I was supposed to do was to take that idea and develop it. I said very little to him about the amount of energy thus receivable and the size of radio sets and so forth. I merely said that I would think it over, and of course I never went near him again. That chap had the concept that the mere formulating of his idea constituted an invention. He is not alone in this misconception.

The law says that a combination of elements which is new and useful is an invention and is patentable. Its use may be utterly trivial. For instance, I made a birdhouse that the pigeons could not enter and eat up all the grub, simply by having the perches supported on springs; when a little bird landed, he could step in and get his food, but if a pigeon landed, the perch dumped him back end over end. This was probably patentable at the time, although of course I didn't try to find out. I have made many such gadgets for the joy of making them. A lot of invention appears merely because of curiosity and with little thought at the time of possible utility. I have made a number that I never followed up, and some of them *could* have been followed up, I think. But the fun was in exploring, not in the hazardous and somewhat repelling job of trying to promote them.

Once I worked out a solar-powered pumping rig for irrigation. I still think it would work. I did some work on an air-conditioning scheme that I think I could have put into commercial form. Here is one on which I am rather sure a small business could have been set up. I do not know whether I invented this, or Warren Mead [2] did; it came out of one of our many conversations while we were playing billiards. He used to trim me easily until he lost an eye. After that we played even.

Nurserymen in this part of the country can transplant shrubs only in the spring and fall, but the demand continues in the summer, especially at summer homes. Take an open-ended tin can of proper size, or a section of stovepipe. In the nursery, when shrubs are being transplanted, as they are to get correct growth, put each of them into one of these cans, which is then buried with its top at ground level. To move the shrub, pick a dry time. One can then lift the can, and the shrub and earth will come with it, without disturbing roots at all. Truck it to the place it is to grow. Plant it, can and all. Water thoroughly. The can will now glide out without disturbing the earth. Much better than ball and burlap, and less expensive. Probably someone has patented this; I never looked it up. I have often been approached for advice by a starry-eyed inventor with a world-beating idea. Usually the best advice is to make a patent search. The more or less obvious ideas have been patented dozens of times in various forms.

I think any man who has a bit of ingenuity in his soul gets quite a lot of fun out of fooling around with things that do not amount to anything, for the reason that when he does so, he has no pressures. If he is working on a subject that is really important, then he is under pressure. He has to get the thing done; he has to get it working right. This was true, for example, when I worked on a new line of hydraulic devices to operate at high pressure and high speed. I very much wanted this to succeed, and I worked hard on it, for one reason because a lot of good engineers had been working with me on it at one time or another, and I did not want to let them down. Even if I was never to make a nickel out of it, I would go into it intensely because of the associations.

I will recite this in a bit of detail, for it illustrates an interesting roadblock in the way of inventions coming into use. Hydraulic pumps and motors have been built in a variety of forms for a hundred years or more. All of them have metal-to-metal contact between moving parts. In all of them this reduces efficiency and causes wear when speeds and pressures are high. It struck me that, if this disability could be removed, there would be some important uses for a long-lived, efficient device. So, after a long series of trials, and working with some keen engineers, I finally came up with a form in which there was no contact whatever between moving parts under operating conditions. We fully proved this. We disassembled one of the units and painted all the moving parts with mechanic's dye, a thin layer of paint. Then we put it together and ran it for one hundred hours under full load. When it was again disassembled, we found that at no point had the dye been rubbed off. The configuration was such that the parts were always securely separated by oil films. This, of course, helped efficiency, but, more important, it indicated that the device would not wear out. One unit we made operated nicely at 5000 pounds per square inch and 5000 revolutions per minute. At such pressures and speeds, units become small in size for a given horsepower output, which is important for some applications.

Now I thought there would be lots of uses for such a device. In particular, I thought one could use it as an excellent transmission for a truck. One would simply put a hydraulic pump on the engine and a hydraulic motor on each drive wheel. In doing so one would get rid of transmission, torque tube, and differential. In fact, on a four-wheel drive truck, one would get rid of three differentials, which are expensive affairs. A very smooth braking system would be available, simply by throttling the hydraulic motors, and this could readily be arranged to help avoid jackknifing of trailer trucks. One of the most important advantages seemed to me to be that such an arrangement would take much of the burden of shifting gears off the driver, for the hydraulic pump would be of variable delivery and this fact alone would give a wide range of speeds. A heavy truck today will have as many as sixteen forward speeds, and in hilly country the driver is continually shifting gears, which is not good for the machinery and not good for the driver.

Did the makers of trucks and off-the-road vehicles go for it? They did not, at least not yet. They seem to be quite happy as they are. There are other uses, but most of these do not call for long life or small size. Or they are uses where the volume of sales is too small to warrant cost of changeover.

Now I am not unduly disturbed. I had my fun, and I proved I was right—technically. But it left me wondering what was wrong with my original thinking on the matter. I believe it is this: In an industry that has become closely standardized, where nearly all competing companies are comfortably making profits, minor improvements can readily be introduced, but major improvements are up against a stone wall. The prosperous company sees no need to change, the borderline company cannot afford the cost. If the product of the industry is diversified, and of such nature that development or changeover costs are small, as in the scientific instrument industry, for example, a new device will soon be put into use, if necessary by a brand new company entering the field. If the industry produces essentially only one product, and that fully standardized, the hazards of change are just too great, and nothing is likely to happen. It is also true that in the young industry there are usually plenty of courageous pioneers, and the old industry tends to go to sleep.

I started learning about inventions, their development and introduction into practice, a long time ago, as I will relate below. I am still learning, and I may learn more in the case of hydraulic machines, for the patents are not yet expired. I think it is probable, however, that some historian of technology many years from now will just recite that I made an improvement in hydraulics, theoretically interesting, but practically unimportant.

All this boils down to the fact that inventions are a dime a dozen. Any reasonably bright chap can make a patentable invention every week if he puts his mind to it, and if he then attempts to patent all of them he can go broke. This is because the government, in its wisdom, will issue a patent on any combination which is new and which conceivably has a use, without the slightest regard for whether it makes practical sense. The government cannot very well do otherwise. The point is that an invention is valueless unless it is joined with a number of other accomplishments—promotion, financing, development, engineering, marketing, and so on. If the independent inventor believes otherwise, his invention may well turn out to be a liability.

Fortunately for me, I learned some of these lessons early, and doing so saved me a lot of grief later. I learned by coming a cropper. I made an invention and it cost me money, when money was a very scarce article. It was not at all a bad invention at that.

It came about when I was still in college. I invented a surveying machine. The patent issued December 31, 1912, was number 1,048,649, and expired some forty years ago. The model I built in the college shop is still in existence. It was rather crudely built—I was not much of a mechanic in those days—but it worked; in fact it worked rather well.

It consisted of an instrument box slung between two small bicycle wheels. The surveyor pushed it over a road, or across a field, and it automatically drew the profile as it went. It was sensitive and fairly accurate. If, going down a road, it ran over a manhole cover, it would duly plot the little bump. If one ran it around a field and came back to the starting point, it would show within a few inches the same elevation as that at which it had started.

The box contained a well-damped pendulum. On this was mounted a disc, driven from the rear wheel. Against this disc rested two sharp-edged rollers. One picked off the vertical distance traveled, and moved a pen. The other picked off the horizontal distance and turned a drum carrying the paper. This much constituted an integrator, and I was to have quite a lot to do with integrators in later years.

For those who are not familiar with integrators, perhaps a bit more explanation is needed. The disc was mounted on the pendulum and was rotated, as the bicycle wheels revolved, by a train of gears. Two of these gears had their contact in the line between the knife edges on which the pendulum swung, so that the resistance to turning of the disc did not tend to deflect the pendulum. Against the face of the disc rested the two rollers, mounted on needle bearings in frames with light springs to apply pressure. One of these rested on the center of the disc, when the machine was level and the pendulum central, and hence did not turn at all as the device moved along a level road. When the pendulum was deflected it rotated an amount proportional to the amount the whole device moved vertically, as a little simple trigonometry shows. It did not have much work to do. Its shaft was threaded, and carried a half nut on which a pen was mounted. As the machine was rolled along, this pen recorded the vertical height moved. The other roller rested on the lower part of the disc, its point of contact on the line of the knife edges, and turned at full speed when the pendulum was central. When the pendulum was deflected (more trigonometry) the roller turned at a rate proportional to the horizontal speed of the machine. Thus, on a hill, it recorded horizontal distance moved, not just distance along the ground. That roller had a lot of work to do, for it had to turn the drum which carried the recording paper, and it could not do so without help.

The device also contained a servomechanism, although that term had not then become adopted. This involved a gear train that kept a spring wound up, and the spring provided the force needed to turn the drum, leaving the roller with no job except to guide the drum. It is remarkable that the device worked as well as it did, for I remember that in that gear train there was a worm gear that ran at least a thirty-second of an inch out of true. Servomechanisms have proliferated greatly since those early days. There are now many textbooks and college courses on the subject. This was a simple one compared to many now used in all sorts of intricate devices.

I thought my surveying machine was quite a gadget. So, fortunately, did my professors, for they authorized a degree for me on the basis of it. (The next degree I took required much more effort.) I also thought the machine had real practical use. I judged that a company designing a new road could save quite a lot of money by using it to make the necessary surveys. I still think so. So I spent some of my money to apply for a patent, money earned incidentally by tutoring, which I think was somewhat in the nature of a racket, and probably still is. A patent duly issued. I wrote to a lot of people in companies that I thought could use the machine or make it to advantage. Then nothing happened. And after a while I woke up.

The trouble was that back in 1913 I was densely ignorant. I knew a bit of physics and mathematics. I had graduated in engineering. But I was not an engineer. An engineer has to know a lot about people, the ways they organize and work together, or against one another, the ways in which business makes a profit or fails to, especially about how new things become conceived, analyzed, developed, manufactured, put into use. So I charged that invention off to experience—and with no regrets. After all, I got a degree out of it, I had a lot of fun with it, I learned something, and I reoriented my thinking. In fact, for the first time I resolved to become a real engineer. I resolved to learn about men as well as about things.

For many years I heard of no such machine coming into use. Then, just before the invasion of Normandy, I learned that men slipped ashore at night, right under the noses of the Nazi troops, and surveyed the invasion beaches. They made profiles by using a device which was probably very much like my old one. This is not at all surprising. Any fairly bright chap, knowing a bit of mechanics, and told the need for such a device, would readily invent it. Even so, I believe roads are still surveyed by methods essentially a century old.

I made a number of inventions after that early surveying machine, nearly always in association with others and incidental to other efforts. One result, as an outgrowth of this activity, was that I was called, long afterward, before the Temporary National Economic Committee of the Congress, the so-called Monopoly Committee, of which Senator Joseph C. O'Mahoney [3] of Wyoming was chairman.

There was a bit of a battle on.* I went into it under rather strange circumstances. The committee had given the Commissioner of Patents, Conway Coe, a couple of days without interruption to explain the patent system. Coe asked me to go on as his first witness when he got through. I had small doubt what was likely to happen to me under those circumstances, for the members of the committee had agreed to keep quiet while Coe was presenting, and when suddenly released, they were bound to land heavily on the next witness. By then, I didn't mind that sort of thing particularly, and sometimes enjoyed it, so I said to Coe that I would go on. Things happened in a hurry. After considerable discussion of research, organization for research, and related matters, they got down to cases, and they began to ask me about the beginnings of Raytheon, and the Spencer Thermostat Company which my college roommate, Laurence K. Marshall, and a little group and I had set up in the early days and of which I shall have some stories to tell later in this chapter. One of the committee members asked me, in so many words, whether we could do the same sort of thing today, and I said "No." He said, "Why not?" and I said substantially, "Because you people sitting on the other side of the table have made it nearly impossible." When they asked how, I told them that in the old days we would have one girl in the office and ten men in the shop, but that as it stood when I testified we would have ten girls in the office filling out forms. I made that remark about nine-thirty in the morning and I did not get off the stand until five o'clock that night—but I certainly got the attention and interest of the committee. They wanted to know if I held any U.S. Patents and I answered, "About twenty or thirty." Had any device I invented ever got into litigation? I had to disappoint them there, for I do not know to this day of a case when an invention of mine did. Did I ever make any money on these patents of mine? The chairman interposed,

* The formal record is in *Investigation of Concentration of Economic Power: Hearings before the Temporary National Economic Committee, Congress of the United States: Part 3, Patents: Proposals for Change in Law and Procedures*, pp. 870–911, Washington, Government Printing Office, 1939. Some of the more informal exchanges, as is usual, appear to have disappeared during the consolidating of that text.

"You do not need to answer that question, Doctor." I said, "Why, Mr. Chairman, I would like to answer it. The answer is no."

That answer calls for a bit of explanation. Literally it was correct; in fact it still is. I had never made any direct income out of a patent of mine. But indirectly is another matter, as they well understood.

I was early a professor and also a consultant to industry. It was a salutary combination; it drew me out of the ivory tower and put life into my teaching, and it greatly helped the family budget. As a consultant I assigned any invention I made to the company whose interests had sparked it. Later, when I became an administrator in academic organization, I habitually assigned to the academic organization anything I might invent. So I assigned some things to the Massachusetts Institute of Technology, and M.I.T. never made any money out of those. I think perhaps one or two might have become worth something if the war had not interrupted, but, as things were, they never did. I assigned several to the Carnegie Institution of Washington. One of these was an idea that I think might be useful. Unfortunately the patent that was issued on it was decidedly limited—so limited that I do not think an industrial concern would really be justified in going ahead on the basis of that patent. Only since I retired have I held possession of patents personally. Like all inventors, I have bright hopes for some of these, and, as with all inventors, these hopes do not become dimmed merely because the world seems to disagree with me. But the early resolve not to try to exploit inventions myself saved me from frittering away effort that was far better devoted to other things that, it seemed to me, were much more worthwhile.

There is an interesting point here. I have long been deeply involved in patent matters, serving on a couple of committees, and under a Presidential appointment, that were supposed to recommend modernization of the system. I have testified before committees of Congress and have known the Commissioners of Patents and worked closely with them. During the war I had a patent organization in the O.S.R.D., as I have noted elsewhere in another connection, for we had to take care of the situation that might have arisen after the war if Tom, Dick, and Harry all over the country had filed patents on things that came out of the war research program. We had to protect the public interest. Considering this connection with the Patent Office, all the patent examiners knew, of course, that I had been deeply involved and had been close to Commissioners of Patents. Hence when the examiners saw a patent application with my name on it, they leaned over backward to be sure that they showed no favoritism. This is exactly correct; but they certainly got rough at times; once was on the device I assigned to the Carnegie Institution of Washington. I never blamed them a bit. In fact I should be much disturbed if I found any other attitude among the examiners. Actually, during the fifty years or so that I have been in touch with the Patent Office in one way or another, I have never seen an instance where there was the slightest question as to the integrity of the patent examiner. They have a fine reputation which they have fully earned.

There is another aspect to this whole strange subject of inventions. Scientists and engineers, in their laboratories, make inventions frequently, almost daily. These are genuine inventions that could result in valid patents if vigorously pursued. The inventors usually do nothing whatever about them, except to write about them when they publish the results of their research. These inventions are merely incidents in the efforts to succeed in the research, ingenious ways of circumventing difficulties in using complex apparatus. There are a number of reasons why scientists thus generally ignore the patent system. First and foremost, they do not want to be bothered with it. Second, they know that most of these things will be of use to only a few fellow scientists thus engaged. Finally, there is a professional feeling that something is amiss when search for new understanding for the good of society is combined with an attempt to make a personal bonanza out of it.

This applies primarily to basic research, of course. Research in industrial laboratories is usually combined with a serious effort to build a patent structure to bolster the company's efforts. Sometimes a man on applied research in a university makes an invention, steps out and forms a company, markets the invention, and makes a success. There is nothing wrong with this, provided that the university's equity in the matter is properly recognized and that the presence of such activity does not injure the morale of other researchers. In fact, the setting up of an independent vigorous young company, in the midst of our competitive industrial system, right in the backyard of large powerful industrial units, confers a real benefit on the public, although the public may not sense it.

As an example of the sort of invention that comes out of basic research, the early history of analytical machines is interesting. I became tangled up early in this affair. My main effort was on the differential analyzer, which could mechanically solve differential equations. The reason for this activity was simple enough. I was trying to solve some of the problems of electric circuitry, such as the ones connected with failures and blackouts in power networks, and I was thoroughly stuck because I could not solve the tough equations the investigation led to. Ralph Booth [4] and I managed to solve one problem, on the stability of a proposed transmission line, but solving it took months of making and manipulating charts and graphs. Incidentally, the study showed that the line would be unstable, and this result caused quite a commotion, for the line had been designed by the engineers of the great electrical manufacturing companies. But better ways of analyzing were certainly needed. So some young chaps and I made a machine to do the work. We did not want to patent it; we did not want to be bothered, and moreover we felt sure it would not come into commercial use for many years. We were right on that; modernized forms of such machines, far more effective, are now made and sold commercially, long after any of our possible patents would have expired.

There is another point to be noted here: Since those days the powerful digital machines have appeared and have relegated the analogue machines, of which the differential analyzer was the first one, to a secondary position. These new digital machines are powerful indeed, as everyone in research or business is bound to realize. They can do in a few minutes what Booth and I struggled over for months, and do it a lot better.

In the meantime power systems have grown enormously and have become interconnected over vast regions. And we have had two severe blackouts and are undoubtedly headed for more. The most forward-looking leaders in our electric power companies are very much worried about this subject. We had no real catastrophe during the great blackout which shut down most of the eastern part of the country; it was a nice mild moonlit night, and there was no panic. But a repetition could kill thousands and injure a million. So some of the power executives are trying to get together, put computers to work, find out where the dangers lie, and take precautions. They are accomplishing something along these lines, but not nearly enough. And they are blocked by a strange element in our system of government.

We have queer ideas about government. We set up a monopoly where it is needed—for example, in electric power, where it would certainly be foolish to have two competing electrical power supply systems covering the same area. Then we set up a commission to regulate that monopoly, to fix its prices, and thus to protect the public. So far makes sense. But we miss out on two points. We have a patchwork of little companies where what are needed are great companies covering great areas, to obtain the benefits of interconnection—and to avoid blackouts. Then our system of regulation is largely negative; it holds a lid on prices but provides little incentive or reward for outstanding performance. We even have a Department of Justice, charged with enhancing competition by use of the antitrust laws, occasionally barging into the area where common sense calls for regulated monopoly rather than competition. One reason the power companies do not analyze the whole power network, and thus protect us against disastrous blackouts, is that they fear to collaborate fully, under the hazard of such attacks. A natural monopoly attacked because it is a monopoly! It is about time the American people, and hence the Congress, got it through their heads that we have two systems of industry—active competition and regulated monopoly. Both are necessary and beneficial. But they can't be mixed. If the regulatory commission doesn't use sense, the cure is to get a new commission, not to circumvent the old one. The day is nearly, but not quite, over when attacks on simple bigness draw applause from the public. We need just a few great power companies covering the entire country. And we have to encourage them to show initiative and courage in attacking our problems. Apparently we are still immature in this country. And if we get more and worse blackouts it will be because we have failed to see where our true interest lies.

Any discussion of inventions has its negative aspects, but let us examine the positive side; how a new idea actually comes into use, with all that that means in terms of initial organization, subsequent ups and downs, growth, development, and ultimate evolution. I witnessed some examples of this when I was still young, a bit naïve, and to a considerable extent bewildered.

In college my roommate was Laurence K. Marshall, of whom I have spoken previously. His middle name was Kennedy, but he was not of the well-known tribe. He was an extraordinary individual with a keen mind and an exceedingly attractive spirit of adventure. We took courses together in subjects which were then considered bizarre and unrealistic for undergraduate engineers. For example, we pursued one on theoretical mechanics, under Professor William R. Ransom,[5] in which we two were the only students. I think I was probably one up on Marshall in regard to the mathematics involved. But he was certainly several jumps ahead of me in understanding the kind of world we proposed to enter and challenge.

Not long after we graduated the first world war came along. I was working on devices for detecting submarines; Marshall was in the Army. There he developed friendships with a small group of men from around Boston, men who had personal funds of some consequence, and, more important, had a bit of nerve and courted adventure. They were the antithesis of the four percenters of the banking world who once gave Boston its sad reputation as a place where pioneering was in decline; these men were worthy descendants of the business stalwarts who sent the clipper ships to China and the Gold Coast of California.

Not long after the war I was consulting for the American Research and Development Corporation, AMRAD for short. It was a venture of J. P. Morgan, founded and run by a chap who had been the wireless operator on Morgan's yacht. It made what we now call radio apparatus, among other things sets for amateurs. In this connection it broadcast programs so the amateurs would have something to listen to. In fact I have always thought it was the first to do so, although KDKA, the Westinghouse station, claims that somewhat dubious honor. It was hence very early established in a rapidly growing field, with fine opportunities before it. Unfortunately its founder did not know much about business management; there was no reason to expect he would. More unfortunately, he thought he did. I operated his research laboratories, but on business and financial problems I certainly was not much help to him, although I tried to be. One of AMRAD's mechanics was John A. Spencer, known as Al. He had grown up in the Maine woods, his father had been killed in a mill accident when Al was four, and he had been brought up by an uncle, working in spool mills and with scant schooling. He got down to the city and made a machinist out of himself without going through an apprenticeship. He was one of the most resourceful and ingenious men I ever knew, and he had a sense of humor which Will Rogers would have cherished.

One day he told me about a little thing that he had built, incidentally on his own time. It was just a thermostatic sheet which he had dished a little bit by tapping it on an anvil with a hammer, and which would click through like the bottom of an oilcan. Of course, being made of thermostatic metal, it would snap through at a certain temperature. To make thermostatic metal one welds, or silver solders together, two blocks of metal, of different expansion coefficients, and then rolls them down to a sheet of the desired thickness, annealing occasionally as the rolling is done. Strips of such metal were then much used, for they would curl up as the temperature changed. The dished sheet which would click on a change of temperature was utterly new. Really striking inventions are sometimes just as simple as that. It did not take much of an eye to see that here was the basis of a positive, powerful, inexpensive, thermostatic electric switch for all sorts of purposes.

I told Al I thought he had a very interesting thing and that I would look into it a bit. So I went to the patent attorneys of AMRAD, stated a hypothetical case, and asked whether, in such circumstances, the company had any equity in the invention. They assured me that it did not, after which I felt perfectly free to tell Marshall and his crowd that Spencer had made what seemed to me to be a very interesting invention.

A little episode followed that nearly wrecked things. Spencer, who had no business experience whatever, came pretty near selling the thing out for a song while Marshall and his group were organizing to do something about it. When Al told me a friend had made him an offer for the invention, I stopped him from accepting; in fact, I told him if he wanted to sell it I would give him twice what he had been offered. So he did not sell and soon Marshall's group made him an offer. He protested that he had already sold it to me, but I assured him he had misunderstood. So they founded a new company, gave him a good husky stock interest in it, and put him on a salary. This was the beginning of the Spencer Thermostat Company.

Here I want to set down one story that I cannot vouch for, that I could not prove, but that I think is true. At one time Spencer Thermostat Company was in difficulty—remember that all of these little companies got into difficulties at various times, could hardly meet their payrolls and so on—and Al Spencer insisted he was going to sell out his stock. Whereupon some of the Boston group bought his stock and then, when the trouble was over and the company was back on its feet, they sold it back to him at the same price. I cannot vouch for this, but I think it is true. If it is, it is one of the most decent ways in which an inventor was ever handled.

I became a consultant to the Spencer Thermostat Company and pretty much operated the laboratories. Hence it came about that L. K. Marshall and I made an excursion to New York soon after the Spencer thermostat was developed and we were trying to get some customers. We went down to make a demonstration of the device in a flatiron. We had only one model and we went down on the Fall River Line. Next morning, in the hotel before the meeting, we thought we had better try the device out to be sure it was operating well. We forgot that the hotel was in the direct-current area, and the thermostat would operate only on alternating current. So when we plugged it in, we promptly burned up the thermostat. Whereupon we made our apologies and took the train back to Boston.

Marshall made an important supplementary invention later, in which he used two metals which had widely different coefficients at high operating temperatures, but nearly identical coefficients at room temperature. Thus the device would not become overstressed in arriving at the operating temperature. This was the first thermostat that would operate a flatiron successfully, and it obviously had a very good market. There were about four or five flatiron manufacturers, and they had all been associated under the nichrome patents, that is, the patents on the alloy which was by far the best one out of which to make a flatiron heater element. The nichrome patents had expired, but the companies still kept up their relationship sub rosa. We approached all of them, or most of them, with the idea of getting them to manufacture the Spencer type of iron on a royalty basis. We got nowhere. It was quite a while before we awoke to the fact they probably had just agreed that no one of them would take the thing exclusively, that the one to pick it up from us would license the others. This situation meant that we had no customer, because no one of them could afford to pick it up, pay the cost of taking it over and doing the expensive job of engineering for production, and then hand it over to all of his competitors on nearly the same basis on which he would be manufacturing it.

We thought that was the case. Of course we could not prove anything. But we counterattacked in various ways and certainly did not deny rumors that J. P. Morgan was backing the Spencer Company and that we were going into manufacture. After a while they signed up with us; they manufactured the irons and paid us royalties. As soon as they had made the contract they released a publicity statement which implied that they had paid a million dollars for the patent. They had not paid anything by that time, but the story resulted in the myth of the million-dollar thermostat. Before they got through paying us royalties they paid us a good deal more than a million dollars. That put the Spencer Thermostat Company on its feet and it did quite well thereafter. After a while it merged with a group in Providence, the General Plate Company, who made gold-plate jewelry. The merger formed the firm known as Metals and Controls.

The man who handled affairs down at General Plate was Russell Grinnell.[6] When he presided at a meeting, he sat up very straight in the chair and presided with great formality. He was a good old New England businessmen. Two interesting things happened at one of those meetings. When we entered into the merger agreement we had an issue of preferred stock in the combined company. The Providence group picked up their share, but the Spencer outfit did not have enough money in the treasury to pick up theirs. So the Spencer stock was put in escrow and our obligation on the preferred stock was to be retired as we got dividends.

This would have been fine except that the great depression came along, and in the middle of the depression things came to a head. We had a meeting. The situation then was such that if Russell Grinnell merely sat tight he could take over the Spencer interest because the Spencer group could not meet their obligations. The stockholders of the group were disinclined, under the pressure they were all under at that time, to put up any more money to bail themselves out.

So we met. Russell Grinnell opened the meeting and said, "Gentlemen, we have an agreement between the Cambridge group and the group in Providence. When we entered into it we felt, all of us, that it was an entirely fair and equitable agreement. As things stand today none of us would enter into such an agreement; we would not think it was a reasonable one. I think therefore that we ought to discuss this matter as though the agreement was now being entered into, as though it did not exist." In other words, having the Spencer Company completely in his hands, he said, "I will not take it over by taking an unfair advantage of my partners in an enterprise." That is the old New England businessman at his best.

The voting power was equally divided at that time between the Providence group and the group in Boston. When Grinnell made his speech and after we had worked out a plan that took the Spencer group off the hook, I spoke up and said, "Mr. Grinnell, the voting power in this company has been equally divided up to the moment. I merely wish to assure you that from now on the voting power of the stock that I hold personally is entirely in your hands." In other words, he had said, "I will not take over the company by reason of a misfortune that none of us could have foreseen," and I replied by saying, "Very well, as another gesture of this sort you now control the company." So he did, and matters went on from there with the General Plate group, or rather Grinnell himself, in control. A good many years later Metals and Controls was merged with Texas Instruments. At the present time I understand that the Metals and Controls Division is the most profitable division of Texas Instruments.

Marshall and his group, with my somewhat immature and erratic technical advice, set up perhaps six or eight small companies formed around new ideas. Two of them survived, which is not a bad batting average. No one got hurt, for the same general group backed all of them, and the two successful ones did very well. It is worthwhile to tell about one more.

At AMRAD I hired a young physicist from Texas who was named C. G. Smith [7] and was always called C.G. The way I hired him is interesting. An interview of that sort is always likely to be on an artificial basis and somewhat embarrassing. So I discussed with him a technical point on which I was then genuinely puzzled. The next day he came in with a neat solution and I hired him at once. He proceeded to bat up all sorts of ingenious ideas, one of which was right on the button. Radio sets in those days were run on batteries, A batteries for the filaments and B batteries for the plate supply of the thermionic tubes used. C.G. came up with an idea for a gaseous rectifier which would eliminate the B battery and allow the set to get its relatively high-voltage direct current from the power line.

In this C.G. was a jump ahead of the engineers of General Electric and Westinghouse, who were undoubtedly trying to do the same thing. So we worked vigorously to remove the gaseous rectifier's faults and develop it for production. We were in this process when the company proceeded to go broke. It was notable that the head of the company—Morgan's onetime wireless operator—did not put me in touch with the source of funds, where I might possibly have been convincing. He performed this function himself, and was not. As the company cracked I had to stay around and try to protect as far as I could the young chaps I had drawn about me. But when crack-up became inevitable I told Marshall that there was something well worth salvage here. The result, after many years and many vicissitudes, was the Raytheon Company, now the largest employer in New England; Marshall became its president in 1922, and subsequently chairman of the Board. At the end of this chapter, and for a specific purpose, I shall come back to Raytheon and C.G.'s invention. But before that there are a few histories of inventions to be recounted.

When AMRAD folded, Mr. Morgan, whom incidentally I never met, took over the only assets, namely the patents. Marshall, who had formed his company, went to New York and bought them. The conversation went something like this: "I understand you have some AMRAD patents and I want to buy them." "What will you give me for them?" "Fifty thousand dollars." "Cash?" "Yes, cash." "All right, they are yours."

But a week or two later Morgan's curiosity was aroused and he called Marshall down again. The result was that Marshall sold him fifty thousand dollars' worth of stock in the new company. Many years later, when Henry and Junius Morgan [8] were good friends of mine, they still had that stock, which had appreciated in value to a considerable extent.

My association with AMRAD brought me into activity in connection with another invention, and my experience there taught me a lot about inventors and one kind of hazard they face. It seems that a German mechanic had invented a new kind of gyroscopic compass, and Mr. Morgan had invested in it. I was asked to look into it, presumably to advise whether more investment was warranted. I did just that. I found out that the invention was indeed exceedingly clever, and it seemed to me the device would be useful and commercially attractive in the right hands. But I also found out, and accordingly reported, that there had been a scandalous series of juggling acts, the formation of new companies, transfer of assets, sale of stock no doubt under false representation, and so on, so that in fact Mr. Morgan no longer owned any equity in the invention. The inventor unquestionably never got a dime, the public never got the benefit of the invention, and Mr. Morgan undoubtedly charged the whole thing off to experience. The patents are now of course long expired, and anyone could make the device and sell it. But no one will, for if he did, competitors could at once copy it, free from the large costs of development.

The device is well worth discussing for a number of reasons. The inventor spoke no English, and my German was not extensive, so I had some trouble arriving at a full understanding. Fortunately I had studied the mathematics of the gyroscope, and that helped. And when I got to the bottom of the matter, I discovered a remarkable thing. The inventor had produced a highly ingenious idea. He had made a model and it operated as predicted. But he did not himself really understand how his own device worked.

Gyroscopic compasses are used everywhere to guide ships. They are expensive, but are much better than magnetic compasses, for they point to the true north, not to the magnetic pole which is far removed from the North Pole, and they are not subject to the many corrections or aberrations of the magnetic compass.

We are all familiar with the top which consists of a little flywheel on a frame, spun by use of a string. It will balance itself upright like any top. But, if it is mounted in gimbals, that is, on a set of rings and bearings, so that it is free to turn in any way whatever, but has no tendency to turn at all by reason of gravity, when the wheel is rotating it will maintain a fixed direction in space. In other words, if friction is well suppressed and balance is well enough achieved, the gyro will point its axis at some star and continue thus to point as the earth turns under it. This is the basis on which inertial guidance systems are built. The gyroscopes now used are marvels of mechanical precision. Inertial guidance systems are so precise in their action that a submarine can navigate for months under the polar ice and know at all times just where it is. They can be installed in a missile that is fired from a submarine while still submerged, and that will then guide itself unerringly to a distant target. It is the heart of the system that guides an Apollo missile to the moon and back. I know of no technical development for which I have more keen respect than this, and where I have more admiration for the men who accomplished it, especially for Stark Draper,[9] who was the prime mover in the whole extraordinary affair, and for some other remarkable engineers who overcame obstacles that I personally thought were insurmountable.

Long ago, in the early days of the century, it was found that, with proper added constraints, a gyroscope of this sort could be made to point north, and the gyroscopic compass resulted. The gyro was mounted as before, in gimbals, so that in the absence of applied forces its axis would remain fixed in space, that is, pointing invariantly at some star. But as the earth turned, forces were brought to bear which caused it to swing toward the north or, to be precise, to precess. One more thing was needed, an added force to damp the precession, that is, to cause the compass's swings back and forth across a north-south line gradually to decrease in amplitude until it settled down and pointed north unvaryingly.

The gyroscope used in these compasses is powerful. A rather heavy flywheel, running in a vacuum, is driven by an electric motor. If the motor is cut off, the flywheel will continue to run at high speed for hours. We all remember playing with the little toy gyroscope, how an attempt to twist it one way would encounter resistance, and it would insist on moving in another direction. This effect, the precession under any applied torque, was powerful indeed in these compass gyros. I remember an open house at M.I.T. in which we brought one up to speed, put it in a suitcase, and set the suitcase down in the middle of the floor. Some visitor was bound to pick it up, whereupon he could not put it down. As he hung onto the handle it would squirm all over the lot, above his head, behind his back, until he was rescued. Then one of us chaps would make it behave beautifully by urging it, apparently in the wrong direction, to make it turn as he wished.

Now the electrically driven gyro compass, with all its appendages, was complex. By comparison the German inventor's device was simple. He had a cup, spherical inside, about two inches in diameter.

Into this he dropped a steel ball which fitted the cup loosely. Through a slit in the bottom of the cup was forced a jet of compressed air. This jet did several things. It lifted the ball a little so that it was no longer in contact with the cup. It made it spin at 30,000 revolutions per minute. The cup was mounted on the compass card, and this was also supported on a compressed air bearing. As the ball precessed, the reaction of the jet caused the card to follow it. Moreover, there was a component of the reaction which damped the precession. With this composite reaction of a single jet, it was no wonder the inventor could not understand his own child. I remember that I asked him what he proposed to do about correction for the ship's speed, for a gyro compass pays attention not only to the earth's rotation, but also to the movement of the ship over the surface of the earth, and conventional gyro compasses have a mechanism for introducing the necessary correction. He replied that his compass required no such correction. Too bad the invention died for lack of proper attention. It would have provided a relatively simple, inexpensive compass for ships that could not afford an expensive one.

Recalling this baffled German inventor and his inability to achieve commercial success from what was a really brilliant invention leads me to comment a bit on one or two other inventors.

Edison was a very good inventor, a still better promoter, but in some ways a poor experimenter. Some of his experimentation was crude, to say the least. When we talk about the Edisonian method, which means to try everything without any theory to guide you, just hit or miss, we are talking about very poor experimentation. But Edison was such a good promoter that he could advance even with poor experimental data. He had good management, through people that he tied in by his promotion. Moreover, he had the facility of seeing where there was a public demand for something. The combination is what made Edison. He had other qualities which made him succeed in spite of the rather clumsy way that he went about his laboratory work, as the history of what he did on the filament for the electric lamp demonstrates.

As an experimenter, as an empiricist, Edison left something to be desired. This was certainly offset by the fact that he had all the other qualities, and his promotional ability was illustrated beautifully by the way he organized electric light companies.

A great change is occurring in the way technical innovations come about. The day of the lone inventor is not quite over, but it has been vastly changed. We have seen how a lone inventor can have a bright idea that people could grab and run with—the Spencer snapping disc was one of these. But in general, advances in technology are being made today by teams of individuals in laboratories where it is not so much a spark of ingenuity that counts as it is a knowledge of physics and mechanics and painstaking work in development.

There are two main ways of going about inventing. One is to see a public need, or desire, and scurry about to find a way of meeting it. The other is to develop new knowledge and see where it leads. The first method was distinctly Edison's. Today the second is the one most commonly used. Of course, the new knowledge must be directed into channels where there may be useful results.

For example, compare the development of the transistor with the way De Forest's [10] three-element thermionic tube came into being. After Langmuir had produced high vacuum in such tubes and hence created a thermionic tube of real technical merit, it appeared that an end point had been reached. But a group of men in the Bell Telephone Laboratories, studying solid state physics, developing abstruse theories of the flow of electrons through semi-conductors and the flow of holes in the opposite direction (a bizarre idea in itself), erected a whole new field of physics in which they developed all sorts of interesting relationships. It soon became inevitable that the transistor, so-called, could be made into a control device. So the whole art of transistors evolved out of a great teamwork operation. Incidentally, since the development occurred in the Bell Laboratories, it was licensed freely and became available to the whole electronic industry. It was not hard to get the transistor into manufacture, and it promptly came into very wide use.

The rise of team development notwithstanding, many people have queer ideas as to how inventions occur, and they usually oversimplify: Bell invented the telephone, Edison the electric light, Marconi wireless telegraphy, and so on. In each of these cases the facts are of course quite different. The inventors, and the men who made the invention possible, were numerous and in most cases the public knows nothing about them. My father told me that when he was in college—and he graduated in 1879—Professor Dolbear [11] had a telephone working between two of the buildings. It was an induction affair and utterly impractical, for it would operate over only a very short distance, but it was a telephone nevertheless. There were others too.

Now this should by no means reduce our gratitude to Bell, or to the industrial pioneers who backed him and thus made a working device commercially possible. It is recited because it would be well if people, youth in particular, recognized the debt society owes to the quiet workers that we never hear of, especially those who are led on by their curiosity and their desire to explore, with very little thought about acclaim or fortune. It would also be well if people in this country generally regarded with more respect the industrial pioneers, who are willing to take a chance, and who furnish a very necessary element in commercial progress. If they are not present and active, very little is likely to happen.

I can illustrate this point by an invention of my own, where the industrial element was indeed absent, and where, as a result, nothing much occurred. I made a mistake, not in the invention, but in the way I handled it, and as a result an advance in techniques which I believe could have been of genuine public benefit moved into use haltingly and only to a limited extent. Yet my intentions were all right, and my procedure was such as would be acclaimed by those who understand only very little about how things occur in industry, or who do not grasp the need for their occurring at all.

Here is the story. In the course of an operation for cancer a piece of suspected tissue is removed, sent to the laboratory, and promptly tested to see whether it is benign or malignant. It makes a great deal of difference, to the patient, whether this is done promptly and correctly. In the laboratory the piece of tissue is frozen, thin slices are cut from it, and these are dyed and then examined under a microscope. The procedure is difficult and takes skill. If the piece is frozen hard it will crumble as the microtome is used to cut the thin section. If it is not frozen hard enough the knife will push it together and distort it. Some types of tissue will break all to pieces on the glass slide of the microscope when a drop of dye impinges on them.

So I worked out a process to avoid these difficulties. I took a very thin sheet of transparent plastic and coated it with a thin layer of gelatin, which was dampened but not wet. When the microtome had cut down to the point where sections were desired this sheet was pressed onto the fresh surface, to which it promptly froze securely. Then the knife moved across under it and cut off a thin section securely attached to the gelatin. It could not fall apart, nor could it distort.

Richard Hewitt,[12] a young chap who was helping me, added another idea. We dyed the gelatin before we started and adjusted its acidity. Then we did not need to add a drop of dye. When the frozen section melted on the slide, the dye transferred to it automatically and it was ready for examination. I timed myself and could perform all the operations, from the moment the frozen tissue was ready on the microtome until it was examined in the microscope, in thirty seconds. When I published a paper on the subject, I joined Hewitt as joint author, which was correct procedure since he contributed one of the main ideas. He hence used to get letters addressed "Dear Dr. Hewitt" asking him abstruse questions about sarcomas or the like, questions which were rather hard for a young photographer not long out of high school to answer.

I did everything one is expected to do in most academic and especially medical circles. I published papers, I sent Hewitt to medical meetings to demonstrate the method, I even made a motion picture which could be loaned and which showed just how to use the device. The one thing I did not do was to patent and commercialize it.

Certainly it was adopted—very slowly and in only a few hospitals, and by research men. I had many talks with surgeons and researchers, all of whom told me they considered it a genuine advance. Never did I hear from anyone who said the old method was better. Yet it has not become generally adopted even today. This came home to me some years later when a close member of my family underwent surgery, and I found the hospital was using the old, and I feel sure less revealing, method.

Why did it happen this way? One element was certainly present: vested interests. An old pathologist was not anxious to introduce a method by which a young girl, after a week's training, could do a better job than he had learned to do after many years. But I believe that this was a minor matter and that the real cause of difficulty lies elsewhere.

It is sad, but a fact, that the American public, professionals and all, have to be sold. They will not buy life insurance if its virtues are merely brought to their attention; they have to be persuaded by a salesman, whose salary, while he spends long hours doing the persuading, is of course ultimately paid by them. Mutual funds, by which an individual can participate in diversified and carefully studied investments, are of two kinds. One charges about 8 per cent to join, most of which covers the cost of selling. The other charges nothing. The ones that charge are large, the others small. Medical men will not adopt a new drug widely if it is merely described in medical journals; there are some thousands of salesmen on the road continuously visiting them and telling about the products, and the cost of this selling of course appears in the price of the drugs. There is no use bewailing all this. It is an inevitable accompaniment of the hectic sort of life we lead and the complex nature of our varied interests. We can try to improve the process, but we cannot afford to abolish it, for it is a part of our system of living as it has been bound to develop.

Now I certainly should have commercialized this particular advance—the improved method of making frozen sections. It would not have made any difference what I did with the proceeds, if any. But do not misunderstand me—this was an unusual case. For most of the inventions made in research laboratories, devoted to basic research and especially to medical research, the right thing to do is just what I did. But there are cases where such action is dead wrong, and no general policy can fit all cases. Research men, in fundamental research, just do not want to be bothered about patents or business affairs. Occasionally they should be. The legend about a better mousetrap is sheer hokum.

Sometimes an inventor remains unknown because he works in secret. We make no fetish of secrecy in the better industrial laboratories in this country. Research men there are allowed, even encouraged, to publish their results, and such a policy is sometimes regarded as foolhardy by old-timers, or even by European contemporaries. Of course there has to be some control of timing, to give a patent department time to act, and to allow the examination of outskirts to an idea, which every invention involves. But the old fear that researchers would by publication become known, and be promptly hired away by competitors, has largely disappeared. Such apprehension is more than offset by the fact that bright young scientists and engineers are keen to join an organization where some of their heroes work, and also by the fact that reasonable opportunity to publish develops loyalty. The Bell Laboratories are outstanding in this regard and in many others. I remember when I was young word was circulating that Eastman Kodak Laboratories were a good place to learn but not a good place to stay, for they were on a tight rein. That has been loosened since.

The inventor who works alone, who is isolated from the current trend of thought, and who hence does not grasp where the real opportunities lie, seldom makes a worthwhile invention. This was markedly true during the war, and I believe is true generally, especially in these days when things move fast in every field. It is also true that the lone scientist, insulated from frank criticism by colleagues, is likely to pursue ghosts or become enamored of phantoms.

One experience of mine illustrated this so forcibly that I am prompted to tell about it, even though it is a long tale.

In 1927 Professor Abram Joffe[13] came from Russia to the Massachusetts Institute of Technology, and there lectured on some theories of electric conduction in solids. The entire subject of solid conduction, which later led to the transistor, was then, of course, almost entirely unexplored. Joffe was president of the Russian Academy of Sciences. In fact, he had held that post under Czar Nicholas II, under Kerenski,[14] and under the Bolsheviki, which indicates that he was a scientist who disavowed any interest whatever in political matters and also that he was fast on his feet. He stayed at my home, and my wife and I soon came to have a sincere regard for him; he was dignified, genial, modest, generously inclined, intellectually honest beyond any doubt.

In his lecture he expounded his theory, and then, to my amazement, told how it could be applied to make an insulating material of far greater dielectric strength than existing materials. This woke me up in a hurry, for, if what he described was correct, it would revolutionize the design of electric machinery and power cables.

I promptly told some of my friends about it, the ones with whom I had been associated in other ventures. They got to work promptly and soon entered an agreement with Amtorg, the Russian trading agency in New York, under which they acquired an option on the American patent rights to the invention. The agreement called for only a small down payment, substantial payments to be made only after an interval in which we would have opportunity to check the results. This latter provision showed definitely that Joffe had no doubt about his results. He turned over to me a whole stack of papers, reciting tests which had been made in Russia and which confirmed his theory and the practical embodiment of his ideas.

During the next month or two, I repeated his tests, and they did not check! They showed the new insulation to be no improvement whatever. What next? Was I to question the great Joffe? Where had I made my error? The result was that I went to Russia to check on the spot.

L. K. Marshall and Mrs. Marshall went with me. Leningrad was a grim place in the latter part of November 1927; there had just been a great famine; there was tension in the air. But I found the Technical Institute at Leningrad full of keen, bright individuals. We had dinner one night; only English was spoken about the table, and the group could no doubt have done the same thing in French or German. From the conversation, the jokes, the appearance of the younger men, one might have thought he was in an American university.

But, one had to remember, many of that group were alive, after the purges, just because they were a part of Joffe's organization and protected by him in his quiet way. One incident especially stands out for me. I said to a young physicist, "I hope you do not mind my asking, but I am interested in the impressive way you make a bow." "Oh, don't you recognize it?" said he. "That is the court bow of the Polish court."

I soon became convinced that the whole theory and practice were fallacious. I tried to conceal the trend of my thinking, but I made one mistake. In talking before the group one day I pointed out a flaw in reasoning. I did so innocently enough, expecting one of them to show that no such flaw existed. No one did. But it was there all right, and the assembled company must have known it. This was to worry me a bit later.

How was it possible for a highly intelligent group of men to pursue diligently for months a false theory, without ever attacking it? The simple fact was that Joffe was above all criticism. One does not question his savior. This was strikingly impressed on me by one incident. Joffe and I were talking one day in a corridor. An oldish man with full beard and piercing eyes came bounding up the stairs two steps at a time. When he came behind Joffe he froze in position and waited, until I called Joffe's attention to him. Then he showed some samples and a sheet of computations, and talked about them excitedly. Joffe evidently said something kind to him, for he beamed and then bounded down again to his cubicle. Fanaticism, utter devotion! The sad fact was that, whenever a worker obtained a result that supported the theory, he reported it with exultation. If he obtained a contrary result, he knew he must be wrong.

This situation became even clearer as we rode the train to Moscow. Joffe had written a paper on another subject in physics and I had studied it. We sat up most of the night in discussion. I expressed careful criticisms. In fact, I knocked the argument out completely. Joffe was not annoyed, quite the contrary. He freely admitted my points and thanked me for pointing them out. He was isolated from criticism by his very eminence, and hungry for the give and take which is an essential part of real progress.

Moscow was crowded (I found out the reason only when we got to Germany); the Supreme Soviet was meeting, in the session which threw Trotsky [15] out of the Party. Joffe had difficulty in getting us rooms. I had a cot in the middle of an enormous chamber; the window draperies were falling apart. When I turned out the light there was a continuous rattle, and when I turned it on again a hundred big cockroaches scurried for cover.

One evening we went to the ballet. The audience was made up of soldiers of the Red Army and peasants in their high wool boots. Marshall and I may have had on the only white collars in the house. The performance was the old Russian ballet in all its magnificence. The setting, the dancing, the music were beyond anything I had ever witnessed. I still remember the music, which was the Hungarian Rhapsody, and the gorgeous dancing, in particular a little group of young apprentice girls off in a corner, allowed to put on a little dance, and no doubt very proud of it. The contrast of the whole warm ballet with the cold Russian winter outside and the tension one could sense in the air made me feel, strangely perhaps, that I would like to work closely with these people and learn to understand them.

When I came out I took a droshky back to my hotel. I evidently picked a driver who was full of vodka, and we had quite a ride. He would say something to me, and I would say "*da*" for want of more useful Russian. Whereupon he would whip up his horse, probably because I had said I wanted to go faster. In the middle of a block he would stand up and yell, others would pull off, and we would tear through an intersection. I fully expected to be dumped out in the snow, and carefully memorized the name of my hotel; I still remember it. But we arrived safely. I gave him an American dollar at which he beamed his gratitude, and probably went off for more vodka.

The time came to leave, and I had a brief conference with Joffe. I said to him, "I suppose you know the nature of the report I will have to give when I get back to America," and he said, "Yes, I know." So I told him that I thought the nature of that report could be held confidential among a small group for six months before it would become generally known, and I asked, "Will that be enough?" He said, "Yes, that will be enough." As it turned out, before the end of the six months he had his laboratory deeply engaged on another subject, and it was not disturbed by the authorities.

Leaving Russia, however, proved to be complicated. We were put off from one day to the next. The authorities had picked up our passports when we entered, and seemed in no hurry to give them back. We had, of course, at that time, no diplomatic representative in Moscow, and matters did not look good. However, after a few days we got our passports and took the train for Warsaw.

The next morning, in a cold gray dawn, we were taken off the train by a bevy of Polish soldiers, all equipped with swords and revolvers, who informed us that we could not pass the frontier and were to be sent back to Moscow. Marshall carried the argument, for his French was far better than mine. We had obtained Polish visas in Paris, but the officials pointed out that these visas had been rendered invalid. Apparently our Russian hosts had altered them a bit and had considered that a train ride to the frontier and back would get us off their necks for a while. I tried one artifice. I was then in the Naval Reserve, and I had in my pocket a letter from the Navy Department giving me permission to leave the country. So I spread it out and started to compose a cablegram to the Navy, which of course I knew would never arrive. Our Polish captors had not admitted they knew any English, but one of them slipped around and read over my shoulder. It may have had an effect, or it may have been Marshall's eloquence. At any rate, they sent us to Warsaw under arrest.

In the police station in Warsaw we had a ball. Our discussion with the captain was interrupted by a prostitute who had been picked up for soliciting in the railroad station. She was a black-eyed amazon. One of the police took hold of her and she bit him. In order to get on with our business, Marshall tried to shut off her torrent by use of a bit of American money, and she threw it in his face. All in all, she got the whole crowd in very good humor, including the captain, who agreed that, since we had to stay in the country for a few days before a hearing, we might as well see some of it. So, with a guard, we started off on a tour of Poland.

By chance we arrived at a customs office up in the hills. There an amenable official, for a consideration, put a new visa on our passports. Armed with this, we appeared at a station at the German frontier and went across. Our guard seemed pleased to get rid of us, and pleased with our parting gift. I judge he split it with the captain. I suppose, technically, the Polish police are still looking for me, but I have not tried to test this. We even saved our baggage. The ride to Berlin in an open car was a bit cool, but we managed to get some blankets. The Adlon was pleasant, and warm.

In Berlin I found out why the Russians wanted to keep us for a while. It appears they were just then negotiating with the Germans over the rights to the Joffe invention, and evidently thought our presence might not help on the deal. I met with Reinhold Rudenberg,[16] who was adviser to the German syndicate, as I was to the American. He was chief engineer of Siemens Schuchert, and professor at Charlottenberg. We agreed to exchange information, but I soon found out that this was a one-way street, so I did not tell him anything. I did get acquainted with him, and somewhat later invited him to lecture at the Massachusetts Institute of Technology.

When I returned to the United States I told our group that we had better save our money, which we did. The Germans went on for quite a while before they also tumbled to the flaw in the Joffe idea.

When Rudenberg came over, the Joffe invention was a thing of the past. We had become good friends and had some serious discussions on affairs in Germany. Soon thereafter he came to the United States permanently and became a professor at Harvard. This was just as well, for Hitler was coming to power, and Frau Rudenberg was the daughter of Minkowski [17] of Heidelberg, and hence Jewish. The Rudenbergs both became highly appreciated in academic circles in Cambridge. He said many times that his conversations with me decided him to leave Germany while there was still time. Hitler's insanity in driving eminent Jews out of Germany cost the Reich greatly; it even cost genuine progress on the taming of the power of the atom.

But the point of this recital is that if you wish to invent usefully, you must not attempt to do it in isolation, or to shield yourself from criticism. The world is full of would-be inventors who do just that. They never invent anything worthwhile. During the war many of them were merely annoying, and of course very unhappy, and convinced there was a conspiracy against them.

As I noted earlier, there is a story to be told in connection with the differential analyzer, for it illustrates a number of things that I am anxious to clarify. There is no question that my young assistants and I developed the differential analyzer at M.I.T. during the twenties. But who invented it? Ah, that is hard to answer for it depends upon how we define invention.

First, what is a differential analyzer? It is the first of the great family of modern analytical machines to appear—the computers, in ordinary parlance. It is an analogue machine. This means that when one has a problem before him, say the problem of how a bridge that has not yet been built will sway in a gusty wind, he proceeds to make a combination of mechanical or electrical elements which will act in exactly the same manner as the bridge—that is, will obey the same differential equations—and then by noting how this combination acts he will be able to predict the performance of the bridge. The trick, in a really useful device, is so to construct this model that by shifting some mechanical connections, or better by switching some electrical circuits, one can make it possible to handle a wide variety of differential equations, and hence of practical problems. If one does not know what a differential equation is, perhaps I can make it clear by a very simple example. Suppose an apple drops from a tree. One is supposed to have done so, to have hit Isaac Newton on the head and thus cleared his mind, although I doubt it. The thing we know about that apple is, to a first approximation, that its acceleration is constant, that is, that the rate at which it gains speed as it falls does not vary. So we just write this fact down in mathematical symbols. That is a differential equation, one very easy to solve, and thus we are enabled to make a plot of the position of the apple at every instant. But suppose we want to include the resistance that air offers to the fall. This just puts another term in our equation but makes it hard to solve formally. We can still very readily solve it on a machine. We simply connect together elements, electrical or mechanical gadgets, that represent the terms of the equation, and watch it perform. Actually, we interconnect integrators, rather than differentiators, for we thus avoid some serious difficulties. I have already discussed an integrator in connection with my old surveying instrument. The integrator is the heart of the differential analyzer.

We actually built three successive analyzers. The first one was just a breadboard machine. That is, it was made out of pieces of steel and anything else that was handy, and the object of it was only to see if the idea was sound. The second one was a mechanical machine. An unusual thing about this was that the integrators were made to be fairly precise—not an easy task. The integrator, as I have noted, could be simply a disc revolving at a controlled speed and a roller that pressed on it, which could be moved controlled distances from the center. The job was to keep the roller from slipping. If it carried any appreciable load, it would slip, and then the integrator would be highly inaccurate. We overcame this by putting on what is called a torque amplifier, but this was itself mechanical. We finally made integrators that had a precision of about one part in 1000, which was fairly good.

This machine was the one that the press talked about. The Army made a duplicate for the station where trajectories were computed. Our machine was copied by Professor Hartree [18] in Manchester, England. He did quite a lot of work on it. The difficulty with it, of course, was that it took quite a little time to change from one problem to another; and of course it did not have the scope that was necessary for many equations.

Finally came the third machine which we got into use just before the war. This one was wholly electrical, except that the integrators were still mechanical. It had a very large number of thermionic tubes, was quite hard to keep cool, and occupied a big space. It did work that was of some use during the war. After the war, when the digital machines began to come along, it was rapidly made obsolete, and we finally tore it down.

Many differential analyzers of the electrical type are now built and sold, and there are lots of engineers who use them. They are not highly precise, but they change easily from one problem to another, they give qualitative results very rapidly, and they are very useful gadgetry. They supplement the digital machine. If you want precision on a problem, you go to the digital. But if you want to explore, if you want to see what happens to the solution of an equation when you change constants, as you often do when you are designing something, the present-day differential analyzers are very convenient indeed. They are made in units that can be plugged together by cables, and they are most versatile. (They also would make great teaching machines if they were not so expensive.)

Now, quite a long time after we had the differential analyzer running, and doing a fairly acceptable job, I found an appendix I had not seen before in a book on dynamics, something of a classic, by Thomson and Tait.* In it Tait described how to connect a pair of integrators to solve a particular differential equation. Was Tait the inventor? As far as I know, he was the first to express the basic idea involved. But he did not describe how to do it or what to use as an integrator, and there were no integrators known in his day that could drive one another without such errors as to be fatal from a practical standpoint. So I make my belated bow to Tait, but I cannot call him the inventor, for inventors are supposed to produce operative results.

I can name an inventor who made a differential analyzer long before I did, or rather who readily could have done so without question if he had put his mind on it. This was Hannibal Ford. He was about as ingenious an individual as I ever heard of. He made the devices, the computers, if you will, to aim the great guns of battleships, to take into account the flight of the shell, including the effect of the rotation of the earth upon its path, the air density at the time, the speed and direction of the enemy target, and so on. This he did principally by interconnecting integrators. And he made a new form

* Thomson, Sir William, and Tait, Peter Guthrie, *Treatise on Natural Philosophy*. Cambridge University Press, 1890, Part I, pp. 488ff.

of integrator which could do the job without the help of a servomechanism. The resulting mechanism was a marvel of precision and completeness. Just how extraordinary it was can be shown by one great example from World War II, for the Germans were by then far advanced on the subject of gun direction, although Ford's great work was much earlier. There was only one decisive battle between battleships during the war. The *Bismarck*, not only the most powerful battleship in the world but also equipped with the most advanced fire control apparatus, put out to sea to attack Allied commerce. Shadowed by British cruisers, it was found and attacked by two British battleships, *Hood* and *Prince of Wales*. The battle opened at a range of 25,000 yards, about fifteen miles. A single salvo from the *Bismarck*, aimed by a combination of integrators, penetrated the deck of *Hood*, the magazine exploded, and a proud ship and three thousand men disappeared.

I knew of the existence of Ford's work, and that was all. I did come close to knowing a lot about it, and if I had, I could have built a far better differential analyzer than my first model, and I would have been honored to collaborate with him. Shortly after the first war I reported on board the battleship *Texas* in Newport harbor. I was in uniform as I was a lieutenant commander in the Naval Reserve, and a group of us went aboard to study the possible development of antiaircraft fire control. We never did. We were supposed to work with the airship *Shenandoah* and she did not show up; she probably went to a county fair at the behest of some senator. We all had a good time; I won a handball contest with the electrical officer of the ship, much to the delight of those who bet on me, but we did not do much to advance the cause of national defense. So I did not learn about Ford's work, beyond generalities, until long after. I wonder what would have happened if we had gotten together, but he was not aboard. He was a fascinating individual. He loved gadgets; his house was full of them: doors that would open when one spoke to them and so on. He did not care a hang about recognition of his work outside Navy circles. He just liked to build ingenious precise devices for people who could appreciate them. Did he invent the differential analyzer? One can say merely that he not only could have, but that he could have been the leader in the whole movement toward the modern computer, if he had wanted to. And the only reason he did not is probably that he did not move about in academic circles to see the need and the opportunity. I hail his memory. It is too bad the Navy monopolized his talents.

The other aspect of the family of modern computers is the digital machine, which today takes the center of the stage, and nearly the whole stage. Who invented the digital computer? I can write at once that I did not, in fact I had little to do with that whole development. The antecedents are interesting. Pascal [19] is the first man who really did anything on computing machinery. In fact, he built a computer, and he wrote about computers quite a bit. He had most of the ideas involved in the ordinary desk-type adding and multiplying machine. He was a pioneer of a very strange type, because he was primarily a theoretical physicist and mathematician, and ended up devoted to theology.

Charles Babbage [20] was the fellow who took these ideas, and some of his own, and started to make a really comprehensive machine to do all sorts of things. That could have been a genuine forerunner of the digital machine of the present day, but the trouble with Babbage was that he bit off more than he could chew. At the time he was working, mechanical devices could not be turned out cheaply or made reliable. The advent of reliable complexity is an exceedingly important phase of modern development. The modern computer, or telephone system for example, could not function if the units out of which it is built were not exceedingly reliable. It is very easy to see why. Suppose there are a million units involved. Assume the functioning of all of them is essential to performance. Let us assume they are quite reliable: For any one the chance of its failing in the next year is only one in a thousand. Still this means we must expect about three failures of the system per day, which is intolerable. There are ways of reducing the grief. In automatic routing of telephone calls, if one path is busy or defective, the machine automatically picks another. In the big computers, if a unit is faulty, the machine will not use it and will report it. Also the big computer has very ingenious ways of checking itself to catch mistakes. What's more, the digital machine can learn from its own experience, and the full import of this fact has not yet been truly realized. I want to return to it a bit later in another connection. Here I merely cite an example. Let us set up a digital computer to play checkers against a human opponent. We store in the computer's memory a catalogue of positions that may occur in a game, and a battery of possible moves to follow them, positions and moves being rated in accordance with assumed values. Position A occurs: the digital machine selects from its memory the best-rated move and makes it. Here follows the critical action. If the move leads to a position of higher value, the machine raises its rating, and, conversely, if the move brings about a poorer position, the machine lowers its rating. Thus the machine learns and approaches expertness at the game. We will return to this after a bit of discussion of reliability, upon which it in no small measure depends.

The nature of reliability is shown by the way ball bearings are sold. The companies do not guarantee a certain life. Rather they will present data which show that, of many of a given design and rating and under specified speed and load, 10 per cent may be expected to fail in, say, five thousand hours of use. It is always a statistical matter. We come to the superlative in reliability when the telephone company puts a cable across the Atlantic, with a complicated amplifier every forty miles, imbedded in the mud at the bottom of the ocean, and expects it to operate without a failure for twenty years.

The present-day reliability of components has been brought about by steady improvement of the design and materials of condensers, resistors, coils, relays, thermionic tubes, transistors, largely because of keen competition. In a socialistic economy, it probably would not happen at all rapidly.

As it is, I can buy a relay and expect it to operate for millions of cycles without fail. Also important, if I am a large customer, I can buy it for very little money. This fact has allowed many things, automation for example, to go forward rapidly.

Consider the automobile. When I drove fifty miles forty years ago, I fully expected something to quit. Now I complain if something goes wrong after ten thousand miles. And I am right—for the automobile companies press their suppliers so hard on prices that they do not even get the best that could be provided. On an automobile selling for $4000 they will cut the cost of the thermostat for the choke from twelve cents to eleven cents, and the $4000 car goes poof.

Babbage had no access to such reliable complexity as I have been talking about. Moreover, he was much too ambitious; instead of making a moderately complex machine which would work and give results (and that would attract attention), he started right in to make a machine that would be comprehensive indeed. And of course he never finished it. Babbage added some of his own ideas to those of Pascal, but primarily he was building rather than inventing. His conception of what he was trying to do fell short of practicality.

There are several somewhat distinct aspects of the whole revolution being brought about by the employment of machines to do man's mental drudgery for him, a development as significant for his future welfare as was the introduction of machines to perform his physical labor.

One such aspect is the handling of data, and this breaks into two parts. First, there is the storing and retrieval of figures and coded instructions. The modern computer has extensive memories for this purpose, and consults them very rapidly indeed. Second is the storing and finding of letters, sheets of figures, diagrams, all the complex records on which the conduct of business and libraries depends. This second phase of data handling has moved forward relatively slowly, but is now speeding up.

Just before World War II I got into this affair and, with the late John H. Howard, at M.I.T. built what we called a rapid selector. It had a somewhat strange history. I include it here because it illustrates well the type of invention which is almost inevitable once the technical elements it combines have advanced to the point where they are adequate for the purpose.

The way it worked was this. A reel of movie film had photographed on it a mass of data, perhaps 200,000 frames, each of some sort of document. The edge of this record film had a set of transparent dots on a black background which coded the adjacent frame. One set up the code of an item to be searched for by depressing a number of keys. As the roll of record film progressed through the machine, every time the set code coincided with the code of a frame a group of photocells triggered a flashing lamp, and that item was photographed onto a new strip of film—the reproduction film. Thus one could run through the reel and receive promptly a reproduction of every item in the collection called for by the set code. To accomplish this several things were necessary. First, the record film had to move rapidly as it left one reel and was wound up on another. This was easy. In a movie camera the film moves at only twenty-four frames per second. But it has to stop at each frame for an exposure and then start again. With continuous motion—no stops—the film could readily travel forty times as fast.

Second, when a desired frame was located, a photograph of it had to be made, without stopping the film, and without blur. Harold Edgerton [21] had solved this problem. His flash lamp gave intense light pulses of very brief duration. Exposures could be made in microseconds instead of centiseconds as in an ordinary camera. Thus no blur occurred in a picture of a fast moving film.

Finally, the coding had to be worked out so that only desired items would be selected and photographed. For this good sensitive photocells were then available. Several methods of coding were developed. The one easiest to describe, though not finally used, was as follows: Opposite each frame in the long film were the transparent dots, say a hundred of them, arranged in groups of ten each. At a keyboard one punched out the code of the item desired, producing a small card with ten holes punched in it, arranged in a pattern according to the keys that had been pressed. The card was inserted in the selector so that the fast film ran close under it and was strongly illuminated. As the record film moved, light passed through the card and the film and impinged on a photocell whenever a hole in the card and a dot in the film registered in position. If nine or fewer such coincidences occurred, the photocells remained inactive, paid no attention. But if there were ten such coincidences, indicating that the frame then in position corresponded exactly to the impressed code, the photocell triggered the flash lamp to take a picture. Since, at the exact instant this occurred, the chosen frame was not in a position to be easily photographed, a delay circuit was introduced, and the flash lamp fired when the fast moving film had advanced two frames, to bring the chosen frame in front of the camera lens. The camera was an ordinary movie camera, with its shutter always wide open. Every time the flash lamp operated, it advanced the sensitive reproduction film one frame. There was some tricky gadgetry, which need not be described, to catch chosen frames that happened to be close together on the record film, and there was a rig so designed that, after a run, the short piece of exposed reproduction film, containing only a few frames, could readily be cut off and developed.

Suppose one had a roll of film containing a million pictures of checks, all duly coded, and wanted to find a check made out for $1036.48. One would punch this number on the keyboard, adding zeros to fill out the ten places. One would then run the film through, taking about sixteen minutes (or less if one stopped as soon as the camera clicked), and would have a picture of the sought check. One could get several pictures if there were coincidences in amounts.

Of course the coding would usually be more complicated than this. For a library, for example, the individual frames of the record film might be summaries of technical articles, coded in accordance with the subject matter, author, date, and so on.

The whole device was not much bigger than a typewriter. Of course it was crude compared to the equipment that can be built today. It was an early step along a path that has now become elaborate indeed. That step had no more than been taken when the war came along, and several things happened. First, the patent application on the device, with its assignment to M.I.T., somehow got lost, or not followed up. Second, the machine itself was taken over by the group working on enemy codes, and that was the last I saw of it. One does not worry much about long-range problems during a war.

After the war the Department of Commerce wanted such machines to handle some of its data. No company could be found to introduce the device into commercial use, for there was no patent to protect them. Commerce finally had one or two made and put to work. But an art like this moves rapidly, and there were soon better machines available. Today there are a wide variety of ways of going about this job of finding a needle in a literary haystack. With magnetic tapes, transistors, printed circuits, there is a whole family of machines for handling data. The Eastman Kodak Company has made an especially interesting one called Minicard. It combines the sorting features of the old punched-card machines with the searching ability of the rapid selector. It is not very fast as yet, but no doubt soon will be.

All this is a long way from a girl digging through a file cabinet. Some day libraries will be fully mechanized. Then, without leaving one's office, it will be possible to pick up the phone, dial in a code, and have the actual paper one is looking for almost instantly at hand. Something of the sort has got to happen, or our libraries will become buried in the mass of books and articles now being printed, and searching in the old way will become hopeless.

Something more than thirty years ago, pondering these latter problems in the light of the work in the selector as well as the analytical machines, I conceived the idea of a machine that should be an extension of the personal memory and body of knowledge belonging to an individual, and should work in a fashion analogous to the working of the human brain—by association rather than by categorical classification. I called the device a memex, and published a discussion of it in the *Atlantic Monthly* in July 1945. Essentially, a memex is a filing system, a repository of information, and a scheme of searching and speedily finding a desired piece of information. It utilizes miniaturization, high-speed photography, memory cores such as computers embody, and provisions for the coding of items for recall, the linking of code to code to form trails, and then refinement or abandonment of trails by the machine as it learns about them. It is an extended, physical supplement for man's mind, and seeks to emulate his mind in its associative linking of items of information, and their retrieval as a result.

No memex could have been built when that article appeared. In the quarter-century since then, the idea has been with me almost constantly, and I have watched new developments in electronics, physics, chemistry, and logic to see how they might help to bring it to reality. That day is not yet here, but has come far closer, as I set out in an essay, "Memex Revisited," published in 1967.*

The heart of the idea is that of associative indexing whereby a particular item is caused to select another at once and automatically. The user of the machine, as he feeds items into it, ties them together by coding to form trails.

For the usual method of retrieving an item from storage we use a process of proceeding from subclass to subclass. Thus in consulting a dictionary or an index, we follow the first letter, then the second, and so on. The rapid selector worked this way. Practically all data retrieval in the great computers follows this method.

The brain and the memex operate on an entirely different basis. With an item in consciousness, or before one, another allied item is

* *Science Is Not Enough.* New York, William Morrow & Co., Inc., 1967, pp. 75–101.

suggested, and the brain or the memex almost instantly jumps to the second item, which suggests a third, and so on. Thus there are built up trails of association in the memory, of brain or machine. These trails bifurcate, cross other trails, become very complex. If not used they fade out; if much used they become emphasized. Thus a desired item may be found far more rapidly than by use of a clumsy index.

Millions of items are stored in man's brain, memories, sheets of data. Suppose I wish to recall what Aunt Susie, whom I haven't seen for twenty years, looked like. I don't start by turning to all of the pictures in my mind or storage where the name begins with S, and so on. Not at all. My brain runs rapidly—so rapidly I do not fully recognize that the process is going on in some cases—over when I saw her, what was the occasion, what were her mannerisms, and suddenly her picture is before my mind's eye. The goal of a memex is comparable—the use of the associative trail. Although we cannot hope to equal the speed and flexibility with which the mind follows an associative trail, it should be possible to beat the mind decisively in the permanence and clarity of the items resurrected from storage.

Here is where the ability of the digital computer to learn from its own experience—of which I spoke earlier—comes into play. Suppose that a memex has as its master a mechanical engineer and that he has a trail which he uses very frequently on the whole subject of heat transfer. The memex notices (we have to use such terms; there are no others) that nearly every time he pursues the trail there are a series of items on which he hardly pauses. It takes them out of the main trail and appends them as a side trail. It also notices that when he comes to a certain item he usually goes off on a side trail, so it proceeds to incorporate this in the main trail.

It can do more than this; it can build trails for its master. Say he suddenly becomes interested in the diffusion of hydrogen through steel at high temperatures, and he has no trail on it. Memex can work when he is not there. So he gives it instructions to search, furnishing the trail codes likely to have pertinent material. All night memex plods on, at ten or more pages a second. Whenever it finds the words "hydrogen" and "diffusion" in the same item, it links that item into a new trail. In the morning its master reviews the new trail, discarding most of the items, and joining the new trail to a pertinent position.

To leave the memex, there is one more story to be told about how secrecy can hide an inventor. In this case I was the one that got hid. Not that I minded at all; in fact it was a joyous excursion.

Along in the thirties the Navy got hold of me and asked me to look into a plan they had before them. By that time I was supposed to know something about machine analysis, and I was vice-president at the Massachusetts Institute of Technology, so I was taken in the front door instead of the back door. The plan was to mechanize their cryptography section, that is, to use machinery to break an enemy's secret cipher. They proposed to use punched-card machinery as originated by Hollerith [22] and then produced for use in business by the International Business Machines Company, the now great I.B.M. Now this breaking of a cipher was, and is, a far more complex matter than one would gather by reading Poe. It is not just a matter of shifting the letters about in accordance with a short key. Enciphering is done by a machine, and deciphering the same way. The key is very long. If it were indefinitely long the cipher would never be broken, except by capturing one of the machines, but then there would be no way of getting a distant station back into step if it ever fell out, and so a compromise is made. The job of breaking an enemy cipher consists of examining thousands of messages by statistical methods, combined with a canny sort of intuition possessed by some individuals. I could readily get intoxicated on the subject.

I made my study and reported that punched-card methods were not nearly fast enough, that they needed a special machine with at least one hundred times the speed. After a while they asked me if I could build such a machine, and I told them I thought I could. So we made a contract.

It was at this point that I derived my only benefit from the undertaking. I learned something about government contracts with universities, and this stood me in good stead later, in the days of N.D.R.C.

I told them I would do the work if they would pay just my out-of-pocket costs, that M.I.T. would contribute the cost of the overhead, that we would turn the whole thing over to the Navy and wished for neither profit nor credit, and that I wanted a simple contract which provided for just that, with no frills or inconvenient clauses. They soon laid before me a draft contract with all sorts of fancy clauses in it, provisions for accounting, rules regarding employment, and so on. It was brought to my hotel by a pleasant Navy captain. I read it, gave it back to him, told him to tell his boss to go to hell, and started to pack my suitcase. But he was back after a bit with a nice simple contract, and I went back home and started work. I think I still have a copy of that contract, and I do not know how they did it, that is, how they avoided all the constraints that fool legislation had imposed on them. They probably just ignored them and took a chance. It is too late to court-martial anyone about it now. And, later on, when I was on the other side of the fence and representing the United States Government, I did just the same thing, of course in a nice way and quite legally.

I went to work at M.I.T. and developed a device. When I got the thing built, I delivered it to the Navy. While this was going on, only four people at M.I.T. (Karl T. Compton, two young fellows I had working on it, and myself) knew anything whatever about it, or even knew the work was going on. When I delivered the machine to the Navy, they took over the whole thing—all the records, all the drawings, the machine itself, and the two men who had worked on it—so that nothing whatever was left at M.I.T., and I kept no copies of anything involved in it except that unusual contract.

Then the war came along, and this machine was undoubtedly used in connection with the breaking of the Japanese code. I cannot judge how instrumental it was in that regard—I certainly know it was used. Some of the young fellows who had worked with me said, in their enthusiasm, that it alone broke the code. I know this is not true, but I certainly know the machine was useful.

This led to incidents that were amusing. I remember one day some of the intelligence people over in the Army got me into a room, shut the door, and began to talk hush-hush. I do not think they looked under the pictures to see if there were any bugs present, but it was evident that they were going to introduce me to something very secret. So I interrupted them to say, "Are you gentlemen going to tell me about the breaking of the Japanese code?" They went up in the air about a foot, because that achievement was being held very secret indeed, and quite properly so, for the fact that we had solved that code gave us an enormous advantage in the Pacific for a long while. We were very careful not to use the information that came out of the intercepts in a way that would give away to the Japs the fact that we knew their code. The military forces had given up some very tempting opportunities rather than reveal this, so no wonder the people in the Army were a little disturbed. They wanted to know how I happened to know about it. I told them, "Well, inasmuch as I built one of the machines you are using, and inasmuch as I trained some of the young fellows who are working for you on this thing, it would be very strange if I did not know something about it."

The next incident took place after the war. Robert A. Lovett [23] was then Secretary of Defense, and the section on cryptanalysis made a request for funds in the budget. It was very hard for anyone to evaluate this request, because an accounting officer could not be told what that section was doing. Secretary Lovett asked me to look over the program and tell him whether the budget request seemed to make sense. I went out and visited the section. When I went in, there was my machine running. I told them I thought they were quite alert, that they had not known that I was coming out to see them until ten o'clock that morning, and in the meanwhile they had got that machine out of the attic, dusted it off, and got it running, and I thought that was pretty fast work. They took me around the corner, and there were six more machines just like it, all running. Then I said that now I was sure there was something the matter with them, that they were using obsolete machinery. As a matter of fact, of course, I looked over the rest of the program and told Lovett I thought it made sense, and the section got its money in the budget.

Visiting this section and talking with those fellows got me to thinking again about the subject, and I came to the conclusion that my machine was indeed obsolete. By that time the whole art had progressed to a point where it was questionable whether anything more could be done. Certainly far more speed was necessary if they were really going to have any success. So I wrote a memorandum and made some suggestions for a very much faster machine of a very different kind. As usual on any such matters, I wrote that memorandum in longhand, made no copies, sealed it, and sent it to the section by special messenger. A day or two later, a young lieutenant commander came down to see me. And I said, "Hope I did not burden you too much by giving you that memorandum in longhand, but I think that is the way to do it. I hope you did not have any trouble reading it—my handwriting is pretty bad." He said, "You forget, sir, that our business is cryptanalysis."

The story of inventions is an interesting one with many facets, involving the efforts of many men in addition to the inventor, and bearing directly on the strength and well-being of the entire nation. It is with this aspect of the story that I want to deal in the rest of this chapter.

The industrial vigor of this country is based on many things—the presence of a great uniform market, with a common language and nearly uniform customs, is certainly one—but two laws have had a great deal to do with that vigor. Neither law is perfect, and each needs improvement in practice. The one has furthered monopoly, the other has forestalled it, and the combination has kept us from stagnating. It has also caused confusion where the laws overlap.

The first, the patent law, gives to the holder of a patent exclusive control for seventeen years of the invention it describes. The second, the antitrust laws, prevent companies from conspiring together to fix prices or otherwise manipulate a market for their products.

Now a patent is a piece of property. Hence in theory if two companies exchange rights under their patents they are combining their property to control a market, and this combination violates the antitrust laws. The actuality is not quite so simple as this example, however.

Certainly if a company makes no patent deals, operates only under patents it owns, and infringes on no others, its monopoly should not be disturbed, and the courts so hold. An excellent example is Polaroid Corporation. Founded by Edwin Land,[24] one of the most ingenious men I ever knew (and also one of the wisest), it has grown and prospered, because of his inventions and those of his team. No one will deny that this accomplishment has benefited the country, and it demonstrates the operation of the patent system at its best.

If two or more companies exchange patent rights, but also agree they will license all comers under these patents on a reasonable basis, this arrangement also is held not to violate the antitrust laws. Again the principle involved is quite correct. Any trouble that arises springs from the definition of what is reasonable.

The object of the patent law is not to reward an inventor. Rather, the law aims to encourage venture capital to undertake the often great risks and expenditures necessary to develop an invention and put it on the market. It also encourages a company to maintain research programs to find new things.

This plan works well when an invention is clear-cut, simply defined. For example, take the pharmaceutical industry. A laboratory produces a new drug, say a new synthetic hormone for control of arthritis. The patent can merely recite the chemical structure or the process of making the new material. The company can go ahead and market, without using anyone else's patent rights. So it can make a profit, and thus justify its large research expenses. Is this good for the country? It certainly is. Since that industry is spending some hundreds of millions of dollars a year on research, we have all sorts of new drugs to cure our ills, to save lives—antibiotics, steroids, vaccines, the whole basis on which the modern physician ministers to our needs. If it were not for the patent system we would not have these blessings to anywhere near the extent that we do now. Government laboratories, valuable though they may be, would not produce them. It takes the profit motive and keen competition to bring them to being. The free enterprise system is not just a nice idea; it is the basis on which we lead the world economy.

But does this not result in a company's finding a drug that is utterly essential and holding the public up for exorbitant prices? Experience shows that this does not often occur. In fact I know of no really extreme case. Whenever a drug of real merit appears it is never very long before other companies get into the field, with competitive products or a better one. There are millions of possible chemical structures, even in a restricted field such as that of antibiotics. The first company to enter a new field wishes, above all, to establish its position, and its reputation with the medical profession. It will not risk its position by charging absurd prices at the outset. The history of the pharmaceutical industry over two generations has shown that it has prospered and made good profits, good enough to attract the capital it needs for its expansion and to support enormous research efforts, yet at the same time the cost of drugs has come down. But no company in the industry has made a profit that is far out of line with those of the most successful companies in other branches of our industry that involve equal risks.

Let us consider a case in which many patents are involved. It is now quite possible to make a household refrigerator which is simple and quiet by using the Peltier effect—the fact that if one passes an electric current, in the right direction, through a junction of two dissimilar metals, the junction will get cold. Interest has been aroused in this subject recently, for it has been found that the effect can be greatly enhanced by using semi-conductors, the sort of materials used in transistors, instead of metals.

Hence a refrigerator with no compressor, just some wires and a small fan, appears possible. It is not a practical product as yet—that is, it is not about to replace refrigerators with compressors in them—but let us have just one more technical advance and it will be. Suppose several companies, having spent a lot of money, and all holding patents on details necessary for a practical result, suddenly come upon the answer. The obvious thing to do is to exchange patent rights and proceed to market a better refrigerator. But the law says, quite properly, that this is a combination in restraint of trade. The companies can go ahead provided they agree to license all the rest of the industry at reasonable royalties. This is all right too; it will open the field and get the public what it wants. But what is a reasonable royalty? The courts are likely to consider 2 or 3 per cent on sales prices reasonable. Lawyers and judges are not likely to understand the costs and risks of a research program or the fact that for every success there are a dozen failures. So the purpose of the patent law, to encourage research and innovation for the benefit of the public, works in such cases in a very halting manner.

Our hypothetical companies hence are in a quandary. If they just exchange patents and go ahead, they are in violation of the antitrust laws. If they license all comers at, say 10 per cent royalty, the courts will probably consider that this is excessive and that they are in effect not licensing at all. If they license at 2 per cent, their competitors, all things considered, will be in a better position than they are, for their pioneering efforts and the false moves inevitable in all invention and development have placed a heavier burden on them than is represented by 2 per cent.

So much for the hypothetical case. Let's look at an actual one. This brings me back, as I forewarned you, to the early days of radio, when the Raytheon Company was just getting under way, back to that company and its then one important product, a gaseous rectifier called an S tube—the tube that C. G. Smith invented.

The heart of radio was then the thermionic tube, which was covered by dozens of patents in the hands of four large companies. But Raytheon's different sort of tube, filled with gas, was very useful indeed in a radio set as they were then built, and Raytheon had a patent on it. As I have said, C.G. had seen a point which the large laboratories had missed, and applied it practically. This was the use of short paths to confine the gas discharge to the places where it would operate correctly. We do not need to discuss the physics of the subject; it was just the sort of idea that profound researchers sometimes miss, and ingenious ones jump on. The Raytheon Company put together a plant employing two hundred or so men to make these tubes, started to manufacture them by the thousands, and was headed toward millions. Profit margins were excellent.

The first incident occurred when the Westinghouse Company decided the Raytheon patent could be fought successfully, and prepared to market essentially a duplicate of the Raytheon tube. This led to a conference I had with Charles Neave, patent attorney for Westinghouse, and a wise man. The encounter was a bit dramatic. He showed me a Westinghouse tube. The glass was coated so that one could not see inside, but he told me Westinghouse did not use the short path idea and hence did not infringe. I said, "Crack it." He looked at me a moment and then cracked the tube open over the edge of his wastebasket; and here was the short path clearly used. So he smiled and said he would advise the Westinghouse Company to keep off the grass.

There are several points here. There is not much doubt that Westinghouse could have ignored the Raytheon patent with impunity, for patent litigation costs a great deal of money and often takes years to come to an issue, and Raytheon probably could not have found the funds to fight the contest through. But Neave, being the man he was, would never have thought in those terms. The other side of the coin is that large companies, taken generally, are chary of infringing patents, which they think are valid, in the hands of small companies, for if the small companies do survive and win, juries are likely to have generous impulses.

The next chapter in the history is more important for our purposes. Westinghouse, General Electric, Radio Corporation, American Telephone and Telegraph all held important thermionic tube patents. They decided to get together, pool their patent rights, and license the entire radio industry. A.T.&T. joined merely because it wished to have no roadblocks in the way of developing improved telephone apparatus. In particular, it was afraid of the Langmuir patent held by General Electric. My own opinion is that that patent was invalid, but this is beside the point. A standard contract was formed and offered to all radio set manufacturers. There was just one thing wrong: Clause 9 of that contract required the manufacturers to buy all their tubes from the big outfits. It did not say "thermionic tubes"; it said "tubes." So the little Raytheon Company had its factory going full blast one week and all its orders cancelled the next.

This was about as clear a violation of the antitrust laws as one could wish. The law says that the injured party can sue and recover triple damages, based on the damage it has been caused. But the law does not say how the damaged party is to sue if the damage is severe enough to put it out of business. And this was just what one might have expected to happen. Today the situation is different, for the Department of Justice would get into the act. But, as matters then stood, it looked like curtains for Raytheon.

The backers of Raytheon were a group of Bostonians, but not of the stamp of George Apley. So Raytheon did not fold. Instead, it started making thermionic tubes, infringing every patent in sight, and selling them on the black market. Since it paid no royalties, it did not do badly. Of course it was sued in return, but, matters being a bit complicated, no cease and desist order issued from the court. Some five years later the whole thing was settled out of court; back royalties were forgiven, a reasonable cash settlement was made, and Raytheon went on to hire many more than two hundred men.

This story is told principally to illustrate the clash between patent laws and antitrust laws. But it also shows that we have come a long way, since those days, in rendering business practices more salutary and that the courts have comparably improved in performance. There is no doubt that the Raytheon Company came close to being exterminated in its infancy by the giants with which it competed. But, looking back over the last twenty years, I can find no case where a young, vigorous concern has been put out of business by unreasonable acts on the part of a great company in the same field. Some have been absorbed, yes, and paid for. Some have been held to reasonable respect for existing patents, but not bludgeoned. We have come a long way in those twenty years, and this is all to the good, for the continued advent of small, virile companies is one of the assurances we have that industrial giants will not become lethargic. It is also one of the ways in which young, ambitious men find opportunity to get ahead, and to benefit their fellows in so doing.

Our antitrust laws have, on balance, conferred great benefits upon us, as nearly all fair-minded men in business will agree. So have our patent statutes. Yet between the antitrust laws and the patent system a sharp conflict exists, as both our hypothetical and our actual case illustrate. This need not have been if the antitrust laws and the patent system were understood by legislators, the informed public, and the courts. The courts have held, quite correctly, that patents are property. But then they have held, quite incorrectly, that patents are subject to much the same rules as other property when it comes to antitrust matters. They have sometimes looked askance on the patent monopoly, holding to the childish concept that all monopoly, unless governmentally regulated, is evil. In the name of antitrust action they have destroyed patents under the delusion that they were conferring a benefit on the public by doing so. They look benignly on the holder of a patent who licenses all comers at low royalties, provided they are very low. All this is not to say that the patent system is perfect, that it does not need modernization, for it does. But it is to say that for a large, highly expensive industrial venture to rely on patent protection alone would too often be foolhardy, for the atmosphere created by the courts is sometimes hostile, and constitutes a roadblock in the path of industrial pioneering. We should not have expected otherwise. After all, few judges have experience in science, or engineering, or research, or invention, or for that matter in business. And all too often the robe and bench, over the years, can hardly fail to produce some feeling of omniscience. We have wise judges, who are positive in their decisions, firm in control, but who have still deep down an intellectual humility. The only difficulty is that we do not have enough of them, and no good system for getting more.

I should not be misunderstood at this point. We have all seen small companies prosper and grow to large ones just because they had a strong patent control and, of course, good management. My objection is just that present trends make this sort of thing more difficult. And I am convinced that this trend occurs because judges, by and large and with notable exceptions, just do not understand what the patent system is all about.

I once had the privilege of meeting the elder Elihu Root,[25] to take up with him the patent policy proposed for M.I.T. At the outset he said that an institution such as M.I.T. could take no course except to dedicate to the public patents which might arise from its research. I then asked him by what right M.I.T. could thus destroy property created under the statutes, which became law in accordance with an explicit clause in the Constitution. He told me that he had never thought of it in those terms, and we had a fine talk, after which he approved the policy.

My admiration for and devotion to wise jurists have been at times deep. I think of Learned Hand, who should have been on the Supreme Court, but was not. I think of Felix Frankfurter, who was, and who was a mainstay we sorely miss. Although I never knew them personally, I revere the names of Hughes, Holmes, Brandeis, Cardozo,[26] and many others. How do we produce giants like these? I wish I knew.

Until we can do so, however, we shall have to face the problem of conflict between antitrust laws and the patent system on a different base. One answer, of course, is for the judges to educate themselves on the subject, and there are plenty of able independent scientists and engineers who would be glad to help them to do so. That would be one sound step toward insuring the flow of innovation and creation essential to our industrial strength.

Another step, to which fortunately we are fairly well committed, is to encourage industrial research. Government research is not a substitute. It is sound practice, it is essential in fact, that government generously subsidize basic or fundamental research, from which all long-range technical progress, as well as fuller understanding of nature, ultimately arises. But we had better leave applied research to industry, where it belongs, and encourage it there. We have plenty of proof that a free enterprise system, and its profit motive, can cut rings around a socialistic centrally managed industrial effort, communistic or otherwise oriented.

One final suggestion remains as to how we might put our patent affairs in better order. In this country we have long used the scheme of commissions in areas where government is involved in highly technical matters. They are neither legislatures to make laws, nor courts to judge infractions, although they partake of the characteristics of both. The general idea is that a group of men brought together because of their background and experience can deal more intelligently with technical problems, to which they devote their full attention, than can a court which of necessity covers a wide range of subjects. Thus we have a Federal Power Commission to regulate our public utilities, a Securities Exchange Commission to control our stock exchanges and our industrial companies in their financial operations, and many others. In general they have operated well, certainly better than if every borderline departure from the law had to wait the laborious and lengthy operation of the judicial system before it could be resolved. They are controlled by the legislature, but not in detail, for the legislature can and does lay down the general rules under which they must operate, and which they interpret as individual problems come before them. There is an appeal from their decisions to the courts, but this should involve only the examination of whether they have decided within the law as it stands, and in accordance with due process. Unfortunately courts sometimes step beyond this and substitute their own judgment on economic and business matters, and this can at times replace the opinion of presumed experts with that of laymen, but the intent of the system is clear. In fact it is hard to see how the everyday problems of a complex technical civilization can be handled with grasp and intelligence unless there is some system by which expert experience is brought to bear upon them, and provided with sufficient authority and independence to do so.

Our patent system suffers because the delays and expense of the judicial process often perpetuate doubt and confusion. We have a Patent Office which, on the whole, works well, and which certainly aims to work justly. But the agency which issues patents should not then sit in judgment when there is conflict between two owners of patents which it has issued. We have a Court of Patent Appeals, but its prime function is to correct errors of the Patent Office.

A patent confers a temporary monopoly. When we have a necessary monopoly, as for example in the case of the railroads, we have a commission charged with the duty of seeing to it that the public gets a square deal. It also has the duty, not always well performed, of seeing to it that the railroads are in healthy condition to serve the public need.

So I would propose a patent commission, and I would give it great authority. Not only would I charge it with resolving all questions of patent infringement and assessing damages when warranted; I would go far beyond this and give it the power to pass upon all patent licensing and patent pooling, under the charge that decisions must be within the law, and also in the public interest. Would we then have the commission passing upon questions under the antitrust laws? We would, and I see no reason why not. The commission should certainly include men with business experience of varied sort, not just lawyers with corporate law experience. Our antitrust laws contain the words "unreasonable restraint of trade," and much of the problem involved in antitrust cases revolves about the word "reasonable." A restraint of trade can indeed be reasonable if it allows a group to bring to the use of the public a device or process which would not arrive at all if there were no restraint. There should, of course, be an appeal to the courts from a decision by the commission. But the review by the courts should examine whether the provisions of the law have been respected and due process has been followed. It should not, for example, sit in judgment as to what is reasonable, and substitute for the opinion of a group of men of experience the opinion of a court the members of which have had little or nothing in the way of experience with the highly technical and subtle subjects of invention, research, pioneering in industry, and the operations of the marketplace.

We need inventors and inventions, and we need to encourage them. In particular, we need to encourage the entrepreneurs, those who supply venture capital, those who have the nerve to take inventions over their initial bumps to create new companies to offset our trend toward industrial giants. Incidentally, we must discourage parasites and wheeler-dealers in this area as well as others. But, above all, we need better understanding of the whole complex affair, on the part of legislators, the courts, the public. There will be no lack of inventions; genuine inventors just can't help inventing. But we want more successful ones, and to get them requires better understanding.

VI

Of Energies, Engines, and Hobbies

Each of us, in his lifetime, eats many tons of food. We proceed to turn part of this into useful energy. Then we have to do something with the energy. Much of what we do with it is dictated by a boss, or by organized society, and by the need for the wherewithal to get the food in the first place. Yet there is always some energy which we use just for the joy of it, and it is that part of the whole that I write about here. And, in so doing, I deal with engines, which also use fuel to produce useful energy, and with the automobile and pollution. And with hobbies, and retirement, and the satisfaction of watching youngsters grow up, and a number of other things. I have something further to say about the evolution of techniques, particularly those involving engines, about their impact on the national life through their application or non-application in industry, and about how we do or do not remove the particular hindrances which their introduction encounters. Further, I want to consider what seems to me to be the sensible disposition of one's later years of comparative freedom of choice about what to do and why—about how then to utilize or apply one's own energies. One can always participate even in those later years—on the side perhaps, but still participate. In so doing it is well to avoid competition with younger men.

Let us examine this latter point with some care, for I find it causes men unnecessary grief in many cases as men get older. I made a decision on the matter early. For some years I was a professor, and also doing some consulting for industry, as I have said. I liked to teach. In fact I liked it so much that, when I came to a bifurcation in the road between academic and business concentration, I took the former without hesitation. After a time I was teaching, in the graduate school at M.I.T., a course on operational methods of circuit analysis. This sounds formidable, and it was at the time, for the subject was then in rather crude shape. Yet the method was badly needed by electrical engineers if they were to deal effectively with the increasingly complex problems they encountered in power networks and in electronics. I had delved into the matter in my doctorate thesis, but not very far. In fact, in later years I shuddered to think of how feeble my attempt had been. Later I wrote a book on the subject, also elementary and now obsolete, but at the time the only book that attempted to open up the subject to students and put it into use. Then I taught a course on operational methods, and bright students flocked to it, so many in fact that I soon had a couple of assistants, John L. Barnes and Murray Gardner,[1] teaching with me.

One spring I received a shock. With these two chaps I was putting together a final examination. Suddenly I came to the realization that they knew more about the subject than I did. In some ways this was not strange; they were concentrating on it, and I was getting involved in other things. But it hit me solidly. And right there I decided that I was not going to get in the way of younger men, and that, when the time came that I could not compete genuinely with them, I would get out. I have acted in accord with that decision many times since and, in the process of doing so, have found stimulation in trying to learn new things, relief in not being in competition with younger men, and satisfaction in watching those men succeed without having me as an obstacle.

I told my two assistants that I was through teaching the course and that it was theirs to develop. I told them my book was out of date and that they should write its successor (they later did). They did not believe me at the time, but I meant it. I did not touch the subject afterward. Today I cannot even read the papers of the extensive literature concerning it. Once in a while I take my own old book off the shelf to straighten out my thinking on some small point.

When I retired as head of the Carnegie Institution of Washington in 1955, I made several resolves. One was that I would not allow my portrait to be added to the gallery of former presidents, for I was gun-shy of formal portraits. Second, I would not allow the final meeting I attended to be lachrymose. Also I resolved to get out of town so that I could not possibly get in the hair of my young successor, Caryl P. Haskins. And, finally, I resolved to use my new freedom to barge into something else equally interesting.

The first matter was readily taken care of. When some of the trustees pressed me for a formal portrait, I told them I would sit for it provided the Chairman of the Board would paint it. Now the Chairman was Elihu Root, Jr.,[2] and he was an amateur painter of no mean accomplishment, a far better one, in my lay and inexpert opinion, than Churchill or Eisenhower, or in fact any other amateur whose work had come before me. So that is exactly what we did. He painted me with a pipe in my teeth, which assured that the result would not go into the formal gallery; it is hung in a side room, and I like it. After all, this was probably the only time that the retiring president has been painted by the Chairman of the Board.

The second matter involved a little more care. There was to be a final meeting of staff and trustees, with a short lecture and reception, in Elihu Root Hall at the Institution. There was a distinct danger that some staff member would rise and make a speech about the dear president who was departing and evidently on his last legs. Paul A. Scherer was my executive officer, and we fixed up a preventive. We installed a stereo system in the hall, placing it so no one would note its presence. It was a good one; when recorded words came over it the audience could tell exactly where the speaker was located on the stage in front of them, except of course that he was not there. At the proper moment I got up to announce the reception, and a brass band went across the stage behind me and completely drowned me out. When the band left, Paul tried to make the announcement, but a locomotive ran out behind him, blew off steam, and clanged its bell. Then we played some good music over the system. At the end of this I rose once more, at the left of the stage, and started again to announce. I was interrupted by a strong voice from the right of the stage: "What, no more music?" It was my own voice, which a student once described as "raucous" and which could not be mistaken. So I proceeded to have an argument with myself across the stage for a while, after which we adjourned. There was no weeping at that farewell party.

What was I to do, now that I had lots of time free? How was I to put to some use the energies that hitherto had been focused? The war was over, I was retired, and I was sure I still had a shot in the locker, or at least I was not going to vegetate even if the last shot had actually already been fired. I was going to avoid carefully the whole field of analytical machinery and computers. The trend had turned in the direction of digital machines; a whole new generation had taken hold. If I mixed with it, I could not possibly catch up with new techniques, and I did not intend to look foolish, or to become a patriarch emitting dull generalities on the subject. I certainly did not intend to try to get back into electrical engineering. So I looked for places where I could enter and find fresh interests, where my ignorance would be expected, and where I thought I had the opportunity to learn, and hoped I still had the ability. I found the opportunity in three places: in the pharmaceutical industry, in engines, and later in investment management. I was certainly ignorant enough in all three.

My opportunity in the pharmaceutical industry came in a roundabout way. During the war we had to face the problem of biological warfare; we could not ignore it because we did not know what the Germans might be doing. We had not the slightest intention of developing it for use, but we did have the task of studying it so that we would know what was possible, what the Germans *might* do, and thus be ready to move rapidly if they introduced it. In the light of some things they did in their concentration camps, we could not be sure how far Hitler would go in the direction of horror.

Among men generally there is a natural aversion toward biological methods in warfare, and it is as old as history. Sometimes, in one of the old sieges, a commander would load his catapults with dead horses and heave them over the walls of the beleaguered town, but the practice was frowned upon by professional military men. Medical men saw the study of biological warfare as contrary to all their tradition and philosophy. Even so, the threat had to be met, and it was met by George Merck,[3] who agreed to head the group engaged on the study. Without the slightest doubt, he did this at great personal risk. Mr. Stimson did not want the thing in the War Department, and I did not want it in O.S.R.D. So we inserted it in an agency headed by Paul McNutt.[4] I don't think Mr. McNutt knew he had it. It was in these circumstances that I became acquainted with George Merck and came to admire him greatly. Hence, when he suggested later that I become a member of the Board of Directors of Merck & Co., Inc., I readily did so. There is no finer company in the pharmaceutical industry, from its traditions, its business acumen, its research program, and its high standards of practice. When George Merck died I became Chairman of the Board and continued thus for some years.

I am pretty sure I did not damage the company any; I learned a lot, and I certainly enjoyed myself. The company's business revolved about chemistry and medicine. My chemistry was almost entirely absent, partly because the professor under whom I had studied it did not know how to teach, and partly because I early concluded, quite wrongly, that it was a dull subject. My knowledge of medicine was equally nonexistent. I had been a patient a number of times, but medical men do not consider, generally, that part of their task is to teach their patients about what is being done to them and why. During the war, I had, under orders from F.D.R., much to do with medical research. There I learned a lot about medical men but little about medicine. Still I remembered that, when I was young, I had had for a time the itch to become a physician and might indeed have headed that way if I had not been rather thoroughly strapped financially.

My best times at Merck were in the research laboratories. Although the subject matter was foreign to me, many of the same methods apply in research whatever the puzzle that is being attacked. Of course, also, it is conducive to comfort in the laboratory to be in the position to ask the questions. All my life I have mixed with all sorts of men. The ones I like best to be with are military men and research men. Just why the former are so much worth knowing appears elsewhere in this book. The research men are pleasant companions for a number of reasons. One is that frank interchange of ideas and information is essential to successful group research and will be found in any well-conducted laboratory. Now that group research is practiced everywhere, this frank interchange, and public opinion regarding it among the group, almost wholly banishes posing, jockeying for position, and evasiveness, and it is a relief to discuss things without them. Then, too, I feel sure that the research teams in the pharmaceutical companies are a jump ahead, in the fields on which they are working, of the medical schools and of physicians generally, in understanding disease and its control by means of drugs. They have to be to stay in business, and they spend several hundred million dollars a year to do so. There is a unique satisfaction in working with a team that is geared to make a profit, but which at the same time is vastly contributing to our health and comfort. Many times I have seen a friend saved from death or distress by a drug of which I witnessed the beginnings and the struggle to make it safe and reasonably free of side effects, and I am proud to have had a small part in it.

My administrative duties were not severe. I was, in a sense, a pinch hitter in an emergency. Still, the post broadened my outlook. I had participated in small companies during their most hazardous days, for friends and I had launched a number of them, two of which survived, as I have recounted elsewhere in this book. I had been second in command to Karl Compton at M.I.T., had been the head of the Carnegie Institution of Washington, and had thus seen academic administration at firsthand. Then I had more than my fill of government organization during the war. But the management of a large established business concern is a far different matter, and I was happy to be close to it for a time. The art of management is different in each of these, and it is not uncommon to find men who fully grasp one and know little about the others. They are likely to think the same methods apply everywhere, and this can cause confusion.

I remember some talks I had with Eisenhower just before he went to Columbia. There is no doubt Ike would have made a strong president of Columbia had he stayed there. There is also no doubt he would have gotten along well with his faculty, if only because of his geniality and the facility of getting men to work together, which he showed so clearly during the war. But he did not understand academic administration. Once when we were talking, we came to the problem of getting rid of a lazy or otherwise incompetent professor, and Ike indicated that, as a last resort, one would just have to fire him. I replied that he might fire the chap, but he would not leave the campus. Of course I could not wish that Ike had stayed longer in academic life, for look where he went. But, if he had stayed, he would have finally learned and understood. He would have learned all about the mysterious rite of academic tenure, and the persuasive ways of the American Association of University Professors. And there are a number of things about academic administration which could stand the type of change he would have brought about. Still, at the outset, he was as ignorant of the new scene into which he had come as would have been a professor of Greek suddenly put in command of a division.

One of the greatest satisfactions in life is to see younger men with whom one has had association step forward, take responsibility, and become important figures in American life. Justice Holmes said that the "prize of the general is not a bigger tent, but command." In the same way the reward of a great teacher lies in the success in later life of his students. This, too, should be the reward of a leader in any field. At Merck and later I saw Jack Connor become president of the company, move from there to be Secretary of Commerce, then back to industry to head another great company. I watched Henry Gadsden [5] succeed him as president at Merck and lead the company to new success. I watched Max Tishler [6] as he struggled and finally became one of the country's great research directors. If I was able really to help any of these on their way, I am sure I made no mistake when I decided to penetrate into the mysteries of the pharmaceutical industry.

But this is enough of how I disposed of energies after I had, in theory, retired—enough for the present, anyhow. The subject of engines is more interesting. It has fascinated me since youth. I have even contributed to it by inventions, although without startling anyone in that regard. Part of the lure was probably due, again, to the fact that the subject is remote from what I am supposed to know something about. And part of the interest, I believe, comes from the conviction, which I have held for fifty years, that the automobile industry in this country is half asleep and incapable of effective innovative cerebration. It gave us fins, and chrome, and wild advertising names—and a system of locking that makes the American car foolishly easy for the sneak thief to make off with. Once you could lock not only the ignition but also the transmission. And only now, after many years and many jumps in insurance rates, the industry is finally going back to locks that really lock. To be blunt, I think the men who manage the automobile industry are dumb, from the level of the chief engineers and plant managers up.

Here I am going to be a bit technical, and the reader who does not enjoy puzzling about technical matters is invited to skip this section and join me again toward the end of the chapter.

Along about 1915 I owned a Stanley Steamer, secondhand of course. It was a marvelous car. It would outperform any gasoline car on the road. I could put its nose against a post, pull down its throttle, and slip its wheels on a dry road. I could start from a dead stop and pass anything on wheels. Driving along a country road, I could hear the birds singing in the trees.

Of course it had its faults. I spent more time under it than in it, for the boiler leaked. It had no condenser, and every fifty miles or so it had to be fed a hundred gallons of water, sometimes by bucket from a roadside brook. I also found that it had certain limitations in climbing icy hills.

I was then courting a girl who lived on a steep hill, the same girl, incidentally, who cautioned me about putting my dirty shoes on the sofa as I wrote this. On cold days there was often an icy patch halfway up that hill. I would chug away slowly up the hill until I hit that patch. Then my wheels would spin, all the steam would be exhausted out of the steam line, and I would drop back. The only way I could get over the patch was to back to the foot of the hill, look about for possible cops, pull the throttle way down, and roar over the patch, shutting off the steam for a moment as I did so.

The old Stanley engine was a regular piston crank affair with a Stevenson-like valve arrangement and a variable cutoff. The throttle operated the cutoff as well as doing the throttling (that is, through operating range it was merely varying the cutoff). You were supposed to run six hundred pounds per square inch in the boiler. I actually ran eight hundred, which was a foolish thing to do because that was what made the boiler leak every little while.

I had some interesting times with that car. It would be going along the road and for some reason or other (probably because the leak in the boiler squirted a little steam out around the edge of a tube) the pilot would get blown out. The controls would turn on the main kerosene line and the whole thing would be flooded with kerosene. When that happened, you stopped at the side of the road and waited until you felt the fumes had blown away. Then you touched a match to the pilot again. If you were too soon with the match, the gaseous mixture would blow up. I remember its blowing up one time when it sent the top of the hood thirty feet in the air and blew all the asbestos off the boiler. Just as I was about to touch a match to it on that occasion, a fellow—one of the kibitzers—was leaning down and looking in, and I told him that he had better move back. Fortunately he did, a few feet, but the blast blew his hat off nevertheless.

If you touched the burner much too early you just got a fire. One night around midnight I was coming back along the Revere Beach Boulevard toward Medford and the thing got afire. It was a pretty blaze. So I pulled over to the side of the road and sat down on the curbstone to wait. A park cop came along and said, "Is that your car?" And I said, "Oh yes, that's my car." He said, "If you want to burn it up, why, there's a good dump right over here." I told him, "No, I'm not going to burn it up; it'll be all right in a minute or two."

He stopped with me and after a little while the fire went out and he said, "What are you going to do now?" I said, "I'm going to drive it home." He said, "The hell you are." I got in and pulled down the throttle, and off I went. After the fire went out the boiler would hold steam enough to drive six or eight miles. So I had no difficulty in getting away that evening, much to the surprise, I am sure, of the cop.

But I was convinced then, and still am, that the steam engine is a better automobile engine than the things we now use. One reason, which we are just now beginning to take seriously, and which the automobile industry ignored for two generations, is that a steam car does not pollute the air the way an internal combustion engine does. With a properly adjusted flame it does not create carbon monoxide to leak into the car and asphyxiate the passengers. Moreover, it has natural characteristics that fit. It is inherently a variable-speed engine, and the automobile certainly provides a variable-speed load. The internal combustion engine is inherently a constant-speed engine, and all sorts of gadgetry have hence been necessary to fit engine and car together.

The steam engine went out for a number of reasons. The Stanleys,[7] who held important patents, became old and quit vigorous development. Perhaps there is a lesson here—that old men should not hold controlling patents. A second factor was this: The gasoline engine developers got legislation enacted in various places which prohibited an open flame in a garage, and the steamer had a pilot flame that generally was kept alight. To keep steam cars, with their pilots burning, out of garages was fairly easy to do because of course town fathers generally were afraid of fire. But so far as the technical aspects went, it would have been possible to prevent flame hazards. One could have enclosed the flame, electric ignition was possible, and in some cases one did not need to have a flame in the garage at all. As I have said, after the fire went out on a steam automobile, you could run it for many miles on the steam still in the boilers. In fact, I used to leave my car in the garage overnight, come out in the morning, run out of the garage on the steam that was still in the boiler, and fire up after I was on the road. Hence, though it was possible for a steam car to be developed that would not have any open flame in the garage, this legislation was one thing that finally killed off the steam automobile.

The same sort of fate befell the old steam launch, that pleasant craft which did not vibrate, was quiet, and was inexpensive to operate. It was a lot better than most of the craft which succeeded it, which shake the liver and make passengers holler so that, though they cannot hear one another, they can readily be heard by those a cable's length away. Of course it did not put 100 horsepower (measured in strange ways) into a sixteen-foot boat, although it could have. And one had to wait a few minutes before leaving the mooring. It was a comforting craft—the kind due to be pushed out by a noisy generation.

But it was also pushed out by an artifice. Laws were passed, all over the land, which provided that only a licensed engineer might operate a steam engine. These laws did not just require a certified sealed safety valve which would positively prevent explosions, or the use of flash boilers with little water, and a shield so that even if an explosion occurred it would be harmless. They said, "Hire an engineer." No doubt the laws were favored by the steam engineers as likely to provide jobs. What they did was to make the steam launch obsolete. Perhaps, if some day we have steam engines in automobiles, as we may, the steam launch will come back. If it does, I want one, for a nice pleasant ride—almost as good as sailing, and available to those whose sailing days are over.

By the time others besides the Stanleys got going on engine development, with flash boilers, good condensers, and so on, the gas car industry was well established. After that one could not successfully horn in on the automobile industry with shoestring money. Kaiser [8] found that out somewhat later.

But the steam car is now again on its way. The engine will probably be hermetically sealed, requiring no lubrication, no makeup water. Its condenser will operate with vacuum. It will use diesel oil as fuel, and will cause very little pollution. It will be quiet, flexible, powerful. It will get off from a cold start in thirty seconds. It will contain so little water that the danger of explosion will be made negligible. It will have a fire, in a furnace instead of inside cylinders, but this will be so shielded, in the manner of a miner's lamp, that it will not present a fire hazard in a garage. The engine itself will be so light that one mechanic can pick it up in his arms. It will last for years without attention. When in full production it will be cheaper to build, and to operate, than present cars. The steam car will not be built by the present automobile industry unless some unit of that industry suddenly sees a great light, or public pressure or foreign imports force the decision, or government orders subsidize a new unit in the industry. It will have competition, as I will discuss below. But in fact, if I were disinterested, and had to bet, I might bet on steam.

The electric car would also be in the running if it had a good battery, but it hasn't. There have been electric batteries ever since Galvani, in 1791, touched a piece of metal to a frog's leg and made it kick. Volta, in 1800, rolled up some sheets of metal, separated by pieces of his wife's flannels saturated with sulphuric acid, and thus made the first of a long series of battery forms. They separate into two classes: primary batteries—the dry cells of our flashlights—which transform chemicals into electrical energy, and quit when the chemicals are used up; and secondary or storage batteries, which are pumped up or charged by running electric current through them to cause chemical changes, and then deliver back some of the energy thus stored.

Today we have a third form, the fuel cell, which is causing quite a stir. It is nothing but a primary battery, but with the important advantage that one can feed the chemicals in continuously, and get the electric energy out, and do so indefinitely unless something goes wrong. It is well to sort some of this out.

Why do we not have better electric automobiles? The general idea seems to be a good one. Just a battery and a motor; no engine, flammable fuel, carburetor, ignition, starter, or a dozen other complications. No exhaust, no air pollution. Plug the battery in to charge at night, use it all day, at costs per mile less than that of gasoline—it sounds like an excellent idea.

And electric automobiles have been around for two generations. In the early days one sat in a glass box, with a stick in his hands, and proceeded with much dignity, little speed, and not very far. Once, forty years ago, in a traffic jam, I sat behind one occupied by a gorgeously bewhiskered patriarch, sitting up very straight. The outer lane was moving, and young couples, inserted into tin jalopies by a shoehorn, jeered at the old man as they went by. To my delight he solemnly thumbed his nose at them. But his type of car has disappeared, except for urban deliveries and for fork trucks in warehouses, just because it did not have power enough to survive.

The trouble is a simple one. The battery weighs too much and costs too much. Planté [9] made a lead storage battery in 1859, and, except in details, it has not changed since. We still store our electric energy, for our starter batteries in cars, by conversion between lead oxide and sulphate, as Planté taught. Edison made a better battery in 1908, using iron and nickel oxide; and André,[10] a generation later, devised one which was much better, using silver and zinc. But these were not great enough improvements, in terms of first cost and watt-hour output per pound of weight, to make the electric automobile a real contender for general use. Lately, following the outcry on pollution, there have been some new proposals. The ones I hear about seem to me bizarre. One, for example, uses hot metallic sodium. Anyone who has ever seen what happens when sodium and water get together does not want to carry the stuff around in his car.

What has blocked the chemists during all these years? Not being a chemist, I would not know. With the millions of chemical compounds to choose from, it would seem to a layman that we ought to do better. When we fill up our gas tank we merely acquire some energy in chemical form, and this will push our car around for quite a while, even though, for reasons that will appear below, only a very small fraction of the stored energy becomes available to turn the wheels. It would seem that we ought to have a chemical of comparable weight to do the job, and one that we could restore to its original condition by connecting it to an electrical power source; that is, we ought to have a lightweight inexpensive storage battery. We do not have it. In fact we still use lead, the heaviest common stuff around. I suspect that there is some subtle chemical principle involved, which I naturally do not understand, but which limits the choice among the materials available to the chemist for this purpose. If this is so, then we may as well write off the automobile powered by an electrical storage battery, except for service where low power and low cruising range can be tolerated. In judging this, however, one should perhaps not overlook the combination of a battery with a small engine to keep it charged. The engine drives the car, except when there is a heavy load for acceleration or on a hill. Then the battery comes in to help out. Here is needed a battery that will endure a very heavy discharge rate, and the nickel-cadmium battery seems to fit the need well.

Now appears the fuel cell and it is exciting, for it may solve the problem. As I have said, a fuel cell is just a primary battery, of which Volta made the first crude form, with one essential difference. In a primary battery, one puts in some chemicals; as they alter in form they produce electric power, and that is fine as long as they last. In the fuel cell one continues to renew them. Usually the chemicals are in the form of gases, hydrogen and oxygen in the simplest form, and one feeds them by pumping them in through porous passages. Of course one would like to use gasoline vapor and air, instead of hydrogen and oxygen. There is no theoretical reason why these couldn't be used; just technical difficulties.

Why would this be better than to use the gasoline in a conventional internal combustion engine? There are two reasons. First, one has a nice simple electric motor drive instead of a car full of gadgetry. Second, one can get all the energy, except for incidental losses, instead of just a small piece of it. With the fuel cell we transform the chemical energy directly into electrical energy, and the latter into mechanical energy, both transformations without any tax. With the internal combustion engine we transform chemical energy into heat, then the latter into mechanical energy, and the tax is heavy. This tax is due to a rule, called the second law of thermodynamics, which records the fact that when one transforms heat energy into any other form one can get out only a fraction of the energy available. In the automobile engine, operating under the most favorable conditions, the tax is about 65 per cent. Under variable conditions of running the automobile, it is far higher. With the fuel cell, no tax. Also, no pollution. Hence the excitement.

Our human mechanism pursues this direct path. We take in fuel by eating a pound or so of food, and walk, play golf, argue all day, for we convert the chemical energy directly into mechanical form in our muscles, and into electrical form in our nerves. Some time ago biologists, short on theory, and joined by philosophers and even theologians, argued as to whether we somehow avoid the second law of thermodynamics as we use the energy in food for our purposes. Of course we do; the law doesn't apply to transformation of chemical to mechanical energy. Its misapplication has caused plenty of confusion. For one thing, the law says that, in a closed system, the entropy tends to increase; that is, the system trends toward a dull uniformity throughout. As I write it is spring, and the crocuses are up in my yard. Instead of a drab uniform bed of soil I have joyous form and color. Does this deny the law? Not at all. My garden is not a closed system. On the other hand, when the philosophers argue that the whole universe is running down, that we are headed eventually for a dead, cold uniformity, they are on sound ground in using the second law of thermodynamics in their arguments, for the universe is a closed system. Even here, however, modern cosmologists prefer to talk about a universe that restores itself every eighty billion years or so, that periodically expands and contracts. I don't quite see how they toss the second law overboard as they paint their pictures. But this puzzle has little to do with engines.

Do we get powerful flexible electric automobiles using fuel cells? Not at once. The technical problems are severe. The cells become dirty and hence poisoned. One has to be sure they do not overheat. Each cell delivers only a small voltage, and electric motors run on small voltages are clumsy. Engineers all over the country are working on the problems and have a tough task. Still, the basic idea is sound, and some day we ought to do better than just using a storage battery to power an electric automobile.

Here begins the story of my competitor engine. Before the days of internal combustion engines, the hot-air engine was widely used.

It had no valves, no ignition system, no noisy exhaust, and it lasted indefinitely. The farmer, to pump water, merely had to build a fire under it, give the wheel a turn, and go away; whereupon it would pump until the fire went out. Its efficiency, even in those early days, was not bad. Its output horsepower per pound of weight, or per cubic foot of space, was sadly low. Various people tried to improve it. Ericsson [11] of *Monitor* fame—credited with the screw propeller, although he probably did not invent it—worked on it but just made it worse. When the internal combustion engine came along, the hot-air engine disappeared.

The hot-air engine is really a form of external combustion engine, that is, one like the steam engine, where the fuel applies the heat externally, not by an explosion inside a cylinder as in the internal combustion engine, gasoline or diesel. It took two forms.

The first—the one the farmers used—had a big cylinder inside which was a loosely fitting displacer, which was nothing but a tin can, moved by a rod projecting out of the cylinder. One built a fire under the bottom of the cylinder and cooled the top with water. When one moved the displacer to the bottom of the cylinder most of the air inside moved to the top and got cooled, and its pressure thus dropped. When the displacer was moved up, the opposite occurred and the pressure rose. All one then had to do was to provide a small piston so that these pressures pushed and pulled, and connect this piston, through a crank, to turn a big flywheel. The final step was to put another crank on the flywheel shaft, at the correct angle, to move the rod and hence the displacer.

The second form is a bit harder to understand than its simple predecessor. It involves two cylinders, each with its piston, and each connected to a crank on the flywheel shaft. The two cranks are at right angles, that is, like the hands of a clock at three o'clock. The two cylinders are connected through a regenerator. One cylinder is heated all over in the furnace, the other is cooled. As the shaft turns, the bulk of the gas is shifted back and forth between hot and cold cylinders, and the pressure rises and falls. It turns out that there is power thus developed to turn the flywheel; but to explain just why this is would take us into trigonometry. The regenerator, which was the Reverend R. Stirling's [12] great contribution, was, in the early engines, just a tube full of iron washers or steel wool. Its purpose was to cause the temperature of the gas to rise or fall gradually, rather than abruptly, as it passed from one cylinder to the other. To describe the benefit of this would require the use of thermodynamics, which is not a subject to enter lightly. Interestingly, Stirling, who was a Scottish clergyman, and who described the principle of the engine in 1816, and worked on it for more than a decade thereafter, probably did not write any thermodynamic equations, but that did not prevent him from making a brilliant and rather subtle invention.

Back in the thirties I conceived the idea that, with modern methods and modern gases and so on, one could make a hot-air engine really comparable to the internal combustion engine; that it would have efficiencies fully as good; that its output per pound or per cubic foot could be made as high; and that once it was made, it would practically never wear out, because of its inherent construction. I was going to make it of metals that would hold their strength at red heat, and would permit a red-hot input instead of a warm input on a cast-iron cylinder. Then I would have helium at high pressure as the working gas, instead of air at ordinary pressure. A stream of turbulent helium at one hundred atmospheres' pressure will take heat out of a surface almost as well as playing a hose on it.

Well, the size and weight came down all right, but not enough. Since then much work has been done on the Stirling engine, even by the great automobile companies, now that they have finally awakened to the threat of pollution. The Philips Company of Eindhoven has spent a lot of money and made hot-gas engines that are decidedly attractive. If the automobile industry did not have hardening of the arteries we would soon have them in cars. I have even added one or two ideas of my own to what they have done. The engine is attractive, for it is about as simple as one could imagine—just an assemblage of cylinders and pipes, and heating one place and cooling another make it run. Perhaps, after being ignored for three-quarters of a century, it is about to appear again on the scene.

A striking advance was made during the war on methods of making liquid oxygen, and the methods used had some kinship with the Stirling hot-gas engines. If one puts a motor on such an engine and runs it backward, it becomes a refrigerator. When it makes a part cold enough, air becomes condensed and we have liquid air. Then it is easy enough to separate the mixture and obtain liquid nitrogen and oxygen. The Air Force badly needed ways of obtaining oxygen for pilots, often in remote regions, and a broad program was instituted involving a section of O.S.R.D. and a number of industrial research laboratories. Out of this came some very novel methods of liquefying gases, notably one by Samuel C. Collins[13] at M.I.T. One result was that the cost of oxygen came down, after the war, to a fraction of what it had been before, and this made practical a whole reordering of methods of making steel. A blast of straight oxygen, into an open hearth or a blast furnace, can make things hum. The whole industry, after a long period of somnolence, is decidedly on the move, with continuous casting and soon, probably, steelmaking direct from the ore. We do not give it a great deal of thought, but the fact that a fine piece of steel can be bought for a few cents makes a great difference in the way we live. I have not computed it, but I suspect that the saving in costs to the steel industry, and hence ultimately to the public, from the oxygen program much more than offset the entire expenditure of O.S.R.D. on war research.

There are other engines we should not overlook. The free-piston engine is decidedly interesting, although it does not help much on the pollution problem. I write about it because it is a fascinating gadget, and because I have found enjoyment in exploring such matters. It is not of great commercial importance and probably never will be. If I were writing about important ways of developing power I would write about the new great atomic energy plants. But this chapter is concerned with devices that are interesting, whether or not they are likely to have commercial importance. In a free-piston engine there are no cranks or connecting rods. Two pistons just oscillate in a cylinder. Between them is a diesel cycle. This is supercharged and the exhaust drives a turbine. In the conventional form there are racks and gears to keep the oscillating pistons in step. I worked on the subject for a while and succeeded in getting rid of this mechanism, so that the pistons become really free, but are still kept in synchronism. Free-piston engines have been used for a long time in Europe—for power supply to isolated towns, for example. Little attention has been paid to them in this country. One automobile company brought in some small ones and put them in automobiles. But there was no intention of adopting them, or even of developing them seriously.

Still they are very fascinating devices which may well have worthwhile uses in the future, and we can spend a few more moments to consider how they work. Consider a plain smooth cylinder closed at both ends, and in it put two simple cylindrical slugs that fit it closely. To make it interesting, assume they are fitted with piston rings made of solid lubricants, such as graphite, so that no lubricating oil is needed. Consider that they are approaching one another at high speed, compressing the air between them. At their closest approach let us squirt in a bit of fuel oil. It will ignite, for the compressed air is very hot, and thus will raise the pressure still higher. This is the diesel procedure. The slugs fly apart violently. When they approach the ends of the cylinder they compress air at those points, and this bounces them back together to repeat the action. As they fly apart they uncover ports, and fresh air comes in for the next explosion. Thus they oscillate continuously.

A basketball player dribbles the ball by bouncing it back and forth between his hand and the floor. In much the same way, the slug in the cylinder bounces back and forth between the cushions of air. One uses two slugs and thus eliminates vibration. In fact, one can balance a nickel on edge on the cylinder of a free-piston engine when it is running at full speed. My contribution was to show that, by proper connection of the bounce volumes, the plugs can be made to keep in step, that is, to maintain synchronism, without any mechanical aid.

If nothing more is done than I have thus far outlined, the oscillation will build up to a higher and higher amplitude, for power is being developed. Various things can be done to use this power. One, which is the oldest form, is to use part of the volume between the slugs and the cylinder ends, which are made usually at larger diameter, to compress air. This can be used as supercharged air for the central portion that is the combustion volume, and then the exhaust from this, still at considerable pressure and temperature, can be used to drive a turbine. This is the form in which the free-piston engine has been used in Europe. Or, without supercharging or a turbine, one can have a simple air compressor. Or one can put in oil pistons and have a hydraulic pump. Or one can add linear alternators and have a dynamo to furnish electric power. In all these cases there are no shafts or connecting rods, and the devices, because of their symmetry, are strikingly free of vibration. There are all sorts of interesting possibilities. Yet, in this country, where we pride ourselves on our ingenuity and resourcefulness, we have done very little indeed toward developing them.

Then there is the gas turbine. This has been much improved lately by the introduction of effective regenerators. It is becoming used for all sorts of things, and has even been used to propel automobiles. I have not had a part in its development. It is about the only engine that I have not tinkered with in one way or another. My personal opinion is that it has a great future, but that there are probably better engines for the automobile field. One can by no means be sure on such a matter. Just one more step would render it the great engine of the future. Turbine rotors, today, are rather expensive. I would not be at all surprised if some ingenious chap came along with a way of making them inexpensively, and this might well do the trick.

When we think about the future we should not overlook the rotary engines. I looked these up some forty years ago and found dozens of patents covering all sorts of forms and details; there are now probably hundreds. Such engines should and could be simple, vibration-free, cheap to build. Yet they have hardly been used at all. The reason is that they are hard to seal. Oh, it can be done, but it is by no means easy. It will be interesting to explore this problem briefly, for the day of the rotary engine may be nearly here.

A rotary engine consists of one or more odd-shaped pieces which revolve inside a casing to which they fit closely at some points. The simplest form consists of a cylindrical steel rotor inserted in a cylindrical cavity, but with its center offset from the center of the cavity. From the surface of the rotor, vanes project and maintain constant contact with the cylindrical wall of the cavity as the rotor revolves. The vanes and the wall of the cavity define volumes which are expanded or contracted as the rotor spins and the vanes respond to its offset position by folding or flexing down and then springing or rising up to maintain their contact with the cavity wall. The enclosed volumes hence vary in size from nearly zero to a maximum as the rotor revolves. Thus the effect is the production of a series of varying volumes which can be used in just the same way as the variable volume produced by the movement of a piston in a cylinder. Through valves, or better through ports, an explosive mixture may be drawn in and compressed, and then may be fired by a spark at the proper moment; output power results through the torque produced on the shaft. One has in this form an internal combustion engine; it may be either two-cycle or four-cycle. One can also omit the spark, use high compression and fuel injection, and produce a rotary diesel. In fact a steam engine or a hot-gas engine can be built to use variable volumes produced in this manner. A lot of ingenuity over the years has gone into designing dozens of forms for the rotating elements, far more complex than the simple form described here. The basic idea underlying all of them seems exceedingly attractive: no crankshafts, connecting rods, or valves; very little vibration; low construction cost. So it has seemed to two generations of inventors.

The trouble is to prevent the high-pressure gas from leaking out to where it does not belong. It is relatively easy to do so with piston and cylinder; all one has to do is to put elastic piston rings on the piston; they will press against the cylinder and make a reasonably tight seal. They will continue to do so as they wear down. Designed with care, they will not injure the cylinder wall unduly.

It is much harder to seal a rotary engine. The problem is not just that of precise control of clearances in manufacture. An engine gets hot, even if it is water-jacketed. Parts come to different temperatures. They expand unequally. The originally close clearances open up, the gas leaks, oil is pumped out, the power output drops. Or the clearances close up, metallic parts weld together, and the engine is ruined.

So inventors over the years have tried all sorts of ingenious ways to produce, in a rotary engine, the equivalent of the piston rings in conventional engines. They have tried sliding vanes, hinged pieces, plates under balanced pressures, and parts that slide in grooves. I doubt if anyone can now think up a solution that has not been advanced many years ago; most of these ideas resulted only in grief to the inventor. The other side of the coin is that many of the old schemes, with a bit of modification, care in construction, better modern materials, can be made to work. So we may at last see practical engines. They will have lots of advantages, small size and weight for a given output being an important one. Unless the problem of sealing is solved, however, there will be loss of compression, pumping of oil, and smoky exhaust. Long life and fully acceptable performance depend on the skill of modern designers, who are now hard at it.

There are extraordinary engines for driving aircraft, which do not fit into use at the slow speeds on the ground. The only contact I have had with this subject was as chairman of the National Advisory Committee for Aeronautics, before the war, and then my contact with technical matters was remote. The query is often raised why we lagged in the development of jet engines during the war, while the British, with Whittle [14] leading the advance, made great progress. One reason among many is that the application of the jet was then to fighter aircraft, and our main interest was in bombers. A more pertinent inquiry would be why the Nazis were so slow in this field. They did of course develop jet fighters, but not in time for full effectiveness. If they had had great fleets of them, they might well have stopped our bombing of their oil refineries, tank-producing plants, and railroads, which accelerated their downfall, and they might have countered our use of pattern bombing on the invasion of Normandy, the breakout from our lines there, the crossing of the Rhine. Instead, they made V-1 and V-2 weapons which, had they been available early enough, might have been a true military menace rather than a harassment of a valiant civilian population. I suppose we just put this down to Hitler's intuition.

In fact, the idea had been anticipated decades earlier. In the first world war, Charles Kettering [15] had built and tested a weapon much like the V-1. It was a true aircraft, with an aircraft engine but no pilot. It was designed to be cheap to build. Known as The Bug, it arrived too late to be of use. When the second world war struck, the device was redesigned, still to be cheap, and with a range of 200 miles and a load of 800 pounds of explosive. It was arranged to fly a preset course for a preset time, and then zero in. It was tested in December of 1941, and later was abandoned because there were no important enemy targets at a 200-mile range. There was a great difference between this and the German effort. The Kettering device was largely conventional, as to air frame and engine. Thus, every time one dropped a bomb, one also dropped an airplane. The Germans, on the other hand, developed a highly ingenious—and inexpensive—engine for their purposes. It was very inefficient and also short-lived, but that did not matter for it was expected to make only one short flight. It consisted of nothing but a chamber, open at the rear, and with a slatted valve in front. It was fueled by a combination of dilute hydrogen peroxide with a combustible such as gasoline. The fuel, injected into the chamber, gave a series of small explosions. On each one the slats closed, and the exhaust at the rear gave a forward impulse. Between pulses the slats opened to let in a fresh volume of air. The Germans planned to plaster England with some three thousand of these a day. Their effort was late, and on a much reduced scale. Finally, the V-1's became ideal targets for radar-controlled guns with proximity-fuzed shells. Fortunately these were not late.

The buzz bomb engine was a war weapon only, whereas the jet engine certainly does have peacetime uses, as our fleets of jet planes attest. For high speeds and high altitudes it is very satisfactory, and it is decidedly simple in concept. It consists primarily of a tube. Air enters at the front end, where it meets turbine blades which compress it. Then farther down the tube, fuel is injected, burns, and heats the air, which next goes through another turbine which furnishes power to drive the compressor. Finally, the heated air emerges from the rear end of the tube, providing thrust to drive the airplane, thousands of pounds of thrust. Why does this simple engine work? Simply because the hot air emerging from the tube as a jet is at a much higher velocity than the entering air, or than the plane itself. At extremely high speeds, one could even omit the turbines and just use a tube with a flame inside. This would constitute a ramjet, the simplest engine ever conceived. Very high speeds indeed would be required to make the ramjet practical, and it would work so poorly at low speeds that some other engine would be needed to get off the ground, so it has not come into use.

Hydrofoil craft also have taken much of my attention over the years. These are boats that use foils in the water to carry the load, in the same way that aircraft use foils in the air. Airfoils, wings, are large; hydrofoils are much smaller for a given load, for water is denser than air. Here is certainly a strange history, for the basic ideas are very old, and we still have no striking use of boats of that sort. Are we to assume there is something inherently wrong with the idea? I think not. Then why has it taken us fifty years either to perfect or discard it? Alexander Graham Bell [16] made a good hydrofoil in 1919 and ran it on the Bras d'Or Lake at speeds of better than seventy miles per hour. A committee of naval officers investigated and reported that Bell's claims of performance were substantiated. Then the Navy did nothing. The Canadians built and used such craft in

World War I. Today many are in use, by the Italians across the Straits of Messina, by the Russians on the Volga River. They all have a notable similarity to the one built by Bell. But general use has not occurred.

There are two varieties of hydrofoil craft. First, there is the surface-piercing type, which is what Bell used. In this the foils, placed well under the hull, rise as the craft gains speed and finally come to the surface, part in and part out of the water, and the craft thus supported skims along above the waves. Second, there is the submerged-foil type. In this the foils do not come to the surface at all; they come partway up. The craft then proceeds with its hull well above the surface and the foils well submerged, the only part projecting through the surface being thin struts to hold the foils. The first type requires no controls at all. The shape of the foils is such that if they are submerged more than normal, their lift is greatly increased, so that they automatically seek the right partial submergence. Thus the craft is held stably at the right height and on a level. The second type has no such automatic stabilization and requires fairly complex controls to hold the foils at the right submergence and to keep the hull level with the water surface.

The first type is ideal, because of its simplicity, for sheltered waters. It is not as efficient as the other type, but the difference is not serious. Sometimes the point is raised that a floating chunk of wood or a porpoise might knock a foil off. One does not need to worry about this; the foils are of rugged steel construction and would cut through any such obstacle. The only real defect of this type is that it is not adapted for high speed in rough seas. The reason is apparent. In normal full-speed operation a foil may be one-quarter submerged, perhaps less. If such a foil meets a wave in which it becomes fully submerged, its lift—the upward force it exerts—will be suddenly multiplied by a considerable factor, and the result can be disastrous. This was shown clearly when various companies offered foil assemblies to be put by youngsters on their outboard motorboats. When such a boat, running fast in smooth water, encountered the waves of a wake, it was likely to flip clear over backward. Of course this hazard could be avoided by a careful youngster, but youngsters are not notably careful.

The second type of hydrofoil craft handles rough water readily.

Its hull is above the waves, and its foils are well below them, so that it nearly ignores them. Nearly, not wholly, for the effect of waves extends below the surface, but this secondary effect is not serious with foils that remain fully submerged. When such a craft is under way it has an eerie appearance. The struts make little disturbance on the surface and the hull seems to be without means of support. One such craft, a small one which friends and I built, ran down a harbor one day—in glassy smooth water—past a freighter, on the side of which was a staging with two men on it chipping paint. One took a look and let out a yell, whereupon the other whirled and fell overboard. When we had a larger craft operating in the Chesapeake, the story went around that it was a superstructure on a submarine.

We built this second one for the Navy. It behaved very well in rough seas, that is, in seas that were of considerable height compared to its size, except when it was going to leeward—running in the same direction as the seas. I have stood on it, when ordinary craft were bobbing about, and it was as steady as standing on the dock. It was controlled by an aircraft autopilot, adapted for its new task. My friends and I were just working on the problem of following seas when the program ended. A Navy crane operator dropped his hook through the craft and sank it, whereupon the Navy cancelled the contract. Since then the Navy has devoted a lot of effort to surface-piercing hydrofoils, which it does not need, and very little effort to the submerged type, which it does need. It needs them because only the submerged-hydrofoil craft could be built to outpace the modern submarine in rough water. The reason, apparently, is that the Navy does not like complex controls, does not like to depend on them. This is strange in a service that has used very complex controls for its guns and its aircraft for over a generation.

Before I leave hydrofoils, let me make a prediction. Some day we will see great fleets of hydrofoil craft driven by sails rather than by engines. In fact the British have already built one that I consider a real advance. The sails will not be just sheets of canvas; they will be solid like the wings of an aircraft. These boats will sail faster than any craft that has ever sailed the seas.

This recital of my varied interests over the years presents a contrast which is worth examining. On the one hand we have the automobile industry where broad innovation has proved to be practically impossible. In order to support this statement one does not have to assume there are other engines which are potentially better than the ones now used; the industry has not until recently even attempted to find out whether they are or not.

On the other hand we have the pharmaceutical industry where the prosperity of the leading companies depends intimately upon the continued advent of new and better drugs and where large sums are devoted to rendering the currently used drugs obsolete.

Before coming to any conclusion, it will be well to consider an intermediate case, the electrical industry. Here indeed there are very large companies operating, General Electric, Westinghouse, RCA, and so on. Yet new companies and radically new devices continually appear, especially in the area of electronics. I participated in such innovations in my early days, and the going was certainly tough at that time. It is easier today, and there are hundreds of small companies, competing and doing well. Part of this change came about because of the pioneering spirit which was developed by a host of young men as technical innovation revolutionized the art of war, on sea and land and in the air. Part of it has come about since because of heavy government subsidy of research, for out of such research have appeared new products, which bright chaps could then commercialize.

The point is that when the product of an industry is widely diversified and highly technical, stagnation does not occur. When there is a unit product it does. We saw it in the railroads, where the stagnation was due to overregulation. We now see it in the automobile industry. What is the solution?

I have been rather rough on the top brass of the automobile industry. Let's take their point of view for a moment. Suppose a large automobile company suddenly had placed before it a fully developed and better engine, produced by some group after many years of research and development and very large expenditures. What can the head of that automobile company do? Of course he can say, "We are doing very well as we are." But suppose he decides to adopt it. He is faced with hundreds of millions of dollars in costs of further refinement of the engine, revised design, new tooling, heavy advertising. When he gets through he has a somewhat better auto to sell. And he knows he can sell an automobile that is not better. But that is not all. If he fails, he is well on the road to catastrophe. If he succeeds and proves his case, his competitors will promptly imitate him without having his pioneering costs and risks, and without these costs may undersell him. Can they do this? Will not the patent system prevent it? Not at all, for the American companies have a patent pool or understanding under which they exchange patents. They are not violating antitrust laws as they do this, for they are quite ready to license all comers. The point is that there are no newcomers to license.

A foreign competitor might do it. But there is another difficulty: The engines that might perform better in automobiles than the present gasoline engines are in general old; the patents on them expired long ago. To erect a controlling patent structure today would be difficult. Still, it may happen. There are a lot of ingenious men in the world. And the pollution problem will ultimately force us to do something definite about getting rid of the conventional internal combustion engine.

Let us look at it from another point of view for a moment. Suppose some courageous group outside the present industry found a better engine and decided to take the gamble and set up a new company on the basis of it, relying on their patent protection, and selling their cars at a higher price and less advertising cost to that section of the public, by no means negligible, which understands and glories in advanced technology. Would this group take the plunge? Probably not. And the reason is that a set of patents is often altogether too weak a reed on which to lean, unless, as in the case, for example, of Polaroid, there is a patent structure which stands almost alone.

I fear that we have to conclude that our commercial system runs into dead ends, when a few great companies make a single product and have no forceful incentive to change, when the cost of a new company's entering the field is prohibitive. To say that this is a place where government itself should enter, to produce a combined system of socialism and private industry, such as obtains in Sweden, is not the answer. To see this, one need merely observe an enlightened telephone industry, avid for technical progress, and a post office completely stagnated. But government subsidy of private efforts is something else again. We have a very lively industry in this country making instruments of a wide variety, and this has appeared largely as a result of government subsidy of research. If government just placed an order for 100,000 steam automobiles a year for its own use, subject of course to a company's meeting a set of design specifications, as government does when it buys a new type of aircraft, things would happen. For one thing, there would be some rugged conferences in Detroit. There may be better ways out of the mess we are in, but I don't see them.

To return to my first point of a man's disposing of energy; hobbies come into it, and most of what I have written above about engines appears because they have been a hobby of mine. I have had a good many hobbies in my time. The first, and the best, has been to work in my own shop. I got into it somewhat of necessity and have continued ever since. If I were not writing this book, I would be there now. My work early put me in a position where I had much to do with skilled machinists. I developed great admiration for some of the most skilled, and genuine friendship with them. Their skill, in their difficult field, was far greater than that of many of my professional colleagues in theirs. Now one can hardly discuss matters usefully with an expert on any subject unless one knows something oneself about that subject, at least something about its possibilities and limitations and its special language. So I got me a little machine shop and started to learn how to operate the tools. I am still learning. I never have gotten to be especially good at it; any first-class machinist could cut rings around me on speed and precision. But I got so I could run any machine in a well-equipped machine shop, and let me tell you brothers who take pride in hitting a golf ball with a club, that is not easy. I also got so that I continued to do it for the sheer pleasure it gave, and the relaxation. I became a pretty good cabinetmaker too. I used to tell my friends that, if my regular work went sour, I could earn a good living as a machinist, and enjoy it as much as juggling mathematics or sheets of paper.

The essence of hobbies is the fact that one does not have to take a thing seriously in order to get fun out of it. For example, I once invented a new method of painting. It came about in this way: In ordinary painting, oil on canvas, there is a limit to the contrast which can be obtained, just as a matter of simple physics, and this limit is far more restrictive than exists in nature. If one paints a sunlit scene, his range from the deep shadows to the wall bathed in sunshine is so compressed as to be only a weak, imperfect copy of the scene itself.

I was greatly intrigued by the subtle skill of artists who could suggest a far greater range than they actually had available in the reflections from a layer of paint. So I wondered what such genius could produce if given a greater range to start with.

I made a deep frame. Its front was ground glass. Inside was a white light to produce nearly uniform illumination on the glass. Then I sprayed the glass with an opaque layer of black paint, using paint that was water soluble. I could sit before this, with a brush dipped in water, and remove the layer to get just the degree of light desired at any point. Then I could use transparent colors. A nice thing about this was that, if a color was not quite right, it could be gradually modified by a weak layer of another color on top. It was not necessary to scrape off and remix.

I even painted a picture. There was a moon over a lake and a moonpath on the water, a campfire in the woods, and its light on the faces of dark figures about it. It was a terrible botch as a painting; I showed it only to close friends. But the moon was as bright as the moon should be, and the woods were really dark.

Of course one would have to run an electric cord to the bulb supplying light to any such painting on a wall. But the lighting is seldom well done on paintings, and this is no more difficult than other ways of making it right. I once went to the Tate Gallery to look again at a painting for which I had great respect. All I could see was a reflection of the group trying to view it. Glass is now made which does not have this fault so glaringly, but it is not much used.

Conventional artists do not care much about physics. There is a trick one can play which interests me in this connection. Lead a friend, with his eyes closed, up to within a few feet of a painting he has not seen. Choose a painting with some perspective in it, a rural scene for example, and choose the distance to obtain about the same visual angles as one would have in viewing the scene itself. Get him to open one eye. He will see the scene in all its perspective and depth. Then get him to open the other eye. The scene will go flat. When both eyes see the same thing they tell the brain that the object before it is a flat surface. I have done this with artists and have concluded that few of them know much about the effect, or care to.

I painted only one picture with my scheme. I have no artistic ability whatever. None of my artistic friends were interested; too artificial, probably. So I quit. But I still think that method of painting makes more sense than welding together odd shapes of sheet iron, or drawing designs in black and white with a T square.

To get back to hobbies in general, I never took to the game of golf. One reason was that it soon appeared I would be a duffer all my life. Another was that I found, as I walked around the course, I was still thinking about my business. If I wanted to walk and think, I did not need a manicured lawn to walk on. An avocation that does not take one's mind off his work is a delusion and a snare.

There has been a lot written about hobbies. They provide release when one is working intensely, and they save lives when men retire. There is nothing more pathetic than the man who retires and does not then know what to do. I have retired twice so far, and found it an excellent idea, so I plan shortly to do it a third time.

Adverse aspects of growing old, and retiring, press heavily upon us. The joints creak, the eyes grow dim, memory falters, and skills become obsolete. But there are bright compensations. One can sit in the shade and watch young men sweat under the sun. Timetables are discarded; one can look out at the snow and decide not to go to the office today. Beauty of nature, the beauty of women lose none of their benediction; they penetrate deeper into consciousness when the hot pace is over. And one of the greatest compensations is to devote one's mind and hand where one will, with a new joy in freedom to learn and create in an untrampled field.

What are the criteria for a good avocation? I believe the central characteristic is that it should be creative. But this word creative needs to be defined for our purposes. Who is to judge whether it is creative or not—it is not to be judged. That is the beauty of the avocation, before or after retirement. Over the door of the shop or garage where it is carried on there should be a sign, "My way, mine alone," or the Latin equivalent if one is likely to be visited by those who insist on Latin mottoes. Perhaps even better would be a sign carrying Dr. Samuel Johnson's shrewd counsel, "If you are idle, be not solitary; if you are solitary, be not idle." Or to be blunt, "Critics barred" would serve.

Suppose one aims to create beauty, in a vase or a painting or a piece of formed steel. People see beauty, or say they do, in strange ways. I see no objection to that, although many of the things I am supposed to gaze at with wonder seem to me silly, or clever means of taking advantage of current fads or prejudices. But when people insist, as they do, on dictating what shall by others be considered beautiful or interesting, or aesthetic, then I object. People exclaim in rapture, or its counterfeit, about a tangle of badly assembled junk, jumbled pieces of metal. I raise no complaint. I find more real beauty in a well designed and fabricated crankshaft. Let them go their ways in peace.

The privilege of the hobbyist is that he can set his own standards and tell the judges to go sell their papers elsewhere. And the joy of creation is real, even when it is admittedly small, even when it is hidden from the eyes of the cognoscenti, even when it is shared only with the humble. And man, being what evolution made him, finds deep satisfaction when he creates, and would not have his idols torn down by those who would scoff at all that is not accepted in their little worlds.

Other criteria for a worthy hobby, I believe, are that it should require work and study, have novelty, and be all-absorbing while it is being pursued. The latter is one thing I like about machine work. I defy anyone to mill a piece of steel to close dimensions and, at the same time, think about a speech he has to make. If a hobby can be mastered offhand, it cannot last. Suppose a chap is going to make what, to him, will be a beautiful bowl out of plastic. He can buy a good-looking one for a dime. He is not convinced that a thing has to be rare in order to be beautiful; he will grant that his dime bowl has beauty, even if there are a million like it. But he thinks he can beat it. He has to learn all sorts of things about how plastics behave under heat and pressure, how to color them, perhaps how to spin a sheet of plastic in a lathe, and there are certainly no handbooks on that. His rejects go into the ash can without regret. Finally he has an idea. This trick of embedding objects in blocks of clear plastic is highly attractive and well worked out. But how about a nicely formed bowl of clear plastic into the body of which have been diffused bands and clouds of delicate color? He has years of work ahead of him. He is entering new ground never before trod. And, when he finally forms the bowl of his vision, he has created new beauty. This is just an illustration; there are hundreds or thousands of such possible enterprises. But, if I were not otherwise engaged, for one thing with this book, I would make me a translucent bowl, in which intertwined threads of color would gleam in the sunshine. And no man shall judge its beauty except myself. And none shall share my joy of creation except those I love.

VII

Of Teachers and Teaching

LIFE on this earth is becoming hectic. We marvel at the rockets and spaceships. We are fascinated and mystified by computers that run machines to build more computers. We fret about a possible supersonic transport trailing supersonic booms across the nation, yet are eager for its great speed. We are staggered by the immensity of the universe as great telescopes and marvelously intricate space probes display it for us. We smile to ourselves as the cosmologists get this universe all sorted out to their satisfaction, and then find quasars which make them start all over again. The physicists build atom smashers a mile long and discover so many sub-particles that they don't know what to do with them. The logicians struggle to pin down their language so that it will finally be precise, and then question whether it means anything whatever.

We cure disease and face population explosion. We struggle to modify our legal and political systems to cope with the changes all about us, and the changes outpace our cures and leave us still tangled. We make progress, lots of progress, in nearly every intellectual field, only to find that the more we probe, the faster our field of ignorance expands. And about the human brain, that interconnected myriad of cells which does it all, we still know very little indeed. As to how to modify that brain, as it develops, how to teach it, we are still muddling along about as usual.

A comparatively tiny insignificant mass of gray matter inside a man's skull struggles with all this complexity, whether it be the surging power of the rocket, the almost inconceivable interrelations within the computer, the vast reaches of the cosmos, the transformation of the law to cope with new human relations, the evolution of systems of government as the public becomes better informed, or the transfer of a human heart from one man to another. This—the fact that man's brain does subsume all these masteries and mysteries—is the great phenomenon. Underlying it is the marvel of learning, the marvel that begins as alteration of the blob of protoplasm speeds up unbelievably when an infant is born and meets all the manifold necessities and challenges of independent life; alteration that must continue at an increasing pace as the child grows and becomes adult.

Teachers and formal teaching are not the whole of education by any means. They are a vital ingredient, perhaps the vital ingredient in the long and delicate process by which the young mind attains the ability to make informed wise choices between alternatives at any level of simplicity or sophistication. The teacher's task, whether in kindergarten or graduate school, is not primarily to impart information. It is to guide the student mind in its search for knowledge—the gathering of information, the understanding of its implications and applications, the consequent growth of knowledge, and it is to be hoped the ultimate growth of wisdom. Teaching is a high calling. Upon the devotion and the acumen with which it is carried on depend the life and well-being of the people—of this or any other nation, and thus of mankind itself. I speak heartfeltly here, for I have been a teacher to the best of my powers, and look back on that experience with pleasure, and with a humility that recognizes that I cannot hope in this essay to be definitive about the great art of the teacher. My experience has been only in colleges and in the graduate school. In this so-called field of higher education I can draw conclusions from what I have witnessed. In regard to secondary education, I have ideas and convictions, like nearly every citizen, but no basis whatever on which to speak from experience. So we deal here with higher education almost entirely.

In writing this chapter I have turned back to the memory of teachers I have known, or suffered under, to some whom I now recognize as men who had genuine influence on my life. It is easy to remember the poor ones, and to see why they were poor. And it is easy to remember a few who excelled. But it is difficult indeed to say why this was so. What follows, then, is no ordered analysis. But I feel it is worthwhile to puzzle about the subject, for good teachers are rare; we should learn how to encourage them, and, if possible, we should learn to extend to them the respect and honor they richly deserve and seldom fully experience.

I doubt if how to teach can be taught. Techniques can be taught, but not the art; and the art, or its absence, is what makes the difference between good teaching and bad. Of course there are pragmatic methods of presentation which can be imparted by a skilled practitioner to an individual or even to a class. But in the practice of teaching one exercises aptitudes which are exceedingly subtle, so subtle that detection of them by available means is extremely difficult if not impossible. Wiggly blocks are not enough. Moreover, there is the great danger that the paraphernalia of professional pedagogy, being seized upon, will become an insurmountable barrier between teacher and student. I believe, rather, that one learns about teaching by observing teachers, by being taught, by teaching, and by thinking about it. Above all, it depends upon the realization that teaching is an art, as elusive as the art of painting or sculpture, that it is an individual art, developed by each man for himself, that it must be genuine and not forced, and that its mastery can bring joy and satisfaction.

When I think of teachers who have molded my own patterns of thought I think at once of my father. I acquired much from him, although I hardly realized it at the time. He was a clergyman, and a good one. He had an interest in all the people about him, and he understood them well. This did not mean just his parishioners, and it did not mean just the upright citizens in the community. He was interested in the chap who kept the saloon down the street, and he understood him too. He had an uncanny sense of how to work with people of all sorts, and I saw him do it.

In days when it was hardly ever done, he worked hand in glove with the local Catholic priest, Father Powers, in civic affairs, such as taming saloonkeepers. Incidentally, my father, who had sailed as cook on a mackerel fisherman at the age of fourteen, worked *his* way through Tufts College by running a coal business. Students had stoves in their rooms, sometimes three flights up, and he delivered the coal on his back. Tutoring, which was how I earned much of my way through, was easier on the back muscles.

My father grew up in Provincetown at the tip of Cape Cod where his family were strict Methodists and where religious strife was intense. I remember my grandfather telling me about how one crowd wanted to build a church and brought the lumber in by ship. One of the competing Protestant faiths proceeded to burn up the lumber. They started again, brought in new lumber, and this time sat over that lumber night and day with shotguns until they got the building framed in. After it was framed in, setting it on fire would have been arson, and the church was built.

For my father to leave Provincetown and go to Tufts College to study for the ministry and become a clergyman in the Universalist Church took courage. He was not unacquainted with some of the seamy side of existence. He hence had a lot of sympathy for anybody struggling with any kind of difficulty. His sympathy was never expressed to a woman except with gentleness, or to a man except with forcibleness. He hit a hard punch when he got stirred. Yet he could temper authority when discretion demanded, as any teacher should be able to do.

He was an extraordinarily good pool player and also a very good billiard player. In fact he became so good that he had to quit, for he began to attract an audience and he feared his fame might reach some of his more rigid parishioners. A woman in his congregation had a son who was sowing wild oats and generally raising the devil. She wanted my father to talk to him, so she invited him out to dinner. The young fellow was quite contemptuous of clergymen and showed it, although he still remained formally polite. After dinner he invited my father down to play a game of pool, with apparently no idea that the minister had ever played the game. They went down to the billiard room, so called, and they set up the balls in the triangle. The minister broke them and knocked them all in. The youngster said, "I've seen miracles in my time and I'll still see more, no doubt, but I'm perfectly sober and can't understand this." My father said, "There isn't any miracle about it." He put them into the triangle again, set them up, again broke them, and put them all in.

They ended the evening without the minister's broaching a single word about the young fellow's performance or morals. They chatted about pool. When my father left, they parted as friends, but that was all. Sure enough, the youngster looked him up after a bit and they gradually got to be good friends, and finally the youngster straightened himself out largely on his own.

But the firmness was there all the time. On another occasion I learned more of its actual application. When my father was alive I joined everything in sight in Masonry. I was a Shriner and went to Shrine affairs principally because it was the only place I ever saw my father in his last years. We went together. After he died I dropped out of Masonry except for the Blue Lodge, which no one ever drops out of, and where I am a Past Master.

Masonry is a complex organization, but the Blue Lodge is its central and reasonably ancient part; all else is embroidery. I found the Blue Lodge, as many a young man did, a quiet haven where men joined in exceedingly simple social and religious association, not, however, devoid of inspiration. Beyond that there was no appeal, except that my father and I met there after our paths had widely diverged.

We went to a Shrine meeting one time, and there was a gathering beforehand. I was sitting beside a man who was either the Potentate of the Shrine at that time or a Past Potentate. They passed the drinks and my dad took one and I took one. The man beside me took a ginger ale and said to me, "I can't ever drink; that damned father of yours won't let me." I thought he was kidding me, but I found out later that that was literally true.

My dad taught me some things about public speaking which have helped me along the way. One point which has saved me many a headache was this: Never start a speech unless you have clearly in mind the sentence with which you are going to conclude. I think of this when I suffer before a speaker who doesn't know how to stop. Another point involved some interesting psychology, of an informal sort. He told me, "When you are making a speech your mind is in three parts. One is paying attention to your actual wording at the moment. Another is roaming ahead to plan what you will say next. A third is following behind, picking up slips you may have made. Suppress that third part or it will get you into trouble." I know this was true in my case when I first began to speak (whatever the formal psychologists may say). Later I suppressed the third part so fully that it no longer appeared at all. He also told me to watch some individual in the back seats. If that chap became restless, I should put more volume in my voice. If he still remained restless, I should quit. Also he told me several ways to secure emphasis on statements I especially wished to put across—changes of pace or of pitch, use of pauses, a number of others.

This subject recalls a story centered around my father that I would like to tell. He belonged to a dining club—a delightful one, thanks to the fact that they had an extensive wine cellar, bequeathed to them by a former member. During prohibition wines for dinner were conveyed, quite legally, under police guard, and the drought was thus made less oppressive to the members. One year they asked my dad to make a speech at a special dinner. He told them, however, that he had a son who was a professor and could probably give them something off their usual path. So I was invited and I made the speech. I started off just as I was expected to, telling about the current advances in physics and predicting all sorts of marvels. But after a bit, and with Dad sitting there, I began gradually to change over and to imitate Dad's language, gestures, subject matter, all of which I knew full well. Dad was the first to tumble as to what was going on. He caught my eye and then subsided so as not to give me away. Then I could see one member after another nudge his neighbor as he caught on. I ended with a peroration which was my dad all over—gestures, resonances, and all, which I could reproduce with some accuracy by that time. It was not a caricature. It was an imitation, and one that expressed my pride in my father. They liked it.

So they elected me a member, the only one of my generation. But, before I could go to another dinner with Dad, seven of the company, including him, died within a short interval, and the club dissolved. I never found out what happened to the wine.

Earlier, when I was working at the General Electric Company as a test man, my father, who was then chaplain of the Ancient and Honorable Artillery Company, went to Canada with the company on an official visit. He came back through Schenectady, and I got him to stop off for a night and visit me. When his train was about to come in, I went out through the front gate of the works and the guard there wanted to know where I was going. I told him I was going across the street to get a beer. Then I went down and met the train. To my surprise, my father got off the train in his full dress of plug hat and Prince Albert or whatever it was he wore as chaplain. At any rate, he was quite a sight. I took him back to the works and of course went through the gate without any question. The guard there merely saluted. He may have thought my companion was on the Board of Directors.

We went inside and I showed my father the test area. I remember we were walking through the main aisle of Building Sixty; coming from the other direction, hung on a crane, was a casting weighing perhaps a hundred tons, moving majestically up the aisle. My father looked at it with a little trepidation, I judged, and I said to him, "You don't need to worry about that." We kept walking along, and sure enough, the crane man moved his casting over into a side aisle until we had gone by.

I took my father to dinner that night at the boardinghouse. The boardinghouse was filled with mechanics who came directly from work, maybe fifteen of us. It was not expensive although the food was good. When I first introduced my father to the crowd you should have seen the faces around the place. They did not know what in the devil they had gotten into. But before the evening was over they were all sitting around telling jokes, the old man included, having a wonderful time. The fact that he, thus dressed, could mix with a group of mechanics and within the first hour or two get on the basis of understanding with them where all embarrassment had disappeared illustrates what I mean by genuine interest in people and concern for them as fundamental to teaching. So I was fortunate in having right at hand the opportunity to observe a true teacher in action. I was less fortunate in my beginning experience as a teacher myself.

I got into teaching by the back door, by accident and of necessity rather than by choice. My start as a pedagogue occurred because I was working for the General Electric Company soon after I graduated from Tufts, and they fired me.

I was one of their test men, at first at Schenectady, where they paid me $11.20 a week for my contribution to their affairs. I remember that when their scout came to the college he explained how we could make eleven dollars cover room, board, and so on, and a chap in the rear popped up and asked what he was supposed to do with the twenty cents. However, when I had worked about three months I discovered that, since I had a master's degree, I was supposed to get \$14 instead of \$11.20. They made up the back pay to me, but this did not help greatly, for the rest of the test men knew of the transaction, and the local beer parlor benefited. The raise did me some good indirectly, however. When, later, Test needed a foreman, they looked me up and thought from my \$14 that I had been with Test for fifteen months instead of three, so I was appointed. After that I directed twenty men or so, testing quite a lot of valuable machinery, still on \$14 a week. Since I was looking only for experience, this suited me even though it did not go far toward accumulating funds to get back to college and try for a doctorate. But, as a side benefit, I learned quite a bit about how not to run a company. I hasten to add that this was over fifty years ago, and that the company has also learned in the interval.

Some of my unintended instruction in company management occurred when I went through a strike. Around twenty thousand men were out, crowds milled about the gates, and test men were allowed through the lines to keep essential facilities running. My sympathies were entirely with the strikers. So I readily took on the task of exploring the works for them, to see whether strikebreakers were being smuggled in and the like. This fact greatly facilitated my going through picket lines manned by some thousands of husky and irritated strikers.

Then I moved to Pittsfield, for I wanted to learn about transformers. There were high voltages about, and consequently there were safety rules. One safety provision was a gate between the switchboard and the transformer, which was in a pit where it was being tested. Opening the gate shut off the power, and this was pointed out to visitors as a fine safety precaution. But the test men could readily use the adjacent gate and often did, bypassing the built-in safeguard. The test men instituted one safety precaution of their own. We had a cocky foreman who liked to operate switches. Whenever he did, someone would yell, "Mooney at the switches," and all testing stopped until he quit.

All the cables that supplied power to the test area ran from the powerhouse through a trench. One night a fire in that trench burned up the cables. The next morning they fired all the test men, or rather laid them off for a few weeks without pay, which was the same thing. I went to my rooming house where my landlady said, "What are you here for?" "Fired," said I. "You owe me three dollars," she said. So I gave her the money and took the train.

Now I had not saved much out of my $14 a week, and I needed a job. I went to the dean of my old college and told him so. He said the only job he had was one I would not want, and I told him I would take it. That is how I got into teaching.

I promptly discovered that I was to teach some elementary mathematics to a class of girl students. This had its hazards. For one thing, I was not much older than they were. I had been bossing a gang of Italians erecting transformers, and inevitably learned some of their language, and was afraid I might forget where I was and be startled into spilling some of it. Also—and here I began to get a glimmer of how to teach—they were not in the slightest degree interested in the subject, and they were right.

My predecessor on that teaching job was a competent mild English gentleman, and they had driven him out. They tried some of the same games on me. One of the best was to toss a pants button so it would land near me, then they would all watch it roll across the floor, and stare at me stonily. I told them, "All right, girls, my fun comes later," and we got along pretty well. I tried my best, without much success, to twist that course around so that it would conceivably have some interest for a group of young women, or at least have some meaning. There was no supervision over my work that I perceived, and no guidance, so I could twist all I wanted to. I told the girls I had the same opinion of the course as I found it as they did, and we would try to put some life into it if possible. There was not much life, but at least I gave them all a passing mark.

My first experience with the practice and art of teaching thus was not reassuring. It was later followed by a worse one. I was given the job of teaching physics to premedical students. The courses were somewhat absurd, the equipment was largely lacking, the assistants assigned me were stodgy, and the students were in rebellion. A previous teacher of chemistry had, so I learned, left by request under unusual circumstances. He had been lecturing to a thoroughly bored roomful, and he wandered down too close to the front row. Someone gave his coat a pull, those on the aisle added shoves, he went clear down the aisle and out the rear door, and never came back. In my first meeting with the full group, I told them that I understood their attitude, agreed more or less with it, and proposed to do something about it. I looked for a textbook of physics properly adapted for premedical students. There wasn't any. So I wrote voluminous class notes and kept one jump ahead of them. I introduced laboratory work that could conceivably have interest. For example, with scales and meter sticks, I got them to work out the stress in the tendon of an arm holding a weight, and sent emissaries into the anatomy laboratory to get the needed dimensions. I had an investigation of pulsing flow through an elastic tube. The boys responded to this sort of thing, and no harm was done when I ejected a professor of German who had arrogantly invaded my class. Our relationship was also helped when the class brought in an organ-grinder, and I listened to a tune, put a quarter in his hat, and told him to pass the hat around the class. But, all in all, it was an uphill job, the teaching remained poor, and I contributed but very little to the knowledge and skill of future physicians.

Now I recite all this not in criticism of my own college, for I have no doubt matters were fully as bad elsewhere, and not to indict the teaching profession, or the profession of educational administrators, but to indicate that much teaching, some fifty years ago, was decidedly crude. I suspect that, in spots, much teaching still is.

I realize, of course, that there were in those early days schools where the teaching was excellent, where the whole atmosphere was inspiring. I know this for I have friends who tell me so. Nor should it be concluded that my own undergraduate college experience was dull. It was not. For there were a few really great men present as I struggled, and the presence of just one great teacher can often make a great school.

If suspicion that there is too much bad teaching about has a basis in fact, we can be pretty sure that it will not last long, for we deal today in the academic world with students who are far more alive and critical than they were, and there are, at least here and there, faculties that match them. Students in the last generation or two have grown up. They are far more knowledgeable than we were in my college days and I believe also more mature. We then paid very little attention indeed to what was going on in the world. Of course we were excited about the Boston police strike when it came along, but that was later and local. In general, we paid very little attention to national affairs, and in fact no attention at all to international affairs. I graduated from college in 1913 and the looming of war in Europe was certainly a topic that you would have thought we would be discussing right and left, but this was not so. In undergraduate gatherings there was hardly any discussion whatever of that sort of thing. Now part of this no doubt was due to the fact that the faculty did not pay very much attention to it either. We had few courses that brought up anything outside the college campus. Our economics, for example, was the old-fashioned stilted economics, which was repulsive and did not really get down to brass tacks on the current scene. I do not remember taking any courses that brought up international affairs—I don't think there were any to take. In recent years I have written a number of times about the subject of engineering education, and have emphasized that the engineer needs to pay attention to men as well as to things. The engineer utilizes science to produce results that the public needs or wants. He stands between science and industry in many cases. He has to deal with men in companies, on boards of directors, in government, and the ability to do so is just as essential to his success as it is that he should be able to apply his technical skills. We had no such concept in the early days.

I write, of course, in generalities. When I was going to school there were certainly men about who had keen grasp of current political affairs, of international relations, of trends in economics. Some of these were teachers and molded the next generation. Until I was in graduate school I just did not happen to come under the influence of any of these. There were groups where men gathered and discussed deep questions, with intelligence and wisdom. I did not know of their existence until much later. It is my conviction that such opportunities are far greater today than they were then, and that the youth of today are in a far better position to grasp them.

Even after I had been a teacher for some years, we did not have much discussion of political affairs; we did not have a great deal of news in the papers or the magazines that contributed to the subject, and to what there was we paid little—too little—attention. We were still, in the colleges, among the faculty as well as the students, provincial. Times have changed since then. Today men in academic life, and citizens generally, give a lot more attention to political and international matters, very much as a result of two wars; young men in the professions and in business are beginning—just beginning, that's all—to participate directly in political activities, except of course for the lawyers, who always have been at it. Part of this is no doubt due to the fact that a political career is likely to be off and on, and a lawyer can afford to be off and on in his professional work, but most other professional men cannot. However, there is now a marked trend toward more interest and more participation on the national level. Scientists are getting into legislative and administrative public activities in a way that helps. In the affairs of the several states this is not yet sufficiently true. I think we can, perhaps, look forward to the time when we will develop in this country vigorous participation in the political affairs of the states by the citizens who have real skills and intelligence and background. If we do, we can make a revolution, and it is probably the only solution for the scandalous situations in state politics in some parts of the country.

The fact that we paid very little attention to political affairs when the twenties were young was a general failure, however, and not confined to academic people. Citizens generally in my part of the country did not pay the attention that they should have. Part of this was because New England was emerging from a period in which it had been dominated by a very aggressive and successful group of pioneers: in the China trade, in business, in setting up the copper industry, and in a dozen other ways. The next generation slacked down and had become, in general, a bunch of four percenters. I accused a group of young Boston bankers of this one time (about 1932) when I was making a speech to them—that they were cold as ice on everything except what that Man in the White House might do—and I think there was a good deal of truth in my tirade.

And do not think, because I speak of the students of today as more mature, that I overlook the present turmoil in the colleges. It is part of an unrest that is world-wide. To treat of it thoroughly would take a book of its own, and one that I am not ready to write, for I am just as bewildered as any by much that I witness. But I believe I know something about students, so I will make just a point or two on their attitude. Students have long been in rebellion in one way or another. They get to the point where they are able to take a broad look at the way in which our affairs are run, politically, economically, internationally, and they don't like it. Who does? Then they divide roughly into three groups. The great majority just accept things as they are, and more or less meekly conform. A small minority attack the system, grow beards, wear ragged clothes, start riots of one sort or another. But there is a third group, usually overlooked, who don't like what they see, propose to do something about it, but propose to accomplish this by working within the system as it stands, and thus to modify it. I believe that this third group is large, that it has as members the best thinkers, that it will be the group that will be running our affairs in the next generation. And I believe we should recognize the presence of this group and help it on its way. For here lies our hope for a better life, if these students succeed in working within the system, rather than against it, there to reform it nearer to their hearts' desire.

Am I just an incurable optimist, painting a rosy picture? Do I have any proof, any example of the presence of a deeply thoughtful body of students, who are destined to lead us some years from now? Is there, in the colleges, a body of men of high ideals, and energy to go with them, along with the plodders and the hippies?

I might write of the young people who followed McCarthy [1] in New Hampshire and elsewhere, and who should be far from discouraged, for they were an important factor in altering the foreign policy of the United States, and in the decision of a President to retire.

But I take a simpler example, one that heartened me, one that made me pause and look more deeply than just at students defying the law, usually with muddy ideas as to what they were trying to accomplish.

Fred Fassett,[2] who helped me write this book, was Dean of Residence at the Massachusetts Institute of Technology. He and his wife Julie lived on the campus, and they were highly regarded. She was not only a gracious hostess, she was often the one to whom a youngster in trouble turned. The Fassetts retired and went to live in Maine. Then Julie died. And the students–mind you, this was the students; faculty and administration were not involved–raised some thousands of dollars and built a garden in her memory. Students did not think, and act, in that way when I was young. Telling this incident, I am not speaking out of a simple liking for students. I am being realistic and looking at the whole picture, not just the sordid part of it that gets into the news.

This digression has been pretty remote from a discussion of teaching, but is nevertheless important to that discussion, for teaching, if it is to be any good at all, has to be concerned with and responsive to larger affairs.

I am often asked which teachers in my college days taught me effectively, and thus greatly influenced my later career. Unfortunately I remember a far larger number of poor teachers under whom I suffered.

For instance, one balmy June day my class was present, under duress, at a lecture in chemistry. The breeze was soft through the window, the lecturer droned on, and the class slept, with its eyes still open as required by statutes. The lecturer held a flask of something aloft. Suddenly he dropped it, or it exploded, and there was a crash. The class came suddenly awake. So did a small mouse who had been sleeping on a pipe over the lecturer's head. There was a moment of silence, while the mouse trotted home along the pipe, followed by all eyes. And this is the only thing that I remember from an extensive course in chemistry.

My teachers were of all sorts, and I shall be careful, as I proceed, not to give a basis for identifying them. There were the timid ones. These never wandered more than a foot from a textbook, ducked all questions not intimately related to the text, and as I remember them, had the sense of humor of a rhinoceros. There were the lazy ones. These never gave a thought to the subject or the students except when they were in the classroom, where they fumbled. It was quite possible, by prompting one of them, to get him to repeat the instruction of the previous class session. Closely allied were those who were working a mild racket. Their teaching was decidedly secondary in their minds; they hurried through class to get back to industry and advise. As a graduate student I had one of these whom we occasionally transformed into a really good teacher for a while by getting him mad, which was easy to do by criticizing something he had written. There were pompous teachers, who droned on and on in lectures, covering in an hour what one could readily read in ten minutes.

There were teachers who were showmen, and these were likely to be good teachers of a particular type. I remember Pete Franklin, who taught physics, not to me but to many generations of students, and did it well. To show the propagation of elastic waves in a solid he would make long sticks of molasses candy. When he hit an end of one of these with a mallet a piece of the other end would fly off. As the session ended the room would be full of chunks of candy, and the students would have learned something. He used to bring his old tomcat to class, to demonstrate how a cat, by a complicated maneuver, could turn himself over in the air. The cat, who knew what was coming, would meeow and give baleful glances. Franklin would hold him upside down by the legs a few feet off the table, let go, and the cat would come down on his feet. By his own weird contortions Franklin would then demonstrate how the cat did it.

I remember well Earle Millard teaching chemistry. Lecturing on solubilities, he would have a jar of liquid and hunt about for a piece of silk to dissolve in it. When the hunt was futile he would grasp a pair of scissors, cut off his own necktie, and use that. He had once bought a crate of defective ties for a few cents apiece.

Then, of course, there were the teachers who were just plain ignorant. The process of entering the teaching profession was so devious, so full of nepotism or old-school-tie ideas, that all sorts of chaps could slip in. No one, administration or faculty committees, ever checked up on teachers, once they were in, and the students were passive. Teaching was all too often a refuge for those who feared the outside world. A professor ruled a small region and tolerated no invaders. I remember remarking in the mid-thirties to the staff of an engineering department that, of course, any full professor in the department should be entirely able to teach any undergraduate course there given. The idea was received with horror and consternation. To ask a professor of applied mechanics to understand thermodynamics was absurd. We have recovered fairly well from such provincialism. And I do not think that, today, one will find many professors who do not know at least their own subjects fairly well. Yet, when I entered M.I.T., the dean refused to sign off my thermodynamics because, he said, the professor who had taught me the subject did not know any thermodynamics. I readily agreed, but added that I was trying to enter, not the professor. So I got in. One should not conclude from this that my undergraduate years were a washout. Quite the contrary. There were a few able, inspiring teachers about, and that is all that is needed to make a good college environment.

One of these was Professor Hooper of Tufts College (now Tufts

University). He was a dignified, impressive individual, with a fine white beard and a commanding presence, and he knew his subject. He taught a course on the mathematics of electric circuits. Viewed today, it was not very advanced mathematics, but at that time it went as far, in all probability, as any course to be had in an American college. He used a textbook, and in the summer before I was to take the course, I read it. Then I went to him in the fall, told him what I had done, and asked if I could make some time available for other things I had in mind by just taking the final examination in the course when it occurred. He gave me an examination right there and then. The questions were directed to the subject, not to the textbook. I went into the next room and worked on it. When I came back he read it. Then he touched me on the shoulder and said he would ask the faculty to give me credit at once, which they did. Now this made an enormous impression on me, and he became my idol, and the man I wanted to be like. And the reason? He took a genuine personal interest in me and showed it. I revere his memory.

But it went further than just gratitude for a leg up. I was a grubby undergraduate. He was a cultured gentleman, proud of his profession, happy in the exercise of it. Subtly, by an obscure process of osmosis, some of his idealism became embedded in my subconscious mentality. He taught me pride of scholarly accomplishment. And he never said a word to me on that subject.

The second is Billy Ransom, Professor William R. Ransom, of Tufts. He is still living as I write, ninety-odd years old, and still keen. He was a professor of mathematics. He was not a great mathematician, and knew it, but he had a homely art of teaching which was effective, especially with those who found mathematics baffling. I first got to know him in a strange way. When I was a sophomore he was holding, for the freshmen, a so-called math lab, where students could come if they wished and be coached by assistants who were picked upperclassmen. Being short on cash, I was one of those assistants. One day, in the lab, Ransom confided to me that he was in a spot. One of his assisting professors was off on a trip, another was ill, and he did not know how all the freshman sections could be taught. I offered to take one for him. He was horrified, said that I could not maintain discipline, that the front office would never approve. I told him there would be no trouble with discipline for, in those days, freshmen did not defy sophomores, and that the front office did not have to know about it. So I took the classes for a while, and all went well, except that, since the front office was kept in ignorance, I could not get paid for the work. Thus began a friendship that still endures.

The finest course I had with Ransom was on the subject of non-Euclidean geometry. This was a strange subject for an embryo engineer; it was after Minkowski and before Einstein's general relativity, but it fitted in with the latter when it came along. Ransom put it in the catalog, but I was the only student registered for it.

Professor Alexander Dillingham, later at the Naval Academy, then joined us, so there were three. We would take turns. One would go to the board and demonstrate a theorem while the other two criticized. He might start off by drawing three lines and saying, "Let this be a triangle with all of its sides parallel in pairs. We will proceed to find its area." Was this a foolish thing for a young engineer to study? It was one of the most valuable courses I ever took. Here was a subject where one depended completely on careful logical reasoning. If one followed his intuition for just an instant he was inevitably lost. We did it again on the four-dimensional vector analysis of special relativity. It was grand teaching. But do not misunderstand me; I was not a mathematician. Mathematicians are like tennis players or chess players. They can be ranked into ten or a dozen grades. A member of one grade can outperform with ease any member of the next lower grade. I was always in about the fourth or fifth echelon. Francis Bitter,[3] a shining light in the field of magnetism, and a good friend, once remarked to me that he could not understand how I had gotten along so well, since I was not at all profound. I completely agreed with him. Neither was Billy Ransom profound. But he was a good teacher.

The third teacher I wish to present was Dugald C. Jackson.[4] He was my boss, as head of the Department of Electrical Engineering at the Massachusetts Institute of Technology, when I joined the staff after I had secured my doctorate and the first world war was over. I owed him much, and I was exceedingly grateful to him, although we battled often and vigorously. He was at times a fire-eater. We used to say that, if one wished to visit him in his office, it was well to toss one's hat in first and then, if it stayed in, to follow it. He worked well only with those who traded him blow for blow, and I was one of these, perhaps thanks to a small trace of Irish in my blood.

My debt to him goes back to my graduate study days. Having been out of college for several years, and not idle on academic matters during that interregnum, I proposed to finish my work for the doctorate in one year. This was quite necessary, for I had funds for only one year, and there were few loan funds or nice teaching fellowships at that time. My thesis, which was pretty crude, was under the supervision of Professor A. E. Kennelly,[5] who was appalled when he learned that I proposed to finish in that year, considered me to be a heretic, and intended to stop what he thought was an end run. I had wisely based the theoretical treatment of my thesis on a branch of mathematics which Kennelly had never studied. When in the spring it was nearly complete, and accorded entirely with a program he had approved in the previous fall, he proceeded to add to the requirement. I appealed. Jackson supported me, and I got the degree, and promptly got married. Could a basis of gratitude ever be more valid? Once during the year Jackson, who knew my plan, called me in and told me he feared I would wreck my health if I persisted, advised me to take a second year, and told me that, if I did, he would try to find funds for me. I told him I would make a deal, that I would visit him occasionally so that he could look me over, that if he concluded I was in real danger and told me to quit, I would quit. So once in a while I would meet him in the corridor and say, "How do I look?" He would back off, look me over, and say, "All right, go to it, but do not kill yourself." As a matter of fact, I ended up in better condition than when I started. I had some troublesome rheumatism and I got rid of it. There is a reasonable chance that, if I had not, I would not have been able to get either my degree or my marriage through on schedule, or at all. I am not sure whether it was hard work, or the degree, or the marriage, which banished the rheumatism. At any rate it never came back.

But I want to write about Jackson as a teacher. One set of incidents will bring out what I have in mind. When I joined the staff, D.C. put me in charge of the first course in electrical engineering for freshmen. I had three chaps assisting me, for there were many sections. Then D.C. took over one of the sections himself. This provided plenty of material for an explosion and one promptly occurred. I issued a set of directions for the next week's work. They covered a particular technical matter on which my approach differed decidedly from the treatment given in a textbook written by D.C. himself. He called me in, told me that I was wrong, and declared that he was accordingly going to alter my directions. I told him that, as head of the department, he could remove me from charge of the course, but that as long as I was running the course I would issue the directions. I also told him that, so long as he remained teacher of one of my sections, he would follow my directions. We did not come to blows or anything like that. In fact we never mentioned the incident afterward. But this is the sort of way in which we became well acquainted —and friends. Later, when I became dean, and thus jumped over his head in the line of command, as far as one exists in an academic institution, he was delighted, boasted that he had brought me up (to a considerable extent he had), and worked with me smoothly, although with occasional sparring matches which I believe that by then we both enjoyed.

The other incident is perhaps more to the point. I was sitting quietly in my office one morning at peace with the world, thinking that the course I had been put in charge of was, after all, going pretty well, when in came a group of fifteen students or so. These were the full group of students in Jackson's section of that course. Their spokesman proceeded to tell me that, in their classes, they were learning a great deal about public utility companies and their management, but they were not learning a doggoned thing about electric circuits, the subject of the course, and that they were expected to pass a final examination. I replied that, as reasonable gentlemen, they had undoubtedly agreed, before they came in, on what they would say, what they would hope I would reply, and what they would expect me to do. Grins spread over the group as they began to realize the spot they had put me in. So I told them I had a suggestion. I told them that my section did not conflict in time with theirs, that I always welcomed visitors, that I never called the roll, but that any visitor might be surprised by being called upon as though he were a member of the class. That fixed it. For the rest of the semester I had a double section. And, as far as I know, D.C. never found out what was going on.

Now, was Jackson a good teacher? He certainly was an irregular one. He was not directly useful in a rigidly prescribed course of study. But he knew his subject, had written a good book on it. He was one of the great pioneers in the nineties when electrical engineering was in its infancy, and he was much respected in industrial circles. The basis on which I believe he was a good teacher is that he radiated an atmosphere of success; he inspired students to set their objectives high. If a teacher does that, what matters a bit of irregularity?

In fact, this criterion applies to institutions as well as to individuals. If a college or university is permeated with a spirit of success, so will its students be, and one who expects success is well on the way to attaining it. I should hasten to add that I do not write of material success only. Jackson was fully as proud of his contributions to the theories of electrical engineering as he was of being accepted as a sound consultant. The spirit which infiltrates every corner of a college should be that of success in living worthily.

My fourth example of a good teacher is going to surprise some of my friends. It is Professor F. Alexander Magoun of M.I.T., who died recently but who left M.I.T. a long time ago. The reason for the surprise is that Magoun's procedures were roundly criticized, often justly so. He was popular with his students and highly unpopular with many of his colleagues. He did not teach me in the classroom, but he taught me something just the same. To give the reason why I include him calls for a bit of digression.

In the nineties the case method of teaching was introduced into the law schools by Langdell [6] and has now become almost universally adopted.* In dealing with the mass of material for which a lawyer must have more than a detailed knowledge, must have an insight and sound judgment, this method presents the student with explicit concrete situations or cases; when he attempts to solve these under guidance he learns how to find and use precedents, and through these comes to understand some of the underlying principles that have emerged over centuries of the practice of law.

The case method has also been widely used in schools of business, where it has been successful, but not with the degree of success it has had in the law schools. Here are presented the sort of puzzles which businessmen meet in their daily activities, and solutions are

* Conant, James B., *Two Modes of Thought.* New York, Trident Press, 1964.

sought in the light of the legal, economic, financial conditions which govern.

There is also some use of the method in advanced schools of engineering, but not nearly enough. Here an explicit problem of design or construction is examined in the light of current technical practice, the constraints present in contracts, the place of research and innovation, the organization of engineering efforts.

In all this there are taught, by clear example and analysis, the methods of the professional man, his sources of knowledge, his strategy if you will. And sometimes, with it, is taught the ethics of the profession. A common content is the subject of human relations at the professional level.

For every professional man, for every man active in the complex affairs of the present day, an understanding of human relations, the acquisition of a sound and effective grasp and practice in regard to them, is fully as important as excellence in the knowledge of facts and principles applicable to the area in which he operates.

Now I hasten to state that I have far more in mind here than strategy and cleverness in relations with one's fellow men, or a grasp of psychology, normal or abnormal, or skill in propaganda or the selling of ideas or of oneself. One time I went into the office of Karl T. Compton and saw on his desk a copy of a book by Dale Carnegie on influencing people. When I called attention to it he seemed embarrassed for a moment, then he said, "The only true art of influencing people is the complete absence of art." It is something of this nature which I have in mind here.

There are three stages in the process by which a man learns to deal well with his fellow men, or fails to. The advanced stage I have dealt with above. The first stage occurs in the home and in the early schools. There are taught, or should be, the common courtesies, the respect for elders, the respect for the law, the relations between the sexes. I could write a full book on this subject, for our failure in this initial stage is one cause of many of the present troubles in our society. I will refrain and make just one point. Our teaching in the intermediate stage—our undergraduate colleges—has, in my opinion, largely failed either because it has been entirely absent or because it has treated of an idealized world which does not exist. And rebellious youth, as it has abandoned fantasy and rigidly impossible codes, has sometimes swung over to the concept of an entirely harsh, cruel, amoral world which also does not exist.

Now, in this region, between the extremes of our educational process, there is a gap: the problem of human relations after youth and before the acquisition of heavy responsibility, professional or otherwise. The youngster leaving college, or high school for that matter, has advanced far beyond the point where the simple counsel of his parents, or of his Sunday School, meets the problems he encounters, as he mixes with his fellows in humble but baffling relationships. Nor does he yet need the understanding of society, and of the relations of men who make it up, that will furnish the core of his later professional success, if his ability, perseverance, and luck take him into the advanced ranks in our social and professional organization.

Magoun attempted to fill that gap, and that is the reason I hail him. And he sought to do so by the use of the case method. He did not originate the method. It was devised, I believe, by Elliott Dunlap Smith, director of personnel of the Dennison Manufacturing Company, who subsequently adapted it to teaching students at Yale. Magoun based his method on Professor Smith's,* adding to it and elaborating in various ways. I discuss Magoun because I watched him work. He dramatized things.

Dramatized! Yes, highly so and for a reason. For a mature law student a case puzzle stated on a piece of paper is nearly good enough. I say nearly, for it teaches little about the emotional stress of a courtroom. But for a youngster in his teens it will not rivet attention. Let me give you an example of how Magoun went about that riveting.

At the opening of the class session a sheet was handed out which stated the problem. The manager of a textile plant and his assistant had been ordered by an absentee owner to fire a girl employee who was active in union organization, and they were about to interview her. Seated behind desks on the low stage were two of the students of the class, representing the manager and assistant. In came the girl. She was an actress, a friend of Magoun's who participated because she thought it would be fun. She was not disappointed. The discussion went on for a half hour, and she made the pair of students very

* See F. Alexander Magoun, *Problems in Human Engineering* (New York: Macmillan, 1932, p. xvii), and *The Teaching of Human Relations by the Case Demonstration Method* (Boston: Beacon Press, 1959, pp. x–xi).

uncomfortable indeed. It was far more intense than many an act of plays I have seen, first because it was so nearly real, and second because no one, on stage or in the audience, knew what was coming. Then there was a class session in which were discussed the errors that had been made, the opportunities missed, the crudities where there might have been an approach to mutual understanding. It included a vigorous discussion of the ethics involved. Magoun steered and kept the affair on the rails, but did little preaching. If the session failed to get anywhere, discussion was carried over to the next class session, and arguments continued in the dormitories. I could give other examples, equally dramatic.

Now this was teaching of a very high order, and Magoun was a great teacher. I state this in spite of the fact that, while Magoun taught human relations in my opinion magnificently, he was well known to get his own human relations with his colleagues in a sorry mess. He attempted to fill a gap which badly needs to be filled. It is still wide open.

The basic function of education is to ensure that the experience of one generation may be passed on to the next. It is the ability to make this transfer which distinguishes man from the beast. That ability began when the period of youthful dependence was lengthened and family structure was strengthened for protection. It stepped ahead mightily when language developed to include abstract symbolism, and when the written word supplemented speech. The expansion is still going on today as we build machines that handle data and communicate with one another.

But we can take a more restricted view as we examine why we teach in our colleges. There are two reasons. First, the practical one: so that the man educated may contribute to the public welfare to the best of his ability, and, in the process of doing so, achieve a good standard of living. Second, so that he may find pleasure and satisfaction as he does so. The respect bestowed by the public on colleges and their teachers should be in accordance with how well these criteria are met. Influence in the community, leadership in this sense, should follow on the heels of respect.

But note also that these criteria are equally applicable to the man who becomes a skilled toolmaker. His manual skills may far exceed those of his dentist. The knowledge he accumulates and on the basis of which he directs his assistants may be intricate, more so perhaps than that used by the president of the local savings bank. Moreover he may be a cultured man, in the sense that he has learned to enjoy life and to convey to his fellows appreciation which makes enjoyment possible. I grant the combination may be rare, but I think it is equally rare among bank presidents. This culture will in all probability not include Early English literature, or any other subject which has been artificially elevated into an accepted category. It might just be an appreciation of some of the beauties of nature, which he can find deeply and aesthetically appealing.

Much of the unbalance of our educational system comes from distorted ambition, not necessarily on the part of youth, often only on the part of parents. It derives of course from the age-old class system, and the barriers to transfer between social classes. This system we have by no means overcome, but we have come closer to doing so than any other nation in history. Hence we aim to educate far more of our youth for positions in an upper class than can ever arrive there: a class that leads, is affluent, has leisure—and has culture in the way in which we have long artificially defined it. I fear we will not escape this for a long time. Meanwhile we will educate many who will be frustrated and unhappy, who cannot succeed in the so-called higher levels, who cannot find a nook in the area where they think they belong. I leave to those who have studied it deeply the discussion of secondary education. But one feature attracts me greatly. That is a new type of school, at high school level, followed by a partial equivalent of college conducted by large industry for its employees. Much of this sort of advance has been made; we need more. A primary point regarding such a school is that it must embody provisions for the unusual and gifted chap to find the opportunity to rise in the organization just as readily as the chap who comes in from the conventional college.

Our difficulty resides in the vestiges of the social class system we inherited. The man who operates the controls of a steel rolling mill exercises as much skill and judgment as the man down the street who runs a brokerage office. He drives just as good a car, eats just as good food, has fully as comfortable a house. He may even be fully as cultured, in the fundamental sense of grasp and enjoyment of the world about him. Fortunately, also, his son has nearly as good a chance of making a financial success, of rising to a position of authority, as has the son of the broker. He may even have a better chance, because he may be of tougher fiber. We should make sure that he has his full opportunity for higher education to the limits of his capabilities, even if that means that the broker's son must demonstrate equal capabilities or give up his place of advantage. We are coming close to doing just this right now in many colleges and universities. But we do not, quite yet, expect to find the steelworker and the broker in the same club. And from this come, at times, distorted ambition and disappointment.

To broach the subject of what we should teach might lead us into a maze of discussion, for it is a broad subject indeed, and there are as many opinions on its details as there are teachers. I am going to touch here on one point only: the conviction that we often gloss over the fundamental and the simple aspects of a profession as we become enamored with its intellectually more provocative advanced aspects. And I am going to do this, first, by reciting a fairly recent experience with two young engineers, and second, by telling about one more good teacher I have watched in action.

A while ago I came into contact with two young engineers in a small company and watched them at work. Each had a small group of assistants under him. They had, as far as I could determine, equally good formal education, and they were both masters of their subjects. One was a success and the other was not. There was no question of integrity, loyalty, or ambition involved. But one was fully trusted by his superiors, and liked by his colleagues and assistants, and the other was not. And, since I liked both and wished them well, I tried to pin this down. It was not that one was tough and the other soft; they were about equally vigorous and self-assured. It came down to small things. One had learned to work well with men, and the other, in spite of an attractive personality, had not. I will give only one illustration. I once discussed a technical problem with these two men. Their subordinates were present. The first man brought his assistant into the discussion, and the assistant showed pride in the group operation, was anxious to support his boss. The second man, on the contrary, chided his assistant in the presence of a knowledgeable visitor. In one case sound practice had become habitual; in the other case not. The subtle matters which distinguish sound practice from its opposite should have been learned at home, in schools, in associations of all sorts. As I see it, they should also have been learned in college, both in connection with formal instruction and in extra-curricular activities. One man had so learned; the other had not. The divergence made the difference between professional success and comparative failure.

Throughout college teaching, on every subject, I fear that we often fail to implant the simple core of a discipline while we rush on to the elaboration of the more advanced aspects.

For one thing, there is little or no sense in delving into a subject and then leaving it untouched for years. In medical education a man takes a four-year liberal arts course, with work in the humanities and classics and fundamental science. Then he takes four years in medical school where such things are nearly entirely absent. Hence, after getting into practice, he does not follow up on his introduction to the humanities. He would be a better physician and citizen if he did. There are moves today to cure this trouble by shifting subjects in both directions.

Another aspect of this same problem appears in engineering education. The engineer deals with things and men. Even if he studies both in college, and in reasonable balance, as certainly is not the usual case, when he gets into practice he is going to be concerned for a while almost entirely with things. He will be designing or testing or the like, and it will be long before he is dealing with government, competition, the public. There is a hiatus here which needs to be filled.

I suspect one of the places where this hiatus is extreme is the business schools. They are likely, I fear, to teach some of the very complex aspects of business management with a whole lot of difficult economics and so on and rather ignore the fundamental stuff on the assumption that every youngster knows about it. As an actual fact, much of the failure of management is due to a mistreatment of simple elementary principles rather than to any lack of handling of complex affairs.

I believe this wrong emphasis occurs in other areas too. My own feeling is that in mathematics, for example, it is perfectly easy to teach the fundamentals of the calculus to a child. It is a good thing to do, and to do very early. But it does not pay to go into very great depth in the calculus at first. It is much better to skip around, in my opinion, from one part of mathematics to another, getting at fundamental interesting things in each place and then later beginning to dig in deeply in some one or two of them. This is for the reason that while a young person can use very fundamental parts of the calculus at once, he cannot use the more subtle parts until he gets much further along in many other subjects as well as his mathematics.

As an example of how easy it is to teach fundamental calculus, when I built the first differential analyzer, which is discussed elsewhere in this book, I had a mechanic who had in fact been hired as a draftsman and as an inexperienced one at that. He had had a high school education. He showed great aptitude and, before he got through with the program, he was managing the little group of mechanics that we had building the machine. He was assembling and maintaining it. When the Army wanted a man to aid them in the design and construction of their own machine, which they built at Aberdeen as a copy of mine, and which they used for ballistic calculations, I loaned them this man. They wanted to pay him a mechanic's wages, and I assured them that when they called in a man to consult on the construction of an important machine they had better pay him a consultant's fee. They did, very much to his surprise.

The point that I am getting at is this. I never consciously taught this man any part of the subject of differential equations; but in building that machine, managing it, he learned what differential equations were himself. He got to the point where when some professor was using the machine and got stuck—things went off-scale or something of the sort—he could discuss the problem with the user and very often find out what was wrong. It was very interesting to discuss this subject with him because he had learned the calculus in mechanical terms—a strange approach, and yet he understood it. That is, he did not understand it in any formal sense, but he understood the fundamentals; he had it under his skin.

There is another incident which shows how readily possible it is to get fundamental ideas across: I wrote a little paper a few years ago on a hydraulic machine for a man who also had a high school education, who started in as an apprentice in a machine shop. Now he has his own machine shop and is a consultant to one company on some of their problems. He is regarded by the engineers of another equally important concern as their equal, and he has his own characteristics which make him one of the best instrument designers I know of.

When I gave him this short paper, he looked it over and said he could not understand it because there was an integral sign or two in it. I said, "What the heck's the matter with you? That's perfectly simple. You just don't understand the symbols." I sat down and probably in ten minutes showed him what those particular expressions meant. He will never be troubled by similar expressions again because he has a physical feeling, a feeling of what an expression means in terms of steel and electrical flow which he can carry over into other areas.

There is a vast difference between understanding a problem in terms of equations and diagrams and understanding it in terms of copper and iron. A physicist can work out the stresses and geometry of a harness, but the farm boy understands the horse. I have known men (I have had them work for me) who were rather helpless on the mathematical analysis of circuits but who could go to a complex relay assemblage that was misbehaving and put their finger right on the fault.

So I think the fundamentals of almost any subject, the simplest part, the core, can be taught to youngsters who are just beginning to learn and can be taught to them easily. If this is done, the student who really has an interest will carry through to quite an extraordinary extent on his own. I do not think it is worthwhile in trying to do this to take the matter into subtleties which will not really come into the youngster's experience for many years. For a principle once learned is soon forgotten unless it gets exercise.

To take this discussion back into the classroom, I think that men who are inclined to be theoretical in their approach must have impressed on them the necessity of getting a barnyard grasp of things.

When I had graduate classes in rather advanced circuit theory (that is, advanced as of that day), they used to be attended by a lot of chaps whose entire approach, entire inclination, was to be mathematical. I did two things to get them in the proper frame of mind. At a class session very early in the course I would pile on the front table a collection of resistors, coils, condensers, and other such things. I would pick up a resistor and hand it to one of the fellows and say, "What's the resistance of that unit?" I would not tell him

I wanted only the order of magnitude, of course, but that was it. The device might be an iron grid. The chap who knew something about the practicality of things would tell me and some other fellow would look completely bewildered. I would do the same thing by giving one of them a coil and saying, "What's the inductance?" The chap could see roughly how many turns the coil had and what air gap was involved, and so forth, and he could get a rough idea in a moment. I did this to get the boys into the frame of mind that the practical common-sense approach has to go along with the theoretical if a lad is going to get anywhere.

Another point: We were working out all sorts of mathematical ways of examining in some detail the transients in circuits, that is, what would happen when a switch was closed or opened. I would draw a complex circuit on the board and say to someone in the class, "What will happen when I close this switch?" Three times out of four the fellow would try to get out a piece of paper and start to compute something. I would say, "No, wait a minute, look at it. What's going to happen? I don't mean to draw a curve on the chart of where the current is going to pile up, but is the current going to increase indefinitely and what's going to happen?" After a bit of that, I could get those fellows to look at the thing from the barnyard point of view.

Now another thing that I think should be emphasized to young engineers is versatility and flexibility of approach. When I was a young consulting engineer, I made a rule with myself that I would never say to anyone who wanted me to do a job, "That is out of my line." The result was that I had jobs that were to a considerable extent in metallurgy, which I had never studied. I had some that were in the nature of mechanical engineering. I had some that were strictly electrical engineering jobs. But because of that rule I got into very interesting work. And also, in competition with other young consultants, I got better fees.

In short, the essence of teaching is that simple fundamentals should not be lost in a maze of intricacy. What can we say of the one on whom the task of teaching rests, it being remembered that it is a task shared by the teacher and his pupil, but one in which the teacher must take the lead? Earlier in this discussion I have presented a few great teachers from my own experience; from specific instances to generalization is the logical next step—if indeed one can generalize about so subtle a subject.

We can discern a few conditions which any teacher worthy of the name must meet. Clearly, he must know the subject he is teaching. But this does not necessarily mean that he must be master of all its higher complexities. In fact, he may be all the better teacher if he does not know too much, especially when he is dealing with introductory courses. If he is far enough ahead of his students to treat the subject with assurance, to be unafraid of their questions, and to be able to explore with them, that is enough. But there is a corollary: He needs to stay that way. Subjects alter and advance. So he has to be a student himself, avid to learn, willing to struggle to keep well ahead.

Clearly, too, he must consciously seek to develop skill in his art. There are tricks to all trades—to teaching no less than to selling insurance or horse-trading, both arts that have considerable kinship to the teacher's art. Earle Millard's necktie, Franklin's candy and his cat, the challenge and zest that Louis Young brought into his freshman physics classes at M.I.T.—each of these devices was formed by a man who had appraised the needs of his students as well as his own special attributes, and had planned to use the latter to meet the former. Here Oppenheimer's comment is excellent: "To teach a mistake is unfortunate; to teach indifference is a crime." Teaching indifference occurs, of course, only if the teacher himself is indifferent, if he is not really and genuinely interested in the subject. It is all right to say, "I don't know," to a student or to a class provided it is said in answer to a good question and carries with it the connotation, "but let's find out." Then I think it is very salutary for a class to reach points where they realize that the professor is working into an area which is even for him a matter of exploration. I think it creates a fine spirit among a class when the professor says, "Well, now, that's a good question, but I can't give you an offhand answer. Let's see what we can work out," and starts to examine the thing in detail right before the class. This attitude is of course at the heart of nearly all good teaching in the graduate school. Much of the way in which classes regard their teacher depends upon whether they think the man is honest, or whether he is just a poser doing some kind of act.

These are all practical matters that have to be taken into account in any effort to determine the teacher's value. Practical, pragmatic, they undergird two less tangible yet even more important criteria which are interlinked, the one depending on the other, and to my mind are of equal weight in any estimate of the teacher.

The first of these—really the key criterion in an appraisal—is that he enjoy teaching, that he find active, keen, varied satisfaction in the give and take of the classroom, in the burgeoning of young minds, in the freedom and vigor of discussion, in the sharing of interest and strength in the search for knowledge. Now this criterion has some secondary aspects. For one thing, a man cannot enjoy teaching unless he likes youngsters and likes to work with them; that is, he enjoys the stimulation of young minds. Nor can a man enjoy teaching if he is afraid, and many teachers are afraid for a variety of reasons. One is that they are doubtful if they are competent in the subject and hence do not dare to open up. Another reason is that they fear taking on the load of strenuous hours that ensue if they give the class a real chance to get moving with them. The teacher who is afraid for one reason or another is incapable of helping his students master the fact that they too are often deeply afraid. Students in general do not dare to show this. Often youthful bluster is really a disguise for that fear. But the fear is there, and it is natural enough, when you consider that they contemplate the vast mass of knowledge and realize what a terrible job it is to assimilate only a small fraction thereof. No small part of the true teacher's satisfaction in his work arises as he sees youth overcome its fear.

Here we come to the allied criterion of which I spoke earlier—the second of the interlinked qualities of primary importance in our evaluation of teachers. This is the spirit of success, and the ability, the deep subtle ability, to transmit it, the mysterious characteristic which inspires to emulation. And, be it emphasized again, this by no means implies just material success, although that should be included too. My most inspiring teachers led me to seek understanding that was not worth a nickel in this world's goods. It is intangible—this spirit—somewhat an aura which the ablest teachers bring with them to lecture hall or laboratory or classroom or conference, and to which in indefinable ways the student mind and spirit respond.

The pragmatic and the philosophic considerations that we have been discussing mark the best teaching as it takes account of the fact that there is a duality today in the relations of nearly every man with his fellows. On the one hand he is usually a member of a highly integrated organization in business, even in a hospital or in organized sport. On the other hand he is in associative relation with his fellows, in clubs, neighborhood gatherings, political parties, colleges and universities. As Caryl P. Haskins has so ably pointed out,* this has come about because of the superposition of integrated pyramidal structures upon the early associative relations of men, which are their normal relations from a biological standpoint. In other words, men would naturally operate more in the pattern of the herd of caribou, or the flock of geese, than in the manner of the nest of ants or the hive of bees. But tight semi-absolute forms of organization have been superposed on this basic pattern because of their greater efficiency, in peaceful production or especially in time of war.

This duality is in our time influenced by two trends. For one, new technology–such innovations as automation in the physical sphere or computer control in the mental–moves us still more rigorously toward more interdependence and pyramidal organization for operating vast manufacturing and service functions. For the other, the very efficiency of these organizations reduces the working burden on the individual, shortens hours, and produces leisure, and thus enhances the opportunity for associative relationships.

The influence of these trends marks our civilization, sets it apart from the past. The old civilizations were in general totalitarian, in the hands of a dictator or an oligarchy, and founded on slave or serf labor. They had to be, for only thus could sustenance and defense be provided for a dense population. There were still associative relations, but for the most part only among the privileged few. Today the scene has become strikingly altered. Thanks to technological advance, a few of the population working full time, or all of the population working a relatively small part of the time, can furnish our physical needs of food, clothing, and shelter, and add a galaxy of luxuries which we do not need, but which we certainly welcome. For the first time in history, the mass has leisure. The question is, what do we do with it?

* Haskins, Caryl P., *Of Societies and Men.* New York, W. W. Norton and Co., Inc., 1951.

This has great significance as we contemplate the function of the teacher. It even furnishes a basis on which we may judge whether a teacher is truly great. Teaching is supposed to prepare youth for active life. But life today has two facets. If we concentrate on either one, and ignore the other, we fail to teach well.

The quandary here presented is now much deepened, for the body of background knowledge is enormously extended, and specialization becomes far more intense, in the pattern of ways in which one man will serve his fellows. To prepare to function effectively as a scientist, an engineer, a lawyer, a physician, demands far greater and far more exacting application and study than it did when the century began. Especially is this true of the man who aspires to reach the boundaries of professional knowledge and then to extend them. Thus there is a pressure on the teachers in any professional school to concentrate on the specific skills which their students will need for their professional careers, and thus to neglect almost everything that would increase their ability and desire to take part in those associative relations with their fellow citizens which are an equally important aspect of a full life.

But the pressure is equally great upon those teachers whose efforts are devoted to the associative relations between men, to the humanities if you will. There was a time, in England notably, when education in the liberal arts in the classic sense provided a means by which an individual could mingle to advantage with his fellows and could proceed from there to enter effectively into the topics of the day, into politics or finance or the broad problems of international relations. A man only thus prepared would find himself today unaccepted if he attempted to proceed to high levels of accomplishment in the practical world. Of course a man may fit himself for a career in music, or art, or literature, and find fruitful associative relations and deep satisfaction therein throughout his life. But I am now writing of a far more general, and more important, set of associations. And here the task of the teacher in the humanities is fully as difficult as that of the teacher who prepares students for professional specialization in order to fit into the pyramidal organization which dominates almost everywhere. It is as difficult because the terms on which associative relations are based have also ramified.

We live in a democracy. Formally, this means merely that the people elect individuals who then govern, in accordance with agreed specifications in regard to their duties and responsibilities. But it is not nearly as simple as that. The whole affair is guided, is in fact controlled, by a host of associative bodies of widely diverse sorts. These are not merely political parties or gatherings, although these are important. The associative groupings to which I refer include every means by which the public will becomes known. We are at present in the process of retiring from Vietnam, after a sad and depressing experience there. The withdrawal is not occurring because the President, or his Cabinet, or the Congress deemed it to be wise. It comes to pass because men have gotten together in all sorts of groups, some of them lawless but most of them orderly, and have made their opinions known. We are about to do something about air and water pollution, and about the slums of the cities, for exactly the same reason. In fact, in the absence of associative action, any democracy could all too readily transform itself into dictatorship by a clique. And no dictatorship can survive unless it suppresses free and informal association. The present struggle of Moscow with the satellites illustrates this.

Another aspect of the duality is a personal one for each of us. The old artisan made a finished product, a bowl or an axe, and could take pride in it. Today he runs an automatic machine which turns out one small part of a complex whole. His personal pride and satisfaction become transferred from himself to participation in a team, often an enormous one, and the transference is seldom complete. But even if it were, there is more to life than merely making something to eat or to wear or to ride in. Even in the days of the old craft guilds individual pleasure was probably seldom reached at the bench or the loom; it was far more likely to be found in the pub in the hours after labor. Most of man's satisfactions, those aspects of life which make it worth living, come from his associations, in the family, at the club, in the gathering devoted to some cause or other, rather than as an element in organization that turns out the food we need, or our shelter, or our raiment, or indeed our means of defending ourselves in a hostile world. Note, too, that there is little correlation between the satisfaction to be derived from the informal relationship and the seriousness of its purpose. The group which goes into the woods to watch birds, or which gathers to contend in a golf tournament or to discuss the latest novel, undoubtedly gets just as much fun out of it as the earnest devoted gathering which would like to force us to refrain from drinking beverages which contain alcohol. The group which gathers to play softball probably finds more joy in living than the group which seeks to impose its concept of morality upon all the rest of us.

The task of teaching in the colleges is not merely to provide students with the skills necessary for a professional career and also to prepare them for the bases on which informal collaboration with their fellows is facilitated, but to go beyond these and provide the foundations for associative relationships that may become worthy, not merely trivial, and which confer genuine satisfaction upon those who participate.

Thus we need a balance. Alongside the course in the mathematics of electric circuits we need a course in the history of ideas. And we need that balance wherever older minds seek to help younger minds on the way of life. What is true in the college classroom is true in the kindergarten as well. Every teacher, no matter his subject, needs to remember that he is preparing most of his students for a life marked by the great duality, one part as a tiny element in a complex social structure, the other in informal relations with his fellows. As he remembers this, and puts it to work in all he does, as he leads his student to be useful, but also to find joy in life, he is a great teacher.

VIII

Of Leaders and Leadership

I WAS listening to Herbert Hoover make a speech, under the trees in the Bohemian Grove. In the background near me was a young Armenian. If I ever saw adoration in a man's eyes, I saw it then. He was a waiter. He worked for a fellow Armenian who dropped all his business affairs each year and came up to the Grove to take care of the Chief, and who did so because he believed, no doubt correctly, that neither he nor his family would be alive if it had not been for Hoover's relief efforts at the time of the first war. There was a large section of the American public that thus revered Herbert Hoover. The rest of the public did not really begin to understand him until near the end.

Hoover was the first of a series of Presidents whom I had the privilege of serving in one way or another. And he was a fellow engineer, so we had a firm meeting ground.

He was a great organizer, and a go-getter in the best sense. But he was no politician as the word is usually understood, even though he became President. I think this was not because he failed to grasp the politician's arts; he just disdained them. Those arts can be genuine, legitimate, and essential for the workings of a democratic system. But they can also be despicable. There is no more sordid chapter in recent years than the attacks on Herbert Hoover by Charles Michelson [1] during the 1932 campaign. I witnessed a bit of its effect when I found myself, during the war, reporting for a time, on quite different bases, to both Hoover and Roosevelt, for it still affected Hoover's attitude. It also furnished moments when I had to be sure I remembered which one I was with.

Hoover's experience of being unfairly attacked in a cruel manner, and Roosevelt's failure to work things out with him during the interregnum, did great harm to the American people. One wonders how long it will take the public to become sufficiently perceptive so that political maneuvers that shock our sense of decency, or that injure our true interests, will certainly backfire. This sounds like a utopian ideal. I am not so sure about that; the American public are not quite so dense as they are often assumed to be, and I have seen backfires recently. At least I can testify that there is now more decency in the political scene than there was when Hoover was reviled. I suspect that part of this is due to the fact that the public can read character in faces, on the TV screen, much more accurately than they are given credit for. At least we have just had a Presidential campaign in which there were almost no personalities, and an interregnum conducted effectively. The American people seem to be growing up.

Hoover had none of the charm of Roosevelt or Churchill, none of the engaging wit of Lovett or Acheson.[2] He was deep rather than brilliant, even though his failure to grasp economic trends as the whole country went berserk in 1929 furnished the saddest chapter in his remarkable career. He wasn't alone. Roosevelt did not understand either. Witness the series of tax increases early in the great depression, when even a little economics called for just the opposite. How is it possible for a man such as Hoover to understand the ways of business—for he did and made a fortune quite legitimately by his efforts in that field—and fail to understand mass psychology and the vagaries of the public? How could he grasp so well the distress of millions during and after World War I and do something about it in magnificent fashion, and fail to meet the plight of the bonus marchers? No one has yet explained it, and it should be done, for we could all learn much if we could see why the gap was present.

One time I was fishing for bass with him, sitting in a rowboat on a pond, dangling lines over the side. Neither of us cared for that sort of fishing, but we had to be courteous to our host, who thought we liked it. We had not had a bite for an hour or so, and he was giving me a lecture on economics. It was a good lecture, although the economics he expounded was obsolescent. He was in the middle of a sentence when a bass struck his line. He got the fish under control and finished the sentence. No one else I ever knew would do that.

He was a keen fisherman, and he wanted to match his wits against the fish. Why do men like to fish? Many, undoubtedly, because they grasp the opportunity to relax and enjoy the beauty of nature. This has never been well expounded in the mountain of books on fishing, except by Lord Grey,[3] and he did it so magnificently that there is no need for anyone else to try. Still, I think there are few who fish for that reason. I know that when I am working up a nice hole in a brook a bird would have to peck me on the ear to get my attention, and a spray of blossoms would distract me only if it entangled my dry fly. We can also disregard those fishermen who try to make a contest out of it, or who find their joy in prancing into camp with a heavy basket; John Buchan[4] has pretty well taken care of their motivations. My guess is that most men who like to fish do so because they want to demonstrate to themselves that they can think like a fish, even though many generations have passed since this was an essential element of survival. And I think Herbert Hoover was thus motivated. Once a thunder shower interrupted our attention to an attractive brook and we sought refuge for an hour on an abandoned piazza. He told me some fascinating yarns about his early days as a mining engineer in Burma and elsewhere. But I do not remember that he mentioned a bird or a flower, or remarked on the pattern of drops on the quiet surface of the brook.

The way in which I met him is revealing. We were guests of William Cameron Forbes[5] on Naushon Island. And, in order to present all the implications, I need to digress to write something about Cam Forbes. He was one of the descendants of the Boston men of affairs who molded the nineteenth and much of the twentieth century.

That tribe was descended from pioneers, those who sent the clipper ships to China and the Gold Coast of California, or who brought molasses from the West Indies for making rum. Some of these old timers had been slavers, but that aspect of their activities was not emphasized. Cam's father was one of the organizers of American

Telephone and Telegraph Company, and Cam never let anyone forget it. He himself had been governor of the Philippine Islands and he never let anyone forget that either.

The tribe felt it had the right, and also the duty, to run everything in sight. There were of course members of the tribe who had no such urge, and some who were bewildered, as John P. Marquand [6] and to some extent Cleveland Amory [7] have made clear. But I am concerned with those who felt strongly that they should carry on in the spirit of their ancestors, even although some of the ancestors had not enhanced the public's welfare appreciably. They accepted into their circle outsiders whose ancestors had not done anything striking, but only after testing them to be sure they should not be thrust into outer darkness.

Now Cam was the monarch of Naushon Island, the sole trustee, who paid the bills. And all the nephews and nieces and so on kowtowed or kept out of his way. He was Chairman of the Board when I joined the Carnegie Institution of Washington, and a minor battle was needed before it became clear who was running the show. I remember one session of the Executive Committee, where I was trying to settle a matter of policy, and Cam took up all the meeting criticizing the English in my report. Fortunately I was sitting beside Walter Gifford,[8] then president of A.T.&.T., and as my steam pressure was reaching the explosion point, he whispered in my ear, "Keep your shirt on, Van; he does the same thing to me."

But don't let me give the wrong impression; Cam had skills, and he developed them. He could work a sailboat out of a crowded anchorage, with a head wind and a stiff breeze, and he could do it far better than I could, as I know, for I tended backstays for him and got well soaked in the process. He kept a fine stable of horses, played match polo until he was sixty, furnished mounts to the Harvard polo team. At his place in Georgia he drove a coach and four through narrow crooked roads. He did this to show me he could, and to impress upon me that I could not. On that occasion I had a fortunate break. Returning from a ride, my wife and Cam went into the gardens, and I rode back to the stables with the Irish stablekeeper. On the way the whole equipage became thoroughly messed up, horses all tangled up in the harnesses, facing the wrong way, rearing and snorting. I think a bee bit a horse. I stood watch to warn if Cam approached while the stablekeeper unsnarled. The next morning when Cam and I went to the stables, the Irishman paraded the horses, and one had a yellow chafe mark on his flank. When Cam yelled, "What's that?" I remarked quietly, "I wondered what that was when we were down here yesterday morning." Cam was not easily fooled, but I took him in that time. I also acquired an Irish friend for life. That friendship saved me many a grief when Cam would put me on one of his smart polo ponies—I trust just under the mistaken impression that I could ride a horse, and not under some impulse to see me dumped in the bushes.

Cam and I would come out of the Manor to where the Irishman was holding two beautiful mounts. I would greet him formally, look at the scenery, and say softly, "What am I up against?" The reply was likely to be, "For God's sake don't stick your foot out where this nag can see it." One time when we were out on a sheep drive, a young member of the clan was having trouble; his mount was rearing, coming down stiff-legged, and looked as though he might bolt. Cam said, "Doctor, I wish you would take that boy's horse. I am afraid he will get hurt." Whether he worried about the boy or the horse I do not know. He certainly didn't worry about me. So I said, "Very good, Governor," and stopped by the Irishman for a moment, who said, "The horse is all right. He just has a sore mouth." So I told the boy the Governor wanted us to exchange horses. He was at first reluctant, but I soon had him aboard my mount, then headed his toward a clump of bushes so that he would at least have to turn before bolting, and swung into the saddle. Nothing happened. I rode him the rest of the day, guiding him by knee and voice, and I never rode a better behaved horse. I never once touched the bit to his mouth.

It was in the Manor House at Naushon that Herbert Hoover and I met. It was a Sunday morning; everyone else had gone to church. I was sitting on the piazza watching the gulls, and Hoover was in the front room writing a speech. After a while he came out and said to me that he had to give the speech the next day, that there was something wrong with the last part, and would I look it over and tell him what caused the disquiet. After reading the speech carefully, I did make a suggestion. He agreed, and the speech was fixed. But no man with an inflated ego ever does that sort of thing with a younger man, and very few men who have held the greatest posts in the country would come anywhere near it. I promptly joined the ranks of the considerable company who were proud to call him "Chief."

Another incident was still more revealing. Hoover and I were chatting in a room of the Manor, probably called the parlor. Cam came in, and in my presence proceeded to take Hoover to task for violating one of Cam's many rules: smoking in the wrong room. Whereupon he left. I was obviously wishing there were a trap door I could drop through, and could not get out a word. But Hoover said, "Forget it. We both know the old man. And he can't help it." It has been said that Hoover was not a keen judge of his fellow man. He certainly was in that instance. For one thing, he created in me a devotion which never left me.

So much has been written about Franklin Roosevelt that I hesitate to add more. But there are one or two incidents that illuminate an aspect of his character which has received little attention. He was a master of the dramatic at times, but he was also, responsibilities aside, essentially simple in his relations with friends and family.

My wife and I went to a state dinner for the Queen of Holland. The gold service was out, and all the uniforms were spick and span and the evening gowns were of course stunning, although I cannot remember ever having been personally stunned by a gown. There were about forty couples and a great oval table. As the dinner ended Mrs. Roosevelt attempted to break it up. But the President was deeply involved in a discussion with the two ladies at his sides. Did she call a waiter and send a message? Not at all. She said, loudly enough to penetrate the conversation, "Yoo hoo, Franklin," and that did it.

The ladies left, and the men gathered in knots of six or eight. The group around the President kept changing, for no one would appear to monopolize that position. Then the President started to rise. There was complete silence, all came to attention, and the group about him drew back. He slowly snapped the iron braces into position on his legs, braced himself on the arms of the chair, slowly rose, and then took the arm of his aide and withdrew. Here was the most powerful man in the world, surrounded by powerful men, with a severe physical handicap which he could disregard or which he could use to give an unequaled dramatic touch.

One time when the submarine war on shipping was critical but the tide was slowly beginning to turn, I was reporting to him on the subject and was answering his questions, which were very much to the point. The door of his office opened, and in came Mrs. Roosevelt who had been away on a short trip. She went around behind his chair, kissed him, greeted me by my nickname (which nearly everyone uses, presumably because they cannot pronounce my full name), sat down across the room and said, "Go ahead, Franklin, we have plenty of time." So we continued our discussion. Once in a while she would ask a question, and the President would turn and explain the point to her. And I came out wondering if people would ever believe that great men, carrying enormous burdens, would thus exemplify what is most heartening, and least recognized, in happy relations between husband and wife. I am convinced that the greater men are, greater in the best sense, the more simple are their relations likely to be, the more wholesome, in their homes and with their real friends.

This quality in President Roosevelt—this quality of warm-heartedness—ran through other experiences that I knew about. For instance, well along in the war, Niels Bohr, the great Danish physicist who was in this country doing what he could to help with the scientific war effort, became convinced that the United States and Great Britain should inform the world about the atomic bomb and thus head off a possible arms race later on. He wanted to see the President and discuss the idea with him. He explained his desire to Tolman, and Tolman told me the problem he had before him. The trouble was that, while Bohr could discuss physics with physicists in masterly fashion, once he undertook discussion outside his normal field he was very hard to follow. We had to bear in mind, too, that the President was a very heavily burdened man. Even so, one day when I saw the President I said I hoped he would see Professor Bohr for a few minutes, explaining who he was and what an eminent scientist he was. "He is very anxious to see you," said I, "and I think it would help toward good will if you would talk with him." The President said, "What's he want to see me about?" And I said, "He has a plan regarding ending the war, and peace terms." "Do you think I will be able to understand him?" F.D.R. asked. And I said, "No, I do not think you probably will." "Never mind," said the President, "I will talk with him." Bohr in time had his say and went away happy. Later I learned that Felix Frankfurter also had sought such a meeting for Bohr. I am sure that the President treated Bohr with the utmost courtesy, and I doubt that the President really understood him at all. But the important fact is that, with all the load he carried, President Roosevelt had time and energy for such a right and friendly act.

I think of another incident, trivial in its way but with a lot of meaning for me. One of the troubles with life in Washington during the war was that air-conditioning raised the very devil with anyone who had to move about the city. I had offices in the Joint Chiefs' building and in the Carnegie Institution of Washington building; I was often in the Pentagon, sometimes up on the Hill; and O.S.R.D. was spread through several buildings—with the result that I was likely to be all over the city. When I was, I was in and out of air conditioning—out into 110 degree heat in a car, and in to 70 degrees in an office—and it was trying indeed. I do not think enough attention is paid to this. It seems to me it puts quite a strain on the human system to make that kind of abrupt transition a dozen times a day.

One day I was due at the White House, and I got so busy that I did not realize, until it was too late, that I had thoroughly sweated through the summer suit I had on. It was a queer white thing, one of those wrinkled summer suits—seersucker. By the time I started for the White House, I was pretty damp and rumpled. But I went right ahead, and when I went in to see F.D.R., he took a good look at me and got a great laugh out of the fact that I was thoroughly soaked. Then he said, "How do you like the temperature in here?" "Since you asked me, Mr. President," I said, "I think it is too damned cold." "Is it?" "Yes," I said, "you freeze your visitors."

A few weeks later I went again to see him, and his first words were, "Well, Van, how do you like the temperature in here *now?*" The remarkable thing about this is that F.D.R., with all that he had on his mind at the time, could be enough concerned with how he handled his visitors to change the temperature in his quarters. He was a kindly individual at heart, even if he did at times undoubtedly put a subordinate on the spot for the fun of it.

A far cry, but consistent in character, is another incident. During the development of the atomic bomb, I reported regularly to the President, told him of the schedule on the bomb, our estimates of enemy progress, the costs of the program, and so on. Of course I never told him that we were sure we could produce it, and I never told him that we were sure we could stick to the schedule. One day I said to him, "You know, sir, if we do not get this thing by the end of the war, we may be in a tough spot," having in mind that it would be fully as serious for him as for me. "Well," he said, "we must get it, because when we come to the end of the war, the country will relax, as it did at the end of the first war; if the program is continued at all, it will be cut way back." And he said, "If this bomb is going to be what you tell me, it had better come into existence in our hands." Now I am not quoting him verbatim, as I made no record of that conversation, but I am sure that was the general tenor of it. F.D.R. took quite a chance in backing me up, and in backing up the whole Manhattan District program, for that matter. But I think it is obvious he also considered the bomb to be inevitable at *some* time, and that for the sake of the free world it must certainly appear in our hands, and not in hostile hands. I still shudder when I think what sort of a world it would have been if we had quit, and Russia had completed the job.

I learned early that when the President asked me a question I had better answer it. One day he asked me about a matter that was way out of my field, and I tried to tell him so. All I got was, "Never mind that, you answer my question." So I said, "Yes, sir," and I did. I suppose he did this with many of his visitors and that this was an effective, but dangerous, way of learning what was going on. Every President needs some way of finding out about things, in addition to official channels, a kennel of bird dogs or the equivalent. Roosevelt had many such informal channels, Truman a few and rather ineffective ones. Eisenhower, unfortunately, had very few, and we all suffered from that lack.

My relations to President Roosevelt on scientific and technical subjects were primarily as an intermediary. If he asked me a question about weapons or something of the sort, I would answer it on the spot to the best of my ability. Then I would turn to the best experts on that particular question, ask them to analyze the situation and post me. Thus I would transmit to the President their opinions, not mine, for *they* were the experts in the field. Of course, I did not go back unless I had found that my own first statement had been incorrect or incomplete. But I always took the point of view that one of my jobs was to bring to bear the best scientific judgment that could be found on any point in which the President was interested. That is the situation that obtained under Killian,[9] Kistiakowsky, Wiesner,[10] and

Hornig,[11] during their terms as scientific advisers to the President, and continues now under DuBridge.

The men who served as Winston Churchill's advisers on scientific matters—notably Professor Frederick A. Lindemann, who became Lord Cherwell—had a far more difficult job. Churchill treated Cherwell as his personal scientific adviser and wanted his personal opinions. I think that this was primarily Churchill's fault, yet I do not believe that Cherwell could have gotten the opinions from the experts readily in any case, for many just did not like him. I know how badly they thought of him because one night out at Oxford in one of the colleges some of the dons recited to me a poem about Cherwell which was scandalous to say the least. I liked Sir Henry Tizard very much indeed and worked with him closely as British-American interchange on weapons began. He did a magnificent job in getting these relations onto sound ground. I was very much interested by Cherwell, and with both these men I had pleasant relationships. There has been a good deal of talk about the battle between them; this I know—no matter if they did have a personal feud, neither would have allowed it to stand between him and doing what he felt to be his best duty toward Britain in the war.

The British organization, as I have said, was very different from ours. It did not head up anywhere in the civilian area, as ours did, so that the adviser to Churchill was in a very different position. He did not have an organization under him that encompassed the whole range of activities. In other words, the British scientific effort was split up among a whole lot of committees. How the British ever made this work I never could understand. They *did* make it work, but only Britishers could have done it.

The relations of Cherwell to Churchill thus were very different from mine to the President of the United States. F.D.R. never interfered. He was interested in all aspects of what was going on, but never did he tell me that I was to do this or that in regard to any item, and he never took steps in regard to them. Moreover, although there were a few attempts to short-circuit me, to get to F.D.R. with a pet scheme without going through channels, they never succeeded in confusing relations.

Churchill, on the other hand, butted in on all sorts of technical matters. He, of course, had an ego that has never been matched anywhere. Just as he did not hesitate to represent himself as an expert on military strategy and tactics, he did not hesitate to consider himself an expert on the application of science to weapons. Cherwell, in the position of carrying out Churchill's orders, was bound to be in conflict with all sorts of committees and individuals; and he was.

This was apparent when I was in England on antisubmarine matters in 1942. Churchill called a meeting of the antisubmarine committee of the Cabinet, so-called, at which he presided. I imagine there were twenty men there in the Cabinet Room at 10 Downing Street—the First Lord of the Admiralty and the Air Chief Marshal and many others around the table. The Americans present were Averell Harriman,[12] Admiral Stark,[13] and myself. Of the three, I was the only one thoroughly posted on the antisubmarine technical situation as it was developing. Stark, I believe, was not abreast of the most recent developments or plans, but he was most helpful, as he had been earlier, in regard to my relations with the Navy. When the Navy had balked and refused to give N.D.R.C. the information it needed to proceed on antisubmarine devices, Stark, then Chief of Naval Operations, called a meeting—I was alone and there were six or eight admirals. I made no argument, simply stated they had the orders of their Commander in Chief and I assumed they would follow them. After some discussion, Stark told them bluntly to turn over such information as was needed. Even then the Navy held back for a long time.

During the Downing Street conference, Churchill twice took a crack at Cherwell. I have always thought he did this primarily to show me, and perhaps Harriman and Stark, how he handled his scientific crew. At any rate, he was decidedly rough on Cherwell, and Cherwell never batted an eye. Apparently he was used to it, and I judge the rest of the British were too. If Roosevelt had ever landed on me like that, I would have been looking for an exit. But Roosevelt treated me with courtesy—even when he was aroused about some matter.

Later on there was another session at 10 Downing Street which was quite an affair. This was in 1943 when the atomic energy effort was full steam ahead, and I knew before I went over that there probably would be some quarrel with the British over our system of interchange of information. Before I left the United States, therefore, I went to see the President, said that I thought this subject of interchange would come up, and asked for his instructions in case it should. As usual in such circumstances, I got no instructions whatever.

The system was this: The O.S.R.D. and the Manhattan District had both provided in their rules that any man engaged in the development of a project was entitled to all the secret information he needed, of any sort, to carry that project forward. On the other hand, he was not entitled to information which was not thus needed. In the field of atomic energy, this meant that the British were brought into consultation, and given full information, on any phase of activities in which they were also engaged. If they were doing fundamental physics or fundamental chemistry on a particular phase of the subject, then they were brought into it. But if they were not working on a particular phase, they were not brought in. This made sense. Groves was administering the Manhattan District, of course, and the rule was fully enforced. But the British, or some of them, objected. They also avoided the rule as far as they could. If it had been rigorously abided by, the Fuchs affair [14] would have been less damaging, and Russia would not have built an atomic bomb quite so soon.

When I got to London on this occasion, I went in to pay my respects to the Prime Minister. The Minister for Air, Sir Stafford Cripps,[15] took me in, and I expected the proceedings would take thirty seconds. But Churchill spent ten or fifteen minutes bawling me out on the interchange affair; it was unfair, it was unreasonable, it did not make sense, he did not like the arrangement, and he damned well did not like me. He was sitting at the table in the Cabinet Room and he had a cigar he was trying to light. I judge he had not bitten the end off it, and he kept throwing burned matches over his shoulder in the direction of the fireplace. He kept the First Lord of the Admiralty and somebody else waiting in the anteroom while he hopped all over my frame. I said nothing. When at last he had come to the end of his tirade, I said simply, "The American atomic energy development is now under the Army. The Secretary of War of the United States is in London, and I certainly do not propose to discuss this subject in his absence." "Very well," said Churchill, "we will have a full-dress discussion." Thus ended that particular session. But during the time Churchill had been jumping on me, he had given away most of his arguments, and on these I had two or three days to check up and get posted. When the full-dress session did occur, I was in shape.

The talk was almost entirely between Churchill and myself. Lord Cherwell and Sir John Anderson [16] were there and, of course, Secretary Stimson and Harvey Bundy. Before we went to the meeting, Mr. Stimson and I had a talk about what we were facing, and I put the case strongly for the American position. He contested it, tried to pick holes in it, and went after me on it. When I put up one argument, he said, "That's the argument of a police court lawyer." But as we were starting into 10 Downing Street he said to me, "Van, I want you to handle this matter." I said, "You mean, sir, that I am to handle it in accordance with my ideas of how it should be done?" "Of course," said he. What Mr. Stimson had been doing was to make sure that I had my arguments in order.

Churchill started the session with a harangue following much the lines as when he had had his private session with me. I was ready for it this time. Moreover, I was not happy over the way he had gone at me. One of the first things he said was, "Now you understand, I am interested only in fighting this war; I am not interested at this time in any question of the postwar affairs of Great Britain, and specifically not interested in atomic energy for peaceful purposes at this time." I said, "If that is so, sir, how does it happen that your representative on this subject in the United States for some time has been an engineer of Imperial Chemical Industries?" Churchill looked at Sir John Anderson and Cherwell to get a negative, did not get one, and went on to the next point. He said in regard to one detail of the program, "Now we developed this particular method of construction and then we turned it over to you Americans to manufacture." And I said, "Well, sir, I have before me the records on that particular point, and I find that the British advice on it began only after we had it on stream." After two or three occasions like that, he stopped and looked at the ceiling. There was a long pause, and then he said, "I will make you a proposition."

He proceeded to state what was practically the American position and the American plan—in very different words from those usually used, but that was it. Mr. Stimson and I said we felt sure the President would be in agreement, and with that the session ended. In other words, finding that he had been given incomplete and incorrect information, probably through his own fault, and finding that he had thus taken himself out on a limb, Churchill abandoned his contention and conceded the position of the opposition. This, very likely, was characteristic of him. But I came out of there quite sure that, if I had had him as my chief, I would not have lasted long.

Sir John Anderson and I were now detailed to put this agreement in form. Sir John came over to the United States; we worked on it and formalized it. It originally had some clauses in it that were not concerned with development but with political matters. I, of course, said to him that I had no authority to agree on the political clauses. He took this up with Mr. Stimson, who said the same thing. These clauses were largely left out, although I believe one of them somehow got into the document at Quebec.

During the time between the first session and the full-dress session, F.D.R. wrote me a letter. It went to 1530 P Street in Washington and there Conant received it. He cabled its contents to me in London. In going through coding and decoding it got more than a little mixed up. When I received it and read it, I said, "Well, this simply tells me to do what I am now doing." So I did not alter my course. Not until I got back to the United States and saw F.D.R.'s original letter did I find out that it, in effect, had ordered me to accede to the British position. I do not know whether Churchill talked to F.D.R., or sent him a message on the matter, but I did adhere to the American position. Churchill finally accepted it, and it was approved at Quebec. F.D.R. did not take me to Quebec, although the interchange agreement was one of the items that was approved there.

Moreover, F.D.R. never again mentioned this subject of interchange to me. I have always felt that he just did not care to bring up the fact that he had been persuaded by Churchill to take a stand that would have been very unfortunate. So naturally I did not mention it either. But F.D.R. had ordered me to make practically full interchange, and such a course would have made all sorts of trouble for him after the war, had he lived. The Senate could and probably would have raised hell about it. The only reason for acceding to the British position would have been to aid them in their postwar use of atomic energy. This was not a war matter, and thus F.D.R.'s broad war powers did not include it. So F.D.R. accepted what happened at that meeting, and said nothing.

I saw Churchill only those two times during the war. I did not see him again until he came over to the Massachusetts Institute of Technology at the time of the big celebration in 1949, when he was a guest of honor, and in quite a little group I was introduced to him. He met me with all indications that he had never seen me before, had never heard of me. I feel quite sure that he knew me the instant he saw me, unless his memory had become very bad. Because, for one thing, when we had finished our wartime full-dress session and were about to leave, he shook hands with me, and after he got hold of my hand he pushed me around a bit, looked me in the eye, and said, "I want to see some more of you," meaning, I think, "God help you if I ever catch you out on a limb." But when he did see me again, he did not know me. This perhaps was just the fact that he had seen a thousand men and his memory had its limits. I rather suspect that he did not want to take the chance of bringing up the subject of our last meeting, in the light of the Fuchs episode.

Some while after that session in London, I was lunching with Secretary Stimson in the dining room of the Joint Chiefs of Staff. Vigorous conversations were going on all over that room: The Dukw had just been very successful in Sicily. Mr. Stimson said something about it and wanted to know who it was in the Army who was primarily responsible for the success. He knew it was my organization that did the development. I told him, "General Devers tested it under conditions where he probably exceeded his orders, and Tony McAuliffe encouraged it. But," I said, "officially, the Army opposed its development and your representative on N.D.R.C. voted against it." He said, "Who was that?" And I said, "General Williams,[17] but he acted under orders." He said, "Why do you tell me all this?" And I said, "Because it is your Army." He said, "It seems to me you are kind of rough about it." So I said, "Well, Mr. Secretary, you must remember that it is only about five weeks ago in London that you called me a 'police court lawyer.' " We left it at that. But I did get a chance to have some of the Joint Chiefs' personnel overhear this conversation and I hoped they would pass it around.

The contrast in operations between Churchill and President Roosevelt seems to me to be an accurate reflection of their different personal qualities. Each was devoted to duty, each was vigorous and hard-driving; one could delegate responsibility and then refrain from arbitrary interference, and the other could not. I imagine it was to this ability to delegate that Roosevelt owed the equanimity that was part of his greatness, even though we do have to admit that sometimes he got his delegations confused and gave essentially the same task to two men at the same time. I am inclined to think that, occasionally, he did this on purpose.

This does not mean that he did not get stirred up at times. Once I went in to him with a sheet of paper on which I sought an "OK—FDR." It was an important move, I remember, something to do with the atomic energy program, on which I had to be very sure he agreed. But he had just seen de Gaulle, or had received some word from him. The Grand Charles had a way of annoying his fellow men which was superb in its way, and which he has apparently been perfecting ever since. The President told me about it—in detail, with gestures. In fact he continued to do so until I had to leave, with my little paper still untouched.

I always tried to come out of that office still in possession of any paper or report I took in, and there was good reason for this: I did not trust the White House handling of such documents, not that there was anything sinister about it—just confusion. Once I missed, and, as it later appeared, so at the same time did General Marshall who left behind a report of his, also without question full of dynamite. For several days I kept telephoning but could not locate my report. Finally the White House called and said, with much relief, that they had found it. Up came a young lieutenant with a sealed envelope and in it a report. Fortunately I opened it at once, and it was Marshall's report! So I promptly sealed it again and dictated a memorandum in which I carefully recited that I had merely looked at the title page. I got the lieutenant to witness this, and sent him back. Then I called Marshall's aide and told him I could tell him where the general's report was located, much to the expressed relief of the aide. I told him it was in the hands of a lieutenant, in a car, en route to the White House. My own report had gone to the general, so that got back to its home also. But this episode caused me to hang onto reports with still more tenacity.

It was a pleasant thing to watch Secretary Stimson and President Roosevelt together. Mr. Stimson paid the President all the deference due his office. And F.D.R. treated Mr. Stimson with all the respect due his long career as a statesman. But they talked about matters directly and frankly, so everything was as it should be. Mr. Stimson and I often went in together to see the President, and one time I had a really important report, for the atomic energy program was nearing its climax, and the last page of the report had several definite recommendations. The President listened and nodded, said he would read the report carefully, but I got no "OK–FDR." So I asked if I should not come back when he had had time to read it, to answer questions. In fact I tried one or two other mild prods. Finally the Secretary said, "Mr. President, what this damn Yankee means is he wants you to read it now." The President grinned, and after a while I came out with my "OK–FDR" and the report.

Mr. Stimson had many younger men about him; he liked to work with young men. But, almost entirely, he dealt with men who were members of his own vast organization, or with other Cabinet members, so that the relations were bound to be a bit formal. On the other hand, I was entirely independent, and with me he could relax. He treated me as he would a son. We became friends, he with kindliness, I with fitting respect. But this did not prevent me from putting in a light note once in a while, and he enjoyed it. There were not many humorous moments in his daily life.

Many of the men in the highest posts during the war, as I shall illustrate later, took the point of view that scientific or highly technical problems were not their concern or responsibility, and that they had plenty to do without trying to grasp the solutions. Not so the elderly Secretary. I remember one day showing him a photograph of an early test of a new sort of smoke generator. A great cloud of smoke was rolling down a valley from the machine. Alongside it the output of one of the old types looked as though someone had dropped a cigarette in the grass. The Secretary was, at that time, much worried about a possible air attack on the Panama Canal, and this seemed to him to provide a needed link in defense. I tried to tell him that the device was in an early experimental stage, that it had to be tested under a variety of conditions, that it would take time to tool up for production. But it was of no use. The power of the Secretary's office was brought to bear to get such smoke generators immediately. And the power of the procurement people went after my hide. I was more careful after that.

We kept reasonably good relations with procurement in spite of our opposing interests. General Somervell, head of the Service of

Supply, once said to me, "Bush, you and your crew are a damned nuisance." Of course I explained to him that, when we ceased to be, we would not be doing our job. As the Dukw, the amazing amphibious vehicle of which I have spoken earlier, approached the point of procurement, he told me that the Army did not want them, and would not use them if they got them. But they got them nevertheless, and they certainly used them, in the landings in Sicily and Normandy and elsewhere.

Secretary Stimson operated at times by bursts of righteous indignation. When he was really on a tear clerks got under their desks and generals trembled. One day he landed on me with both feet when I went in to see him; I forget what I had done to arouse his ire. I sat and smiled. Suddenly his expression changed, and I could see him say to himself, "This man is not in my outfit." He stopped in the middle of a sentence, there was a pause, and then he went ahead on an entirely different subject, and in an entirely different tone of voice.

There were no big scandals over procurement during World War II. To anyone who can remember the Spanish War, or even World War I, this fact was heartening. I have no doubt that the Secretary's violent reaction when he suspected chicanery seeped down through the whole organization so that even the sergeant in a PX hesitated to slip a carton of cigarettes to his buddies. The fact that Mr. Stimson made it clear that he controlled the Army, aggressively, not from an armchair, was, I know, at times a trial to General Marshall. Yet their relations remained excellent throughout. This condition was partly due to Marshall's caliber as a leader; it was partly due to Mr. Stimson's wisdom. But much also was due to the fact that the Secretary never interfered with military judgments, and confined his acts to ensuring that those judgments were being made by the ablest men that could be found.

Probably the most disagreeable task I had during the war was to prevent the Army and Selective Service from taking key men out of the laboratories and putting them into uniform. My toughest antagonist on this subject was Lucius D. Clay.[18] He just could not understand how any sane man could struggle to keep young men out of the Army. I could not understand how he could miss the fact that the whole practice of warfare was being revised by the laboratories. So we went at it, in gentlemanly fashion, but nonetheless vigorously.

I remember one meeting at which, under orders from the Secretary, he presented the Army position, which was far from his own point of view. He did so accurately and fully. Then, when he sat down beside me, I got his own reactions emphatically in my ear, along with the warning that he had just started to get his way. Yet, since each of us respected the other's sincerity, we became friends. After the war, when he was sent to Germany as High Commissioner, I wrote him a letter in which I said I knew of no man in the Army in whom I had more confidence for that difficult task. He dropped around to my office to thank me. His achievement more than justified the confidence of all his friends. Among his accomplishments is the fact that he was responsible for the airlift which broke the Russian blockade on Berlin. Not only did this save Berlin for the Western world; it probably saved West Germany itself, and quite probably prevented a real war.

If the efforts to move men from the laboratories had been successful, there would have been no short-wavelength radar, no proximity fuze. There would still have been an A-bomb, for General Groves, with Mr. Stimson's support, and with the drive that Groves could exert, could protect his organization against all comers. There was a cry for equality of sacrifice, with of course exception for organized labor, to replace a policy of placing men where they could best contribute. Sadly, among senior officers, there were still remnants of the conviction that all wars would be fought with the weapons which existed at the beginning of the war.

One day I met Mr. Stimson on the Pentagon steps and walked up to his office with him. He asked me how things were going, and I told him as usual, Budget was trying to control my funds, and the Army was trying to swipe my men, but otherwise things were in order. He said, "Now, Van, when you have a really tough problem with the Army, you bring it to me." However, I explained to him that this would not do, that my people and his were working together, or contesting with one another, at all levels, and the atmosphere, generally excellent, would be polluted if I engaged in end runs. He understood and we parted.

Some weeks later a really dangerous situation arose. A new regulation was promulgated under which men engaged in research or development would be exempt from draft only if engaged on a project certified by Army or Navy. I immediately insisted that as head of O.S.R.D. I also have authority to certify, and I got nowhere. This was a critical matter; all sorts of things could be abruptly terminated by such a regulation. Military officers usually became convinced of the value of a development only when it had been fully demonstrated, and sometimes not even then, and the officers involved in manpower requirements were hardly the ones that should judge such matters. I did not go to the Secretary, but I certainly went all over the Pentagon. My language was unconventional; I probably insulted a few.

By the end of a day or two a group of officers, who thought I would go to the Secretary, and did not know I would not, aimed to precede me there. Apparently they told the Secretary I had gone berserk, that I did not make sense. He asked, "What does he want?" "Why," they told him, as I got the story later, "a perfect absurdity; he wants a ruling that, when he certifies a project, the Army certification will be automatic." "That seems to be perfectly reasonable," said the Secretary. "We will do it that way." That afternoon I had a group of officers in my office proposing a compromise. Naturally I knew that when that particular band of officers softened up there was good reason, so I was stiff and unreasonable and got what I wanted. Mr. Stimson was a wise man. Incidentally, much of the harmonious relations which finally developed between O.S.R.D. and the Services was due to the fact that it was widely known that I could take an appeal directly to the President or to the Secretary and it was also known that I never did.

Once in a while Mr. Stimson would send me word that he wanted to talk over a matter with me, and would ask me to drop around to his house after dinner. My wife and I would go. The Secretary would take me to his study, leaving the ladies to chat. It would not take us long to discuss the problem; there usually wasn't any. Then we four would sit around and talk about Mr. Stimson's early experiences, delve into albums, and discuss anything but the war. This was the way in which he relaxed. He could not do it quite this way with someone who reported to him.

He used to indulge in exercise, under the impression that it kept him in good physical condition. I have always preferred the theory that the older a man gets, the less he needs exercise, and that excess is dangerous. This is a much more comfortable point of view. But Mr.

Stimson played deck tennis. One day he met me in the corridor and invited me to join him. "I'm sorry, Mr. Secretary [I never called him anything else], I have an important appointment at that hour." He pointed his finger at me and said, "You're afraid I'll trim you." "I know damned well you'd trim me," I said, "but that isn't the reason I won't play." "What is the reason?" "Well, Mr. Secretary, if you will be as careful as I am and refrain from these dissipations such as deck tennis, then when you get to be as old as I am, you will be in my obviously fine physical condition." He loved it, probably because he saw no one, day after day, who dared to joke with him.

There is another story, and it is a sad one. Some weeks before President Roosevelt died I received a letter from him. From its nature, and from the shakiness of the signature, I knew he had not dictated it, and I doubted if he had read it before signing. It was also evident that Mr. Stimson had a similar letter. So I went to the Secretary's office. After a brief greeting I laid my letter before him and said nothing. After a bit he took a letter from the drawer of his desk and placed the two side by side. After a long pause he looked up and said, "Van, I hope we are not in for another Wilson episode." I just bowed. We shook hands solemnly and I left.

The day Mr. Stimson left Washington, to retire on Long Island, he took me to a Cabinet meeting with him and then we lunched together. I told him that, as an older man, I felt I had a duty to give him some advice. So I spoke of the inevitable reaction from dropping a heavy load suddenly, and the tendency of men to conjure up imaginary ills. Then he picked up Mrs. Stimson and drove to the airport. Every general officer in Washington was there, drawn up in lines from the car to the plane. General Marshall joined him and they walked down between those lines to the plane together. Never was a Secretary more respected and revered. Some months later he had a slight heart attack from which he quickly recovered. He wrote me a note, which is among my most prized treasures, in which he recalled our lunch and my advice. What a man he was!

One further recollection of President Roosevelt should be set down; it centers on Henry Wallace, whom I had come to know before the war. In fact, when I lived in a Washington hotel, he had a suite over mine and sometimes we would take a walk together on a Sunday morning. He was a delightful individual to be with. I remember visiting his Beltsville show where we took great joy in kidding some of the people who were raising turkeys and what not. He and I played the game back and forth for the fun of it. He had a good knowledge of biology, he was a good fellow to reason with, and he had a sense of humor.

Then during the war the first I encountered him was just as the atomic energy enterprise began to have very large implications, when it had become clear that the bomb could probably be built. I said to the President one day, "Mr. President, I am carrying quite a load of responsibility on this thing, and it is a matter requiring strange judgments. I would feel a little more comfortable if I had someone else, some group to report to, so that I would not be bothering you any more than necessary as this project proceeds." He said, "Very well, let's have a policy committee." He named Mr. Stimson, General Marshall, James B. Conant, Henry Wallace, and of course myself. I said to him, "How about Mr. Knox?" He looked at me with one of his strange smiles and said, "No, I guess not, not now."

I do not think that there ever was any formal appointment of that committee. It certainly seldom met, but after that whenever I had a report of any consequence to go to the President, I would go to each of these individuals beforehand, tell him what was in it, and talk to him about it. With Mr. Stimson I would be likely to go into it in some detail. With General Marshall I would just mention it; he would not go much further, or Mr. Stimson would indicate that he would take it up with the general. I also took it up with Henry Wallace. Then one day I went in with a report to the President. When I gave it to him I said, "Mr. President, your policy committee has approved this, with the exception of Mr. Wallace, who is not available." He grinned at me and said, "Well, Henry's out West making political speeches. I do not think we need to worry him." That was my cue and I never went near Wallace on atomic energy matters after that.

This is the way Roosevelt operated on such things. He would not say flatly, "Now we will drop Wallace off the committee," or anything like it. He would just give an implication and expect me to catch it. So that was that. There were no hard feelings between Wallace and myself as a result. But I think this was the time when Roosevelt had decided that he would not continue to regard Wallace as the man to be again nominated for the Vice-Presidency—and that is how Harry Truman came to be President of the United States.

President Roosevelt died at Warm Springs on April 12, 1945, and the first Cabinet meeting under President Truman took place late that day. It was brief. At the close, Mr. Stimson stayed on and told the President of the existence of the vast project from which a new weapon of unprecedented power was forthcoming. He gave President Truman the broad outlines of the endeavor and said that I could tell him the full story. Hence I was called to the White House later on, sat down with President Truman, and gave him the detailed fine structure of the affair—a full account of the atomic bomb as it then stood. Subsequently this report was enriched and amplified by Mr. Stimson and General Groves, with special reference to the probabilities of the future as regarded our then current monopoly of the weapon and the prospect that other nations, notably the Soviet Union, would attain it.

My own meeting with President Truman was the first time I had ever seen him. We got on a good basis of exchange at that first session. Later on he relied heavily on me, for a while, for information on scientific and technical matters. We had an interesting relationship, as long as it lasted.

I developed great respect for President Truman. I saw him in action several times when he was a real statesman. I saw him display superb courage. In regard to the briefing on the bomb, I simply told him the full story, which produced a few remarks that would be regarded as characteristic of Truman.

It may be well to pause here and tell about how I first told General Styer about the bomb. When I told Mr. Stimson that progress on the bomb had reached the point where a very large amount of money had to be spent for manufacturing facilities and so forth, I suggested to him that the Army take over at that point, and the Manhattan District was formed. Mr. Stimson put the job of forming it into the hands of General Styer, who was a very able general indeed. He was deputy to General Somervell. Styer met with me and discussed the appointment. Then he came over to get posted on the affair. I told him the general outlines, what the bomb would be like, if it ever came to reality, and suggested that he go over some of the records on it which I had gotten out of the safe for the purpose. I sat him down in an adjoining room to spend his time looking these over. And every little while I would hear exclamations by him—profane exclamations, incidentally. The same sort of reaction had occurred when I went through the story with Truman. This was typical whenever I briefed anyone—first interest but skepticism, and also no doubt disbelief—they could not at first really take in the significance. Then, as figures began to sink in, surprise, then excitement.

Later, after the bomb had done its work and the war was over, the question of making public some knowledge of the whole enterprise had to be faced, and here President Truman was, in my opinion, magnificent.

Toward the end of the war, Henry D. Smyth of Princeton compiled a summary volume on atomic energy in order to tell American science all that could be told about the affair in all its phases. His instructions were to put in everything that would be helpful to American science in pursuing the subject further, but not to put in anything unless it was known to the Russians. Groves, with his counterespionage work, knew well what had been transmitted to the Russians, and he knew well how it had been done. Under the conditions of the war, with Russia as an ally there was not a great deal that one could do if he came across such a trail except to pinch off its source of information, whereupon it would merely turn elsewhere. But Groves was in touch with all the preliminaries of the Smyth report and he went over the final document with great care. Groves, Conant, and I were all in the position to say that this book contained nothing that the Russians did not already know, but it contained a great deal that Americans needed to know. Then Admiral Leahy [19] objected to its publication.

We met in the President's office. President Truman sat at his desk, and the rest of us gathered around—Groves, Conant, myself, Admiral Leahy, Smyth, and a few others. After a bit of discussion, President Truman went around the room and said, "I want each one of you to state fully your opinion and the reasons for it." Then he sat back as one after another of us spoke up.

Groves, Conant, and I described the care with which the report had been prepared, outlined the good it could do as we looked forward to atomic power, and held that it would not benefit the Russians. But Admiral Leahy objected to release. Of course he did not know much about the subject, but that did not deter him. At one time during the war he was similarly stating, presumably to F.D.R., that rockets had long ago proved useless in war. To a certain type of mind, fortunately few, high command brings a conviction of omniscience. So he was very positive. His view was like the postwar attitude of some of the public and of many in Congress: There was an atomic bomb "secret," written perhaps on a single sheet of paper, some sort of magic formula. If we guarded this, we alone could have atomic bombs indefinitely.

When we had all spoken, the President sat back. There was a silence; he looked at the ceiling. Then he said, "I regret that I have to make decisions such as this." There was another pause, then, "You will release the report; the meeting is adjourned." I came out walking on air. We had a real leader. Later I was to find that we also had, in crises, a real statesman. I respected him enormously after that meeting, and after other incidents such as a moment when he and Forrestal and I were talking about something, and Forrestal spoke of the political implications of the decision. President Truman said to him, "Look, Jim, when you take a thing as serious as this to the American public you should forget political considerations." I believe that was an honest statement and that is what he actually did when he faced a tough question.

The next step on the bomb after the war was a Cabinet meeting on the subject of how to handle the atomic energy problem in the days of peace. Should we go to the United Nations with it? If so, should we discuss this with Russia before doing so? Mr. Stimson took me with him to the meeting, to present one aspect on which he and I were fully agreed. There have been various reports on that meeting, mostly wrong. The papers came out with an account of a "Wallace plan." If there was such a plan, I did not hear it. Forrestal's diary has an account. I do not know how it got in there in the form in which it appeared. Certainly it is not in accord with my recollection.

But I do remember clearly one incident. At a point in the discussion Forrestal made a remark which indicated that we were to make some sort of majority decision, and the President at once picked him up. "Jim, you forget, this is a Cabinet. Each of you will give me his full advice and I will make the decision." Truman certainly understood what it meant to be President of the United States. At the end of the meeting he asked each man to put a brief statement of his position in his hands the next day. Naturally, as I was the only non-member present, I asked him if I was also to do so. "You're damned right you are, what the hell do you think?" I do not believe I ever learned any new profanity from Truman, but I occasionally got refreshed on the use of some of old vintage.

Next was the so-called Attlee Conference,* which was primarily concerned with tactics in the approach to the United Nations, the principals at which were the President, Prime Minister Clement Attlee,[20] and Prime Minister MacKenzie King of Canada.[21] I never heard of an international conference that was worse conducted. It came out all right in spite of this fault, but nearly did not. A few days before it convened I found that almost nothing had been done about it, so I visited Secretary Byrnes[22] and urged that there be an American program prepared, that a State Department committee be put to work, and a secretariat appointed. Byrnes asked me what I meant by a program and I told him, following what had in my judgment been the President's plan for going to the United Nations. Byrnes told me to write it up, and I did so that evening. The next day we visited the President, and my paper became the American program. This was certainly a crude way to put such an important matter together. I hustled around to see Ben Cohen[23] in the State Department to tell him I was not trying to run off with the ball, that in fact I was trying to get his chief to use his own team.

The conference ran smoothly enough, producing drafts of a possible statement. Late one afternoon everything seemed to be agreed upon. A committee hence was appointed to draw up the final and agreed declaration of the conference to be presented for approval the next morning. This committee consisted of Sir John Anderson, Lester Pearson[24] of Canada, and myself. I had worked smoothly with Anderson before and knew his skill in such matters, so I was not worried. We all went to dinner at the British Embassy, but Anderson, Pearson, and I cut our dinner short and went to work.

* In his remarkable recent book, *Present at the Creation,* Dean Acheson characterizes this conference as "crucial" and "secret," and speculates that it was held on a Navy yacht. He declares that he is not sure since, even though he was Under Secretary of State at the time, he was not consulted. He is right about the characterization, but wrong about the yacht. We had no yacht. We didn't even have a stenographer at the meeting.

We were getting on rather well—we even had a first draft typed out—when, about nine-thirty, word was brought to us that the conference would not wait until morning, but would convene again at ten o'clock that night. So we went, each with a first draft, but no recording secretary. We read, and the conference made changes in wording, which I inserted in longhand in my copy. As discussion got a bit complicated I finally had the only fully corrected copy. Along toward midnight the principals were all satisfied and told me to read it back as it stood, which I did successfully but with some difficulty due to my inserted scrawls. Everyone was happy, the meeting adjourned; we all had a drink, and I left the White House with the only copy of the conclusions of the conference in my pocket. I remember also seeing Jimmy Byrnes leave, his straw hat just a bit aslant, with apparently not a care in the world.

The next morning at nine o'clock I was again in the White House. The declaration was gotten on the cable to London; clean copies were made for signature by the President and the two Prime Ministers. At one point I found a duplication, the same phrase in two places. So I went into the next room to see the President. He told me to take either one out and he would tell the Prime Ministers. The formal signing went smoothly enough, except that Senator Connally [25] took the President to task, a bit roughly and in the presence of our guests, for failing to consult the Senate Foreign Relations Committee. When it was all over, the President sought me out and said some kind things to me. So did Attlee a year or two later at a meeting in England which I shall mention in a digression later in this chapter. But all in all, this was no way to run an important conference.

There was an amusing incident some time later concerning this declaration. At a meeting in the Joint Chiefs organization I got into a bit of an argument with a general I had not met before, and it turned on the possibility of biological warfare. He smacked the table and said to me, "Don't you realize that the Attlee declaration contains the words 'and other methods of mass destruction'?" "Yes," I said, "I knew they were in there; in fact I put them in." He did not believe me, naturally, but it happened to be true. I had suggested it, and Sir John had promptly agreed. We both thought that, while we were attempting to bring reason to bear on one terrible weapon, we might as well include another that could be equally terrible, and which might indeed have become so if the atomic bomb had not taken the center of the stage.

The next step was to get the plan ready for the United Nations. I do not need to review this in detail. A committee was set up by Dean Acheson, and we worked hard on it; the fact that we felt the U.S.S.R. would block action in the United Nations was no excuse for not trying. Then Bernard Baruch [26] was called in to make the presentation to the United Nations, and it promptly became the "Baruch Plan." This annoyed the hard-working group, but they were more annoyed when they heard that Baruch's team was going to revise the plan. I was still more annoyed when the papers carried a note that Conant and I were going to serve under Baruch. But I knew Bernie well, and it did not take long to get these matters straightened out. I had to tell him that, when I started to work for him, hell would be frozen over. Of course, the approach to the United Nations got nowhere, but we all felt better for the try.

As I have remarked earlier, I have enjoyed associating with military men more than with any other group, scientists, businessmen, professors. Part of this is due, no doubt, to the fact that widely diverse backgrounds lead to interesting and useful interchange. But I think most of it is due to something else. Military men learn the art of command; it is central to their whole professional careers. They also learn to behave well in tight groups, and this again is necessary, for the wardroom of a ship, operating for long monotonous months, would be a shambles if officers had not developed a code of conduct which preserves decorum. It is not a stiff code; it is decidedly flexible, but it is an incorrigible officer indeed who does not emerge with an exceedingly attractive attitude of courtesy in places where courtesy is called for. At any rate, with very rare exception, military men are an admirable crowd, with whom I have been privileged and happy to associate.

One of the most pleasant experiences of the sort occurred soon after the war when I was invited by the Royal Air Force to exercises at a station on Salisbury Plain. A number of American officers were in attendance by assignment. General Spaatz [27] and I were the invited guests. We had red-carpet treatment; for example, I was promptly assigned a batman, as is the British practice.

The subject of the exercises was planning for R.A.F. action should Britain be attacked again within the next few years. About a hundred officers were present; the discussion was vigorous, but the atmosphere was a bit gloomy, for Britain, and especially the R.A.F., was weary, and the economic outlook was grim.

On the second day I was surprised that no mention at all had been made of guided missiles in the defense of Britain. This was some years before the days when practical intercontinental missiles became possible; the problems of adequate rocket engines, guidance, re-entry, were by no means solved. But rocket missiles that would automatically home on a hostile aircraft or ship had been made and used in various forms. Some were heat seekers, some were radar-guided, some were even controlled by television schemes. One of my worries toward the end of the war had been that the Nazis would begin to use them against our bomber fleets. But one of the foolish things they did was to neglect this possibility.

During a break I inquired of Air Marshal Tedder,[28] who was in charge, whether this subject had purposely been left out. He told me it had not, and urged me to introduce it. Naturally, I told him that, being a guest, I would not want to start an argument. But he said, "You just mention it and I will follow up."

So when we again convened I made some mild remark to the effect that I assumed we would come to missile defense later. Whereupon Air Marshal Slessor,[29] sometime Chief of the Coastal Air Command, rose and gave his group the devil, the core of his remarks being that they had better learn to think in modern terms. He had taken a piece of shrapnel in his hip in some fracas, so he steadied himself with two canes as he faced the group. Just as he got going all out, a cane slipped and he nearly went to the floor. A junior officer in a front seat caught him and steadied him back on his feet. Whereupon he said, "Damn that leg. Excuse me, gentlemen. As I was saying . . ." When we broke up toward the end of the afternoon, I had lots of questions on American experience in the development of guided missiles and on their probable future. None of us at that time thought we would shoot them across an ocean, and be able to hit something, in the foreseeable future. That capability waited for computer development and inertial guidance. But we all thought enemy skies would be tough places to fly in. In fact I, for one, thought flying over a fully defended area would become suicidal. It has not worked out quite that way.

Post-session gatherings in the bar were an important part of the exercises. We signed chits, bought one another drinks, and discussed the future. I promptly made arrangements so that I could sign chits, to avoid embarrassment, but when I came to leave, I was told that somehow all my chits had become lost. The last event was a dinner which Prime Minister Attlee attended. He seated me beside him, and we talked about the U.N., and atomic bomb control. I had great respect for his keen understanding.

One day I told Tedder that I had always wanted to drop a dry fly into a British chalk stream, for that is the last word in delicate fishing. I should have known what would occur. Soon I was invited to spend a day on such a stream. Sir Henry Tizard and I went down there, were entertained in the heartening British manner, and had a most enjoyable time. One fishes with a number 22 fly, on a nine-foot leader. The fish were highly educated. One fishes lightly, fine and far off, or he might as well quit. I hooked one fish and lost him in the weeds. My host apologized that, because of the war, they had not been able to keep the weeds mowed out of the stream. I had brought a bottle of bourbon, and we had a happy evening. The British learned the virtues of bourbon during the war.

This reminds me of a party in London, just as the tide was turning in the antisubmarine battle of the Atlantic. I had been in Britain for a week, and many had been kind to me. So, trying to express appreciation, I set up a cocktail party at Claridge's before I left. Bennett Archambault, head of my London office, told me we would have some Scotch at that party, if I would not inquire where it came from. So, when we assembled, about two hundred of us, there was a long table at one side of the room lined with bottles of Scotch. No one went near it. Apparently the presence of that much Scotch in London was considered an impossibility. So I got up on a chair, told the group that, unless a miscalculation had been made, there were two drinks there for each man, that I judged the Scotch had crossed the ocean twice, and that it would be sad for it to cross a third time. It promptly disappeared.

To return to the Salisbury Plain exercises—tobacco was also scarce in England at that time. So, I found out later, were matches and soap.

In getting ready to go to England for the Salisbury affair, I had put a lot of tobacco in my suitcase, in plastic pouches I could pass to smokers. There was a limit on how much tobacco one was allowed to take in, and the Customs officer who opened my suitcase noted that I had a lot of tobacco. I told him I was a heavy smoker. So he smiled and closed the suitcase.

Matches were different. I had put in two boxes of about a hundred folders of paper matches each, and had opened one of these and used a few folders. When I left Salisbury my batman did the packing. I had inquired about tips and had given him a dollar American. But as he was about to put in the opened box of matches, I told him he could have them, little realizing that he could sell the folders for a few cents each. This had a strange sequel.

I went from Salisbury to Lincoln College, Oxford, to visit my old friend, Professor Sidgwick.[30] He put me in a room that was furnished about the time of Queen Elizabeth. Lincoln itself dates from 1427. When I rejoined him, he asked me if I had brought any soap. It seems soap was so scarce, at least in the civilian population, that they had stopped dressing for dinner, a hardship on any British group, because they did not have soap with which to wash their stiff shirts. I told him that, unfortunately, I had no soap. But when I went back and opened my suitcase, there was a complete layer of soap on top, which I gathered up and dumped on Sidgwick's desk. That evening at dinner when I was introduced to some professor he would say, "Oh, you're the chappie that brought the soap." Explanation? Well, I think my batman was so grateful for the matches that he gathered the soap from all over the floor of the barracks and put it in my suitcase.

One more small event. I left Oxford and went to visit my old and valued friend Cockcroft, where we discussed civilian uses of atomic energy, which Cockcroft guided for some years with great skill. When I left he furnished me with an automobile driven by a nice-looking WAAF. We started for London, and I told her I was going to the Athenaeum to meet some friends. I was astounded when she told me she had never before driven into London. So I told her just to follow the crowd, and after a while I began to recognize places, and we got to the Athenaeum without having to ask directions. Sheer luck. I am sure I could not have done it in Los Angeles.

Some time later, after the National Science Foundation had been established, President Truman was about to appoint the board specified in the legislation. At that time there was an Armed Services Day dinner at which I presided, and the President spoke. It was in the Mayflower Hotel, and I met him at the entrance for automobiles and conducted him in. Knowing Mr. Truman's habits pretty well, I said to him, "Mr. President, I am afraid this is going to be a dry dinner. Would you like a drink first?" And he said, "I certainly would." We turned aside into a room, and left the Secret Service men outside, and at the end of the room was a little table with a chap with a white coat on, and the President walked up and we had a drink. He had his drink in his usual manner, that is, neat, followed with a chaser. So of course I did the same thing, hoping that I would not choke when I tossed the neat whiskey down my throat. Then we proceeded in to the dinner.

During the dinner he sat on my right and Louis Johnson,[31] Secretary of Defense, was sitting beyond him. Apparently he had talked to Johnson all he wanted to, so he talked to me. The subject of the science board came up, and I said, "Mr. President, I wish you would leave me off that board. I know my name is on the list, but I wish you would leave me off." He said, "Why?" and I said, "Well, I have been running about everything scientific during the war, and somewhat since, and I think people are getting tired of seeing this guy Bush run things around here. I think this outfit would do better if it had some new leadership. If you put me on the board, they will elect me chairman, and I do not think that the body of scientists are going to like this continuation of one man in the top post. So I think you would do better to let somebody else do it." Well, after a bit more talk, he agreed to leave me off the board. Then he said, "Well, Van, you are not looking for a job, are you?" And I said, "No, Mr. President, I am not looking for a job." He said, "You cannot say I went looking for this job that I am in." And I said, "No, Mr. President, not the first time," which tickled him a bit. He poked me in the ribs and said, "Van, you should be a politician. You have some of the instincts." I said, "Mr. President, what the hell do you think I was doing around this town for five or six years?"

That was my relationship with Truman. Very pleasant, very informal, and on a basis which I enjoyed greatly. For quite a while

I was close to him and helped him all I could. But then it all stopped. I always thought, perhaps without reason, that I became inconvenient to Truman's palace guard and got poisoned. At any rate, all contact ceased. I never knew why. One time I was talking with Senator Anderson,[32] and he said something about a thing I ought to take up with the President. I said, "Clint, I have no influence in the White House whatever." And he said, "What happened to you?" I said, "I don't know. I guess I got in the way, and I got poisoned." He said, "You know, that happened to me once, and the President would not talk to me for months, and I never did find out what it was all about." And I never did find out, either, what it was all about. But that is what happened.

When Truman wanted a report on postwar science he named as chairman one of his staff, John Steelman.[33] He appointed a committee, but Steelman did the work and wrote the report. I had been chairman, after the war, of the Joint Research and Development Board of the Joint Chiefs of Staff. When the Unification Act was passed it provided formally for a Research and Development Board, in the Department of Defense, and Forrestal asked me to take the post of chairman. I said to him, "Jim, it's no good. The President has lost confidence in me; it would not work." "Well," he said, "we had better see the President."

We went to see Truman and Truman said, "I want you to become chairman of that board." I said, "Mr. President, it is no good. You have evidently lost confidence in me; you had better have a man you have full confidence in." He hit the roof, using characteristic language for Truman, and said, "What makes you think so?" I said, "For example, when F.D.R. wanted a report on postwar science, he called on me. When you wanted a report, you called on John Steelman." "Yes," he said, "but you were on the committee." I said, "Sure I was on the committee, but I never saw the report until it was in print." There was quite a silence after that. I do not think he had realized anything of the sort. Then he gave me a long harangue to the effect that he had full confidence in me, he wanted me to do this job, that he was going to lean on me just as much as he ever had, and so forth and so on. I said, "Very good, Mr. President, I will take the job," and I did. After which he did not call on me again.

Now I do not know what came in to spoil that relationship. I probably never will know. But there is a possible though seemingly trivial explanation. It has to do with awards and honors. After the war, as after all wars, there was considerable to-do about recognition, credit, and so on, for work done. Lists of proposed recipients of medals and certificates were being put together by various agencies.

The Army and Navy had gotten out a list of civilian scientists and, as it appeared, it was not very good, not very accurate in appraisal of contributions. Conant and I felt we must delay this, and we delayed it by the simple statement that we would not join in that list and accept a medal unless the list was improved.

It was improved, but doing so took quite a little while. In the course of the time when we were waiting and getting it revised, the White House called me up and told me to be down there at eleven o'clock the next morning, to get decorated. I replied, "Unfortunately I cannot do so. I have the influenza." I had no influenza. But that was about the only basis on which one could refuse to go to the White House. I think somebody told Truman that I ducked the ceremony, and I think that may have been why, or one reason why, he got mad at me. He certainly sounded mad the next time I saw him.

I also think Truman later found out that there was nothing in it, and this is why I think so. A few years ago, one of my friends went into the museum where Truman's papers are collected. I have never been there, but I would like to go there for this reason: When this friend of mine went in, one of the first things he saw was my picture hanging on the wall. However, it was not in a collection of pictures, according to him. It was hanging by itself. I rather think that Truman found out later that the fairy tale he had accepted had no basis in fact. He did not want to say so, and I have never met him since those days, so that I could bring it up, but I think when he was arranging things for his museum, he just stuck that picture up on the wall for that reason. This is pure surmise, but it may be so. If he did, it would be just like him.

Finally let me say this: Not only were my relations with Truman excellent, but I really had admiration for the man. I saw him act as a statesman on a number of occasions. I saw him make tough decisions, and I think he was extraordinary. He enhanced my faith that our sometimes absurd political processes can and do produce leaders of stature.

Life in Washington can have its light moments. In fact I often tell friends that no one should work in government, with all its frustrations, unless he has a well developed sense of humor. The time came when Truman finally did pin a medal on me. The event promised to be interesting, for, as I have said, I thought he was still a bit mad at me. My wife, a young grandson, and I were waiting in the anteroom when a chap came in, said, "Hyar you, Van," and I said, "Hello, John," and he went out. "Who is that?" asked Phoebe. "That," said I, "is John Steelman." "But I thought you didn't like John Steelman." "I don't; he hates my guts." "But you greeted each other like old friends." "That," I said, "is the custom of the town." The ceremony went smoothly enough. Truman was not mad at me after all; he took my grandson on his knee and presented him with a pen marked "Swiped from the desk of Harry S Truman."

Neither Truman nor for that matter Eisenhower at first understood the art of effective relations between the President and the scientific and engineering fraternity. It revolves about working with and through men who are genuinely and widely accepted as belonging to an unorganized but nevertheless real central group of leaders in the field. Fortunately, Eisenhower later saw the real point, and when Kennedy took over, he understood it very well indeed, and effective lines of communication were again established, and have continued ever since. True, we no longer have an independent agency concerned with the development of new weapons. There is much doubt whether any such organization could succeed in times of peace, or for that matter in times of limited war. Certainly it could never perform except under a President who took a real interest in it.

This brings me to a matter that has puzzled me deeply. In my opinion our military leaders during World War II were by far the finest this country ever produced. This is not merely that they were masters of military strategy, which they were. It is not merely that they thoroughly understood the handling of men, and how to inspire them to supreme effort. In addition, they caused a military alliance to operate effectively in ways in which no such alliance had ever operated in the history of warfare. Our military establishment, our military thought, in these respects were superb. Yet in the crucial area of the modification of warfare by the impact of new weapons, new materials, we were far from alert at some of the top echelons.

In thinking about this matter we must distinguish between commanders in the field, so burdened with daily decisions that they could hardly be expected to keep up with the evolution of weapons, and those at home base, where they at least had the opportunity to do so. Admiral King, as I have noted, did not, and his stubborn adherence to the convoy system, long after direct attack on the submarine had become possible, cost us severely.

Neither Marshall nor Eisenhower had close contact with the advent of new weapons. Both of them dealt with interference from above and with the gibes of jealous men, and were not deflected in the slightest degree from pursuit of their real objective. I link them in this tribute, although they differed greatly in personality. Yet neither was deeply interested in the evolution of modern weapons, an evolution which revolutionized the art of warfare which they had studied all their lives. Most of this gap was due, no doubt, to the fact that they were so burdened that there was not time for them to become acquainted with what was going on. Even so—and this perplexes me—as the new weapons appeared, they altered, sometimes radically, the massive effort these two men controlled, and yet, so far as I know, neither attempted to look ahead to visualize what might be coming. Marshall was formally in the direct line of responsibility on the atomic bomb. I never discussed the subject in any detail with him. The only time he asked me about a new weapon, it was on a very minor matter. When I went to France and took up the introduction of the proximity fuze into land combat, a move which Patton said revealed that the principles of such combat had become obsolete, it was Bedell Smith whom I talked to at Versailles, not Eisenhower. So it was when I discussed whether Nazi progress on the atomic bomb threatened to become determining in the last days of the war in Europe.

There was only one time when I discussed new weapons with Eisenhower, and that was just after he became Supreme Commander for Overlord, the invasion of Normandy. We had long known of the German program on missiles. For the V-1, the buzz bomb, the Germans had constructed numerous launching sites on the Pas de Calais, in our full view. We had a group of civilians working closely with Army Intelligence on the matter. This group was headed by Alfred Loomis. There could not have been a better man for the purpose, for he had a unique background. He had made a fortune in the organization of public utilities, and later had been elected to the National Academy of Sciences on the basis of a distinct scientific accomplishment; not many men ever made that combination. There was no doubt of the German plan, the date of introduction of the buzz bomb, its general characteristics, the numbers per day that it was proposed to launch. Only the nature of the payload remained a mystery, even though the intelligence group had interviewed a French contractor who had built one of the launching sites. We feared that some of the strange construction indicated a payload of poison gas, or even radioactive materials.

It became my task to brief Eisenhower on the threat. I did so, and said that, if the German program went as scheduled, with the buzz bombs directed on Plymouth and Bristol, it might well knock the plans for Overlord into chaos. When I finished Ike said, "You scare the hell out of me. What do we do?" I told him that was out of my field, but that it seemed to me intense bombing of sites was the only answer. Of course there were many other conferences on the matter. We know the outcome. The sites were bombed; Overlord went through as scheduled. Later, when radar-controlled antiaircraft guns, using proximity fuzes, were mounted on the coast of Britain, the buzz bomb was completely countered.

The day the first buzz bomb fell on London, Secretary Stimson and I were bound for Capitol Hill in Washington, for a conference with congressional leaders about the budget. As we rode along Pennsylvania Avenue, both of us thinking silently of the buzz bomb news, he put his hand on my knee and said, "Well, Van, how do you feel now?" I said, "Very much relieved." That was all. The payload of the buzz bomb was conventional explosive. If it had been nerve gas, civilization would have had an even heavier jolt than the one received at Hiroshima. And nerve gas would have been much less terrible than the possibilities of biological warfare.

When Eisenhower became President he appointed Lewis Strauss [34] chairman of the Atomic Energy Commission, and Strauss became his link for a time, as far as there was one, between the President and American science. The word is that the appointment was made at the recommendation of Herbert Hoover. Certainly neither one discussed it with me or, as far as I can determine, with anyone who had had close association with the field of atomic energy. I am not here going to analyze the career of Lewis Strauss, although it would be a fascinating subject for a biographer. Suffice it to say that Strauss was not accepted as a fellow member by the scientific community. One does not enter that community through success in banking, unless there is also a real contribution to scientific knowledge, as was the case with Loomis. Strauss was not anxious for my collaboration on liaison between the White House and science. So I never saw Ike while he was President, except on a social basis. There he was his genial, attractive self. We were always thus on good terms. But I regretted that I could not be of more aid to him, for scientific and technical matters were still boiling in the country.

Later, as we know, just after the flight of Sputnik, Eisenhower brought in James R. Killian as Scientific Adviser to the President, together with a scientific advisory committee. This form of organization has persisted, and it certainly provides a direct means by which the President can obtain promptly the best of scientific advice on any problem before him. It will not work, of course, unless the President wants to use it. But it is excellent in concept. Through the members of the advisory group there is a linkage with all aspects of American science. We have had, ever since Lincoln established it, a National Academy of Sciences, later supplemented by the National Research Council, charged with advice to government on scientific matters. For long years it operated feebly, for neither the administrative branch nor the Congress knew how to use it, or cared to. This condition has changed, and the Academy now is decidedly active on all sorts of problems, perhaps at times too active. It is to be noted that the advice is not rendered as the opinion of the Academy. It is the opinion of a small group, chosen by the Academy and Council, not necessarily from its own membership, selected solely on the basis of competence. Thus, while I am a member of the Academy, it does not disturb me to see an opinion rendered with which I disagree heartily. Through the organization now existing in the President's office, this whole mechanism is directly available to him, and to his Cabinet members and bureau chiefs. The establishment in 1964 of a National Academy of Engineering should also help. In addition there have been established within the military establishment offices held by scientists or engineers, advisory to the Secretaries, charged with supervision of the scientific and technical research and development conducted by Army, Navy, and Air Force, both within their own organizations and by contract with universities and industry.

This is certainly a well-rounded organization for providing scientific and engineering advice where it is needed. But committees of Congress still get their scientific and technical advice in strange ways or not at all. On the whole, they do not utilize the machinery which is readily available to them for getting expert unbiased advice along those lines as they need it. They had better do so, and soon. There are exceptions of course, for there are a few men in the Congress who know a great deal about science and engineering. But scientists and engineers seldom sit in legislative bodies. Neither do physicians, nor for that matter men from active business, nor men from the ranks of organized labor. Legislatures are made up principally of lawyers. I have nothing against lawyers as a group. But our lawmakers would do a better job if they represented all phases of American life, especially the professions. But, as with many other minor defects of our political system, we seem to be stuck with this one. The best we can hope for is that legislators will learn to get sound independent professional advice when they need it. They have not learned yet.

This history has been set down with two momentous subjects in view. The first is the relation between government and the broad process by which new technological ideas are fostered and developed for security and progress of the country. In the military field we can view the relation with some satisfaction. Before World War II there was no really workable organization for the purpose in existence. Under the pressure of war, as has been told in another chapter, new organization was hastily innovated, and fortunately it worked. Since then an elaborate pattern has been developed, which is, without question, adequate for its intended purpose, provided it is really used.

No matter what the organization may be, unless the President wishes to use it and knows how to do so, it will be futile. This does not mean that the President must be burdened with one more task, added to the nearly impossible burden he inevitably carries. Rather, it merely means that interest and understanding on his part are required. For, if that interest is present and made evident, his senior subordinates, civilian and military, will heed it, and the organization will be effective. He will see that they heed it, for he makes the choices of our leaders in the Cabinet and Armed Services. Unfortunately, the converse is also true. If his genuine interest and encouragement are not present, the whole thing will falter. And, when it does, the real leaders in science and technology will become discouraged and look the other way. This is not just a question of the further development of military weapons and systems. Advanced technology enters inevitably in all sorts of ways in our modern affairs, in urban renewal, transportation, crime control, pollution, and a dozen other serious subjects. Does this mean that we need Presidents with scientific or technical backgrounds, a modern form of Plato's philosopher-king? Not at all. It calls for leadership in a far broader sense. It involves the ability to use, to inspire, to sustain and encourage all sorts of men of ability, to earn their loyalty and urge them on. That is what a President is for.

This takes me to the second subject with which this history is concerned. As I think over the seven Presidents I have had the privilege of serving under, or to whom I have had relations in less formal ways, my prime impression is that we, the American people, have had far better leaders than we deserve, far better than one would think our political process would produce.

For our method of electing a President is crude in the extreme. Preconvention juggling with local political bigwigs is obscure (often, one fears, on a low plane), and it, and the travel and speechmaking, cost millions, the sources of which are sometimes questionable. Our national conventions are at best ludicrous, and at worst scandalous.

They are conducted by delegates, chosen ofttimes without much attention to the public will, and committed to vote in strange ways, or not committed at all. Out of all this appear a nominee and a platform, the latter often drawn by others, to which the nominee is assumed to be committed, but to which no one pays much attention. Then follows the campaign—more travel, speeches, television interviews, which would exhaust a Spartan. Finally there eventuates, not an election, but the action of a thoroughly obsolete electoral college, which may elect a man, and has done so, with fewer votes than his opponent, or which may elect no one and throw the ultimate choice into the House of Representatives, where each state has one vote, and Alaska is as powerful as New York or California. What an absurd performance.

Yet out of it have appeared great Presidents, ranging from men such as Hoover or Eisenhower, of no originally apparent political skill, to such as John F. Kennedy, as skilled a man politically as this country has produced, yet one who developed an approach to the minds and hearts of people, not only here but everywhere in the world, to the extent that when he died he was mourned more sincerely than any man since Lincoln. Or Truman, picked as Vice-Presidents usually are merely to garner a few more votes for the ticket, who became a statesman under stress. Or Roosevelt, who, whatever else might be said, led us out of the wilderness.

There must be something more, beyond the absurd machinery, some subtle way in which the public will becomes exerted, some force beyond political manipulation, unrecognized yet powerful. I believe there is, and that I have seen it work.

And I believe this is becoming more effective as the years go by. We are slowly becoming more mature. Television has helped here, whatever it may have done in the way of distorting some of our concepts of civilization. So have more facile transportation, the press, and the commentators. When I get downcast I read history. On this matter I go back and read about Tippecanoe and Tyler too—slogans, mudslinging, torchlight processions, and not a policy or principle in sight. We may yet be crude, but we have come far.

So I end by paying tribute to the Presidents who have touched and altered my life and the lives of millions: far different one from another, human with human frailties, but also with human capability to rise to the occasion, great men all.

Biographical Notes

Note: The task of assembling reliable biographical data on distinguished contemporaries before they have settled firmly into the history books is substantial. The notes that follow were gathered from widely scattered sources: the most commonly accepted reference books, newspaper notices, almanacs, directories, and—where available—official records of the U.S. Government. Despite careful checking and rechecking, a certain number of inconsistencies and discrepancies have proved difficult to eliminate. Military ranks and titles present a special problem, particularly for the war period, when there were frequent shifts between temporary and permanent ranks and rapid changes of command. In general, the data emphasize the period of the man's life to which the book makes reference and are not intended to present a complete résumé. Most of the persons mentioned have garnered in their lifetimes lists of honors and awards so extensive that they have been omitted from the notes. An occasional exception has been made where an honor has special relevance or historic interest.

Chapter I

[1] General Hugh S. Johnson, 1882–1942. He resigned from military service after World War I with the rank of brigadier general, and became organizer of the Moline Implement Company. He was associated with Bernard M. Baruch, New York City, 1927–1933. Administrator, National Recovery Administration, 1933–1934, and Works Progress Administrator, New York City, August to October, 1935. Editorial commentator for Scripps-Howard and other papers and for radio from 1934 on.

[2] Huey Pierce Long, 1893–1935. Governor of Louisiana, 1928–1931. U.S. Senator, 1931–1935. Entertained Presidential aspirations on a "Share-the-Wealth" platform. Assassinated in the rotunda of the state capitol by Dr. Carl A. Weiss, September 8, 1935.

[3] Richard Whitney. President of the New York Stock Exchange, 1930–1935. Indicted by New York County and New York State in 1938 for

embezzlement and misuse of funds. Pleaded guilty and was sentenced to prison; permanently enjoined from security business. Paroled in 1941.

[4] Samuel Insull, 1859–1938. Private secretary to Thomas A. Edison. President of Chicago Edison Company, 1892, Commonwealth Electric Company of Chicago, 1898, People's Gas Light and Coke Company, Chicago, and other companies. Indicted, 1932; arrested, 1934; tried, 1934, 1935, and acquitted.

[5] Al Capone, 1899–1947. Chicago gangster who headed a syndicate active in such varied underworld activities as gambling, prostitution, and especially the illegal sale of liquor.

[6] Frederick Lewis Allen, 1890–1954. Editor *Harper's Magazine;* author of *Only Yesterday, The Lords of Creation, Since Yesterday.*

[7] George Santayana, 1863–1952. Spanish-American poet and philosopher. Teacher of philosophy at Harvard, 1889–1912. Author of numerous philosophical works.

[8] Leroy Robert Paige. Negro baseball pitcher who achieved success in the Negro leagues before being signed by the Cleveland Indians in 1948. In later years famed as a relief pitcher and philosopher.

[9] General Omar Nelson Bradley. Commanding General 2d Corps, Northern Tunisian campaign, 1943, Sicilian campaign, 1943, 1st U.S. Army, Normandy campaign, 1944, 12th Army Group, France, Belgium, Holland, Luxembourg, Germany campaigns, 1944–1945; Administrator, Veterans Affairs, 1945–1947; Chief of Staff, U.S. Army, 1948–1949; Chairman, U.S. Joint Chiefs of Staff, 1949–1953; General of the Army, 1950. Chairman of the Board, Bulova Watch, 1958. Author, *A Soldier's Story,* 1951.

[10] Montgomery of Alamein, 1st Viscount, cr. 1946 of Hindhead; Field Marshal Bernard Law Montgomery. Commander, 8th Army, from July 1942, during campaigns in North Africa, Sicily, and Italy; Commander in Chief, British Group of Armies and Allied Armies, North France, 1944; Commanded 21st Army Group, 1944–1945; commanded British Army of the Rhine, 1945–1946. Chief of the Imperial General Staff, 1946–1948. Chairman of Western Europe Commanders in Chief Committee, 1948–1951; Deputy Supreme Allied Commander, Europe, 1951–1958.

[11] Judge Learned Hand, 1872–1961. Distinguished American jurist. U.S. District judge, Southern District of New York, 1909–1924; judge U.S. Circuit Court, 2nd Circuit, 1924–1951.

[12] Charles Evans Hughes, 1862–1948. Associate Justice, U.S. Supreme Court, 1910–1916; U.S. Secretary of State, 1921–1925; member Hague Tribunal, 1926–1930; Judge on Permanent Court of International Justice, 1928–1930; Chief Justice U.S. Supreme Court, 1930–1941.

[13] Caryl P. Haskins. Physiologist. President and Director of Research, Haskins Laboratories, 1935–1955. During World War II, special assistant to the director, senior assistant liaison officer, O.S.R.D.; executive assistant to the chairman of N.D.R.C. and deputy executive officer. President, Carnegie Institution of Washington, since 1956.

[14] Will Rogers, 1879–1935. American actor, lecturer, and humorist. Starred in a number of motion pictures. Wrote syndicated newspaper articles in *The New York Times* daily from 1926. Also author of several books. Killed in airplane crash with Wiley Post.

[15] Charles Spencer Chaplin. Motion picture actor. Made famous his characterization of the little clown. Films include *The Kid*, *The Circus*, *The Gold Rush*.

[16] Ernest T. Pyle, 1900–1945. Long-time Scripps-Howard news reporter. Gained fame as a war correspondent during World War II. Noted for his empathy with the GI's and his coverage of "the GI's war." Author, *Here Is Your War* (1943) and *Brave Men* (1944).

Chapter II

[1] Mountbatten of Burma; Admiral of the Fleet Louis (Francis, Albert, Victor, Nicholas) Mountbatten. British Naval officer, great-grandson of Queen Victoria; Supreme Allied Commander, Southeast Asia, 1943–1946. Later high posts include First Sea Lord and Chief of Naval Staff, 1955–1959; Chief of Defense Staff, also Chairman of the Chiefs of Staff Committee, 1959–1965. Governor, Isle of Wight, 1965– . Personal A.D.C. to the Queen since 1953.

[2] William Joseph Donovan, 1883–1959. Unofficial observer for Secretary of the Navy, Great Britain, July–August 1940. Appointed Coordinator of Information, July 1941. Director, Office of Strategic Services, June 1942; rank of Major General U.S. Army.

[3] Sir Henry Tizard, 1885–1959. During World War II, the first and only civilian member of the Air Council. Made chairman of the Defense Research Policy Committee of the United Kingdom and later also appointed chairman of the British Advisory Council on Scientific Policy. Earlier in his career he was rector of the Imperial College of Science and Technology in London, then president of Magdalen College, Oxford.

[4] Frank Baldwin Jewett, 1879–1949. President, Bell Telephone Laboratories, Inc., 1925–1940, Chairman of the Board, 1940–1944; President, National Academy of Sciences, 1939–1947.

[5] James Bryant Conant. Scientist and educator. President Harvard University, 1933–1953, now president emeritus. Chairman of the National Defense Research Committee, 1941–1946; deputy director, O.S.R.D., 1941–1946; member general advisory committee, Atomic Energy Commission, 1947–1952. United States Ambassador to the Federal Republic of Germany, 1955–1957. Made studies of the American public high school and the education of American teachers, 1957– , under grant from the Carnegie Corporation of New York. His recent book, *My Several Lives*, contains his personal recollections of the wartime years.

[6] Richard Chace Tolman, 1881–1948. Professor of physical chemistry and mathematical physics and Dean of the Graduate School, California Institute of Technology after 1922. Vice-chairman of the National Defense Research Committee, 1940, and chairman of the Armor and Ordnance Division of that committee. Science adviser to United States Representative, United Nations Atomic Energy Commission, 1946.

[7] Karl Taylor Compton, 1887–1954. Physicist and educator. President, Massachusetts Institute of Technology, 1930–1948, Chairman of the Corporation, 1948–1954. Member National Defense Research Committee, 1940–1947; Chief, Office of Field Services, 1943–1945, Director Pacific Branch, 1945; Member Science Intelligence Mission to Japan, 1945; Special representative Secretary of War in S.W. Pacific Area, 1943–1944; Chairman, Research and Development Board, 1948–1949.

[8] John Victory, "Mr. Aviation." The first employee of the National Advisory Committee for Aeronautics when it was established in 1915. Appointed secretary in 1921 and executive secretary in general charge of administration in 1948. Upon the establishment of the National Aeronautics and Space Administration and its subsequent absorption of N.A.C.A., became special assistant to T. Keith Glennan, administrator. Honors and awards include the Wright Brothers Memorial Trophy in 1958, for "significant public service of enduring value to aviation in the U.S." Retired in 1958 after fifty years of public service.

[9] Oscar Sydney Cox, 1905–1966. Assistant to General Counsel, U.S. Treasury Department, 1938–1941; General Counsel, Lend-Lease Administration, 1941–1943; also Office for Emergency Management, 1941–1943; Assistant Solicitor General of the United States, February 1942–December 1943. General Counsel, Foreign Economic Administration, 1943–1945, Deputy Administrator, January to October 1945. Member law firm Fox, Langford & Brown, Washington, D.C.

[10] Harry L. Hopkins, 1890–1946. Long-time friend and adviser of President Franklin D. Roosevelt. Resigned as Secretary of Commerce in 1940 to head Lend-Lease program. Later took up post as Adviser in Residence to President Roosevelt, 1941–1945.

[11] Conway P. Coe. Named Commissioner of Patents, June 25, 1933, the youngest man ever to serve in that post; resigned June 15, 1945, to return to private practice of law. During World War II, patents member of the N.D.R.C., member of the National Inventors' Council, and executive secretary of the National Patent Planning Commission. R.C.A. Patents Dept., 1967; now retired.

[12] Brigadier General George Veazey Strong, 1880–1946. Commissioned 2nd Lieutenant Cavalry, U.S. Army, June 15, 1904, and advanced through grades to Major General, April 1941; retired 1944. Assistant Chief of Staff, October 1938; Commanding General 8th Army Corps, May 1941; appointed head of Military Intelligence, June 1942; Joint Chiefs of Staff, April 1944–1945.

[13] Rear Admiral Harold G. Bowen, 1883–1965. Commissioned ensign, U.S. Navy, 1907, advanced through grades to Vice-Admiral, 1946. Director, Naval Research Laboratory, also technical aide to the Secretary of the Navy, 1939–1942. Special assistant to Undersecretary of the Navy and Secretary of the Navy, 1942–1947. Director, Office of Patents and Inventions, 1945; Chief Office Research and Inventions, May 1946; Chief of Naval Research, August 1946; retired 1947.

[14] Members of the N.D.R.C.: Commissioner Coe served from June 28, 1941, to September 14, 1945, being succeeded on September 15 by Casper W. Ooms, Brigadier General Strong was succeeded as Army member by Brigadier General R. C. Moore, Major General C. C. Williams, Brigadier General Walter A. Wood, Jr., Colonel Lee A. Denson, Jr., Colonel P. R. Faymonville, Brigadier General E. A. Regnier, Colonel Michael M. Irvine, and Colonel Edward A. Routheau. Admiral Bowen, as Navy member, was followed by Captain Lybrand P. Smith, Rear Admiral J. A. Furer, Rear Admiral A. H. Van Keuren, and Commander H. A. Schade.

[15] Irvin Stewart. Member Federal Communications Commission and chairman of its telegraph division, 1934–1937; vice-chairman of Commission, 1935–1937; director, Committee on Scientific Aids to Learning of the National Research Council, 1937–1946. Executive secretary and contracting officer, Office of Scientific Research and Development, 1941–1945; deputy director, O.S.R.D., 1946. President, West Virginia University, 1946–1958. Chairman, President's Communications Policy Board, 1950. Now consultant, National Academy of Public Administration, Washington, D.C.

[16] Clarence Cannon, 1879–1964. Served continuously as member of Congress from 1923 until his death in 1964, representing the 9th Missouri District. Chairman of the House Committee on Appropriations during the critical war years, 1941–1947, and again from 1949–1953 and 1955–1964.

[17] Isidor I. Rabi. Physicist. Associate director, Radiation Laboratory, M.I.T., during World War II. Winner of the 1944 Nobel prize in physics "for his resonance method for recording the magnetic properties of atomic nuclei." Taught physics at Columbia University, 1929– , Eugene Higgins Professor of Physics, 1950–1964, University Professor, 1964–1967, Emeritus, 1967– . Chairman, General Advisory Committee, Atomic Energy Commission, 1952–1956. Chairman, President's Science Advisory Committee, 1957. President, Associated Universities, Inc., April 1961–October 1962.

[18] John Thomas Connor. General counsel, O.S.R.D., 1942–1944. Counsel, Office of Naval Research, also special assistant to the Secretary of the Navy, 1945–1947. President and director of Merck & Co., 1955–1965. Secretary of Commerce, 1965–1967. Now president and chief operating officer, director, Allied Chemical Corporation.

[19] Oscar Melick Ruebhausen. Attorney, Lend-Lease Administration, 1942–1944. General counsel, O.S.R.D., 1944–1946. Counsel, International Development Advisory Board, 1950–1951. Director, International Basic Economy Corporation, 1956– . Chairman, executive committee, 1951–1961. Secretary, director, Fund for Peaceful Atomic Development, Inc., 1954–

[20] Carroll Louis Wilson. After serving as assistant to Karl T. Compton, president of Massachusetts Institute of Technology, 1932–1936, and special adviser to Vannevar Bush, vice-president and dean of Engineering, M.I.T., 1936–1937, became head of Patents Department, Research Corporation of New York, 1937–1940. Assistant to Vannevar Bush, N.D.R.C., 1940–1941; senior liaison officer, 1941–1942; executive assistant to director, O.S.R.D., 1942–1946. General manager, U.S. Atomic Energy Commission, 1947–1951. Vice-president and general manager of Metals and Controls Corporation, 1956, president and general manager, 1956–1958, chairman, executive committee, 1958–1959. Now professor, Massachusetts Institute of Technology, director of M.I.T. Fellows in Africa, Colombia, Urban Affairs.

[21] Howard Andrews Poillon, 1879–1954. President, Research Corporation, 1927–1945. Vice-chairman, Division of Engineering and Industrial Research, National Research Council, 1936–1945.

[22] Frederick L. Hovde. Rhodes scholar from North Dakota, Oxford University, 1929–1932. Assistant to president, University of Rochester, and executive secretary of Rochester prize scholarships, 1936–1941. Head, London Mission of O.S.R.D., 1941–1942; executive assistant to chairman, N.D.R.C., 1942–1943; chief, Division 3, N.D.R.C., 1943–1945. President, Purdue University, since 1946.

[23] Archibald MacLeish. Noted American poet, Librarian of Congress, 1939–1944; also director, U.S. Office of Facts and Figures, 1941–1942; as-

sistant director, Office of War Information, 1942–1943; Assistant Secretary of State, 1944–1945.

[24] Marguerite Alice (Missy) LeHand, 1892(?)–1944. Born Potsdam, N.Y., educated Somerville, Mass. Secretary and personal aide to President Roosevelt, with whom she had been associated since 1920 when he was Assistant Secretary of the Navy. Until her illness, resided in the White House.

[25] Samuel Irving Rosenman. Counsel to Franklin D. Roosevelt when he was Governor of New York, 1929–1932; he was appointed Justice of the Supreme Court of New York in 1932; resigned to become special counsel to President Roosevelt in 1943.

[26] Roger Adams. Long associated with the chemistry department, University of Illinois, he became head of the department of chemistry and chemical engineering, 1926–1954, research professor, 1954–1957, emeritus professor, 1957– . Member, N.D.R.C., 1941–1946. Scientific Adviser to U.S. Deputy Military Governor of Germany, 1945. Chairman, Science Advisory Committee, U.S. Military Government, Japan, 1947. Member, Science Advisory Mission to Japan, 1948.

[27] Vice-Admiral Ross T. McIntire, 1889–1959. Surgeon General of the Navy and chief, Bureau of Medicine and Surgery, 1938–1946. White House physician, 1933–1945.

[28] Lewis Hill Weed, 1886–1952. Director, School of Medicine, Johns Hopkins University, 1929–1946. He also served as military director, Army Neuro-Surgery Laboratory, Johns Hopkins Medical School. Research associate, Carnegie Institution of Washington, 1922–1935. Member Medical Fellowship Board, National Research Council, 1935–1939, chairman, Division of Medical Sciences, N.R.C., 1939–1949. Member and vice-chairman, Committee on Medical Research, O.S.R.D., 1941–1947.

[29] A. Newton Richards, 1876–1966. Vice-president in charge of Medical Affairs, University of Pennsylvania, 1939–1948. A physiological chemist, he was the first to provide detailed explication of the physiology of the kidney. He established a brilliant teaching course in pharmacology that helped to bring the University's medical school new stature. Chairman, Committee on Medical Research, O.S.R.D., 1941–1946. President of the National Academy of Sciences, 1947–1950.

[30] Chester S. Keefer. Medical administrative officer of the Committee on Medical Research of the O.S.R.D. Wade Professor of Medicine at the Boston University School of Medicine, 1940–1964, and Wade Professor of Medicine Emeritus, 1964– . Director, Robert Dawson Evans Memorial, 1940–1959; Physician in Chief, Massachusetts Memorial Hospital, 1940–1959.

[31] Frederick E. Terman. Executive head, Electrical Engineering Department, Stanford University, 1937–1945; dean of engineering, 1945–1948, provost, 1955–1965, vice-president, 1959–1965, emeritus, 1965– . President, Southern Methodist University Foundation, Science and Technology, 1965– . Director, Harvard Radio Research Laboratory, 1942–1945. Member, Divisions 14 and 15, N.D.R.C., 1942–1945.

[32] Lee Alvin DuBridge. Professor of physics, University of Rochester, 1934–1946; dean of the Faculty Arts and Sciences, 1938–1941. Director, Radiation Laboratory, M.I.T., 1940–1945. President, California Institute of Technology, 1946–1968. Science adviser to the President, and director, Office of Science and Technology, 1969– .

[33] F. Wheeler Loomis. Professor and head, Department of Physics, University of Illinois, 1929–1957; director, Control Systems Laboratory, 1952–1959; emeritus professor, 1959– . Associate director, Radiation Laboratory, M.I.T., 1941–1946; director, Lincoln Laboratory and Project Charles, 1951–1952; member, Lexington Project, 1948; U.S. Air Force Project Paris, 1954.

[34] Ivan Alexander Getting. Radiation Laboratory, Massachusetts Institute of Technology, 1940–1945; associate professor, 1945–1946; professor, electrical engineering, 1946–1951. On leave, 1950–1951, to serve U.S. Air Force, Assistant for Development Planning, Deputy Chief of Staff Development. Vice-president, Research and Engineering, Raytheon Company, 1951–1960. President Aerospace Corporation, El Segundo, California, 1960–

[35] Luis W. Alvarez. Physics department, University of California 1936–1945; professor of physics, 1945– ; associate director, Lawrence Radiation Laboratory, 1954–1959. In radar research and development, Massachusetts Institute of Technology, 1940–1943; Los Alamos, 1944–1945. 1968 Nobel Laureate in Physics.

[36] Robert F. Loeb. Director of Medical Services, Presbyterian Hospital, New York, 1947–1960. Associate professor, College of Physicians and Surgeons, 1930–1938; professor of medicine, 1942–1947; Bard Professor, 1947–1960, emeritus; acting Regius Professor of Medicine, University of Oxford, 1962. Vice-chairman, Division of Medicine, National Research Council, 1943–1944.

[37] Cherwell, 1st Viscount of Oxford, cr. 1956; 1st Baron, cr. 1941; Frederick Alexander Lindemann, 1886–1957. Professor of Experimental Philosophy, Oxford, Fellow of Wadham College, 1919, and again professor, 1953–1956. Experimental pilot, director of Physical Laboratory of Royal Air Force, Farnborough. Personal assistant to Prime Minister in 1940; Paymaster General, 1942–1945 and 1951–1953. Author, *The Physical Significance of the Quantum Theory.*

[38] Rear Admiral Julius Augustus Furer, 1880–1963. Coordinator of Research and Development, Navy Department, 1941–1945; retired from active duty, November 1945; recalled to active duty, 1951– . Author, *Administration of Navy Department in World War II*, published in 1959. Invented submersible pontoons for raising sunken ships, 1915, adopted as standard salvage equipment for submarine bases.

[39] Henry L. Stimson, 1867–1950. Secretary of State, 1929–1933; Secretary of War, 1940–1945. Author (with McGeorge Bundy), *On Active Service in Peace and War.*

[40] Frederic Sutterle Gordon, 1887–1951. First vice-president, director, and member Executive Committee, U.S. Pipe and Foundry Company. Director, Sloss Sheffield Steel and Iron Company. Vice-president, trustee, New York Museum of Science and Industry.

[41] Paul A. Scherer. Engineer, holder of patents in the field of refrigeration. In the early days of World War II, director of research for the AiResearch Manufacturing Company of Los Angeles, working on heat transfer problems for aircraft. Head, Engineering and Transition Office of the N.D.R.C. Following the war, made executive officer, Carnegie Institution of Washington, until his retirement in 1957. Served as associate director for administration, National Science Foundation, until 1963.

[42] Alan T. Waterman, 1892–1967. Associate professor of physics, Yale University, 1931–1948. Vice-chairman, Division D, National Defense Research Committee, 1942–1943. Deputy chief, Office of Field Services, O.S.R.D., 1943–1945, chief, 1945. Deputy chief and chief scientist, Office of Naval Research, Navy Department, 1946–1951. Director, National Science Foundation, 1951–1963.

[43] Brigadier General Raymond G. Moses. Served February 1942–August 1943, Office, Chief of Staff, United States Army; September 1943–February 1946, 1st United States Army, 12th Army Group, and later 15th Army, Europe. March 1946–October 1946, Office, Chief of Engineers, Washington, D.C.; 1946–1948, New England Division, Office, Chief of Engineers, Boston. Retired from active service in 1949.

[44] Rear Admiral Willis A. Lee, Jr., 1888–1945. In 1942 assigned as Chief of Staff to Commander in Chief, U.S. Fleet, and after six months sent to the Pacific theater, serving consecutively as Commander Battleship Division 6, Commander Battleships, Pacific Fleet (with additional duty in command of Division 6), and as Commander Battleship Squadron 2. Awarded Navy Cross for extraordinary heroism as commander of a task force in action against Japanese surface vessels off Guadalcanal, November 14–15, 1942. Decorated further for successful operations against Japanese forces from April through June 1944. Advanced to Vice-Admiral, 1944. Died of coronary thrombosis while still in command of Squadron 2.

[45] Detlev Wulf Bronk. Coordinator of Research, Air Surgeon's Office, Headquarters Army Air Forces, 1942–1946. Director, Johnson Research Foundation, University of Pennsylvania, 1929–1949; chairman, National Research Council, 1946–1950. President, National Academy of Sciences, 1950–1962. President of Johns Hopkins University, 1949–1953; president of Rockefeller University, 1953–1968. Chairman, National Science Board, National Science Foundation, 1956–1964. Member, President's Science Advisory Committee, 1956–1963.

[46] Hartley Rowe, 1882–1966. For many years chief engineer of the United Fruit Company. During World War II, chief of Division 12 of the N.D.R.C., Transportation and Naval Architecture. Also technical adviser to Supreme Commander, A.E.F., England and France, 1944. Consultant to the Manhattan District, Los Alamos, 1944–1945. Member, General Advisory Committee, A.E.C., 1946–1950.

[47] Major General Anthony C. McAuliffe. Famous as the defender of Bastogne in the Battle of the Bulge (December 1944) when he replied "Nuts" to the German demand for surrender. Later, Army secretary of the Joint Research and Development Board (1946), and deputy director for Research and Development of the Logistics Division of the General Staff (1947). Made chief of the Army Chemical Corps, December 1949. Retired June 1, 1956, with rank of general. His last staff position was Commander in Chief, U.S. Army, Europe. Director, American Cyanamid Company, 1960–1963.

[48] Captain Carroll L. Tyler. Graduated from the United States Naval Academy in 1919. Advanced through the ranks; made captain in April 1941. In June 1938 became gunnery officer of the U.S.S. *San Francisco*, and in this capacity he was present at Pearl Harbor during the Japanese attack, December 7, 1941. He became assistant director and later director of the Research and Development Division of the Bureau of Ordnance and special assistant to the director of the Office of Research and Development. It was in this last capacity that he played a key role in the development of the proximity fuze. He retired in September of 1947.

[49] Commander William Sterling Parsons, 1901–1953. Graduated from the U.S. Naval Academy in 1922, advanced to the rank of rear admiral, 1948. In May 1942 he was made special assistant to the director of O.S.R.D. in connection with the development of the proximity fuze. Later assigned to the Los Alamos Laboratory where he became, ultimately, officer in charge of the Overseas (Tinian, Marianas) Technical Group of the Los Alamos Scientific Laboratory. As weaponeer he flew from Tinian to Hiroshima in the bomber that dropped the atomic bomb on Hiroshima, August 6, 1945. He completed the assembly of the bomb in the bomb bay during flight. For this service he was awarded the Distinguished Service Medal by the Navy and the Silver Star Medal by the War Department.

[50] Major General Leslie Richard Groves. Deputy chief for construction, Corps of Engineers, 1941; headed Manhattan Engineer District (Atomic Development Project), 1942–1947; retired, 1948, with the rank of lieutenant general. Vice-president, Remington division, Sperry Rand Corporation, 1948–1961. Author of *Now It Can Be Told, The Story of the Manhattan Project.*

[51] Niels Henrik David Bohr, 1885–1962. Director of the Institute for Theoretical Physics, Copenhagen, pioneer in the advance of nuclear physics in the thirties. With J. A. Wheeler of Princeton worked out the theory of nuclear fission that became the basis for the work in atomic energy. During World War II served as consultant at Los Alamos. Winner of the 1922 Nobel prize in physics for his work on the structure of the atom. First winner (1957) of the Atoms for Peace Award.

[52] Otto Robert Frisch. Nephew and collaborator of Lise Meitner. Since 1947 Jacksonian Professor of Natural Philosophy, University of Cambridge. Member of the Liverpool nuclear physics group which in 1943 was moved into the American atomic research establishment at Los Alamos. Author numerous works, including *Meet the Atoms, Atomic Physics Today, Working with Atoms.*

[53] Lise Meitner, 1878–1968. Head of the nuclear physics department of the Kaiser Wilhelm Institute (1917–1938), where she conducted experiments on radioactivity with Otto Hahn. Escaped from the Germans into Copenhagen, where she joined her nephew, Otto Frisch, and Niels Bohr. While repeating experiments of Hahn and Strassmann, she demonstrated that nuclear fission had occurred when she bombarded uranium 238 and obtained the nuclei of barium and krypton.

[54] Otto Hahn, 1879–1968. Studied radioactivity under Sir William Ramsay at University College, London. Later spent a year with Ernest Rutherford at McGill University, Montreal. Joined the staff of the Kaiser Wilhelm Institute for Chemistry in 1912, becoming its director in 1928. He was joined there by Lise Meitner. With his younger associate, Friedrich Strassmann, he discovered that the atom could be split, a discovery for which he received the 1944 Nobel prize in chemistry. He remained in his laboratory until 1945, when he was interned by the British and held in England until January 1, 1946. In 1966 he shared the Enrico Fermi Award from the U.S. Atomic Energy Commission with Dr. Meitner and Dr. Strassmann.

[55] Friedrich Strassmann. Head of Chemistry Department, Kaiser Wilhelm Institute; professor of inorganic and nuclear chemistry and director, Chemistry Institute, University of Mainz, 1946; director, Chemical Department, Max Planck Institute for Chemistry, 1953. Published technical studies with Lise Meitner and Otto Hahn on radioactive isotopes from uranium and thorium, 1934–1938.

[56] Enrico Fermi, 1901–1954. Professor theoretical physics, University of Rome, 1927–1938; professor of physics, Columbia University, 1939–1942. Research, atomic bomb project, University of Chicago, 1942–1945; professor of physics, 1946– . Winner of the 1938 Nobel prize in physics "for his demonstrations of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow electrons."

[57] Lyman J. Briggs, 1874–1963. Director, National Bureau of Standards, 1933–1945; retired, 1945. Member, National Advisory Committee for Aeronautics, 1933–1945; vice-chairman, 1942–1945; chairman subcommittee on aircraft structures, 1937–1945. President, National Conference on Weights and Measures, 1935–1945.

[58] Robert Emmet Sherwood, 1896–1955. Playwright. Special assistant to Secretary of War, 1940; to Secretary of the Navy, 1945. Served as director, Overseas Branch Office of War Information; resigned in September 1944. Author of many successful plays including *The Road to Rome, Reunion in Vienna, The Petrified Forest, There Shall Be No Night.*

[59] Ernest Orlando Lawrence, 1901–1958. Professor of physics, University of California, 1930– . Director, University of California Radiation Laboratory at Berkeley, 1936– . Awarded the 1939 Nobel prize in physics "for the invention and development of the cyclotron and for the results obtained by its aid, especially with regard to artificially radioactive elements." 1957 recipient of the Enrico Fermi award for his contributions to nuclear physics and atomic energy.

[60] Alfred Loomis. President, Loomis Institute for Scientific Research, Inc., 1928–1965; chief, Division 14 (Radar), National Defense Research Committee, 1940–1947. Trustee, Carnegie Institution of Washington.

[61] General George Catlett Marshall, 1880–1959. Chief of Staff, 1939–1945; General of the Army, 1944. Secretary of State, 1947–1949; Secretary of Defense, 1950–1951.

[62] Henry Agard Wallace, 1883–1965. Editor and agriculturist. Secretary of Agriculture, 1933–1940; Vice-President of United States, 1941–1945; Secretary of Commerce, 1945–1946; candidate for President, 1948. Author of books on agriculture and technology.

[63] Lieutenant General Wilhelm Delp Styer. A graduate of the U.S. Military Academy. At the outbreak of World War II he was Deputy Chief of the Construction Division, Office of the Quartermaster General, and from 1942–1945 was Deputy Commander and Chief of Staff, U.S. Army Service Forces. From 1942–1945 he served as War Department liaison officer to the S-1 Section, Bureau of Standards, and as Army member of the Military Policy Committee appointed by the Secretary of War

to provide policy supervision over and assist in the development and production of the atomic bomb. In 1945–1946 General Styer was Commander, U.S. Army Forces of the Western Pacific. He retired in 1948.

[64] Rear Admiral William R. Purnell, 1886–1955. In the early days of World War II assigned to duty as Chief of Staff and aide to the commander, U.S. Naval Forces, Southwest Pacific. During April and May 1942, in command of the Anzac Forces and U.S. Naval Forces, Western Australia. In June 1942, assigned duty with the Commander in Chief, U.S. Fleet. Later served as Assistant Chief of Staff and Deputy Chief of Staff. In October 1943, appointed Assistant Chief of Naval Operations (Materiel Command) Navy Department. Retired 1946.

[65] Glenn T. Seaborg. Since 1961, chairman, U.S. Atomic Energy Commission. Nobel prize in chemistry (with E. M. McMillan) in 1951 "for their discoveries in the chemistry of the transuranium elements." Associated with the department of chemistry and with the Radiation Laboratory, University of California at Berkeley, from 1937; associate director, Radiation Laboratory, 1954–1961; chancellor, University of California at Berkeley, 1958–1961.

[66] James Forrestal, 1892–1949. Investment banker, president, Dillon, Read & Co. One of President Roosevelt's "bright young men with a passion for anonymity," he had served in that post less than two months when he was named (August 1940) to the newly created post of Undersecretary of the Navy. He was credited with the prompt execution of contracts that led to a greatly expanded Navy. In 1944 he succeeded Knox as Secretary of the Navy, and in 1947 he became first Secretary of Defense.

Chapter III

[1] Robert Andrews Millikan, 1868–1953. Noted American physicist and educator. Awarded the Nobel prize in physics in 1923 "for his work on the elementary charge of electricity and on the photoelectric effect."

[2] J. P. Morgan, 1837–1913. American financier, philanthropist, and art collector; founder of J. P. Morgan and Company and organizer of the United States Steel Corporation.

[3] Ralph Davenport Mershon, 1868–1952. Inventor, consulting engineer, specializing in power generation and transmission. A member of the Officers Reserve Corps, he served as major and lieutenant colonel, Engineers, U.S. Army, 1917–1919. He was detailed to the Naval Consulting Board.

[4] Michael Idvorsky Pupin, 1858–1935. American physicist and inventor (born in Yugoslavia). Professor of electromechanics, Columbia Uni-

versity, 1901–1930. Patented the "loaded line" which made possible long-distance telephony. His book, *From Immigrant to Inventor* (Scribner, 1923), won the 1924 Pulitzer Prize.

[5] George Ashley Campbell, 1870–1954. Telephone research engineer. With American Telephone and Telegraph from 1897–1934. With Bell Telephone Laboratories, 1934–1935. Pioneering research in connection with loading, crosstalk, 4-wire repeater circuits, sidetone reduction electric wave filters, Fourier integrals and electrical units. Author, *Collected Papers*, 1937.

[6] Ernest Fox Nichols, 1869–1924. American physicist, president of Dartmouth College, 1908–1916; elected president of M.I.T., 1921, but unable to serve because of ill health. Known for his experiments in measuring planetary heat, determining light pressure, etc., by means of a radiometer. Died while delivering a scientific paper at the National Academy of Sciences.

[7] John Torrence Tate, 1889–1950. American physicist, University of Minnesota, 1914 until his death. Dean, College of Science, Literature and Arts, 1937–1943. Professor of physics, 1943–1950. Chief, Division 6 (Undersea Warfare), National Defense Research Committee, 1941–1945.

Timothy Edward Shea. Director of Research, Columbia University Division of War Research, 1941–1945. Since 1945 he has held executive positions with Western Electric Co., AT&T Co., and Bell Telephone Laboratories. Now vice-president, Engineering, Western Electric Company, New York.

T. Keith Glennan. Director of U.S. Navy Underwater Sound Laboratory, New London, Conn., under Columbia University Division of War Research, 1942–1945. Subsequently president of Case University, 1947–1965. President, Associated Universities, Inc., since 1965.

[8] Captain Robert A. Lavender. Career Naval Officer on active duty from 1912 until 1939 when he was retired. Recalled to duty in October 1941 and assigned by the Secretary of the Navy to the O.S.R.D. as adviser on patent matters. By Presidential directive of January 1, 1947, made available to the Atomic Energy Commission as adviser on patent matters. Captain Lavender's medals and awards include the Navy Cross "for distinguished service in the line of his profession as a member of the crew of the airplane NC-3, which made a long overseas flight from Newfoundland to the vicinity of the Azores in May 1919."

[9] Bennett Archambault. Head, London Mission of O.S.R.D., 1942–1945. Vice-president and general manager of the M. W. Kellogg Co., New York City, 1946–1954. Later, president and director of the Stewart-Warner Corporation; since 1959 Chairman of the Board.

[10] Sir Charles Seymour Wright. Director of Scientific Research, Admiralty, 1934–1946; Chief of Royal Naval Scientific Service, 1946–1947.

Contractor for Defence Research Board of Canada, 1956. Now lecturer in geophysics, Institute of Earth Sciences, University of British Columbia, Canada.

[11] Vice-Admiral Cedric Swinton Holland, 1889–1950. Served in the European war, 1914–1918. British naval attaché, Paris, 1938–1940. Then captain, he was sent by Admiral Somerville of the Royal Navy, by way of destroyer, on July 3, 1940, to Oran to negotiate with the Free French for the disposition of French warships in that port. In a brief engagement one French ship was blown up and two others seriously damaged, negotiations having been unavailing. From 1940 to 1941 Captain Holland commanded HMS *Ark Royal.* He was director of the Signal Department, Admiralty, 1942–1943. He retired from the Royal Navy in 1946.

[12] Cecil Scott Forester, 1899–1966. English novelist, author: *Horatio Hornblower, The African Queen, The Naval War of 1812, Hunting the Bismarck.*

[13] Sol Hurok. Impresario of many famous stars and performing groups including Anna Pavlova, Feodor Chaliapin, Isadora Duncan, the Russian Ballet, the Old Vic, the Sadler's Wells Ballet, and in more recent years, Comédie Française, Moiseyev Folk Ballet, Bolshoi Ballet.

[14] Admiral Ernest J. King, 1878–1956. Detached from the Naval Academy in 1898 to serve as a midshipman in the Spanish-American War, he was graduated in 1901. Advancing through the grades, he was assistant to the chief of staff of the commander in chief, U.S. Atlantic Fleet, in World War I. In the mid-1920's, while commanding the submarine base at New London, Conn., he was awarded the D.S.M. for the successful raising of the sunken S-51 in 1925. He qualified as a naval aviator in 1927, subsequently serving as chief of the Bureau of Aeronautics with the rank of rear admiral. Named commander in chief of the United States Fleet December 20, 1941, he became, in addition, chief of naval operations March 9, 1942, so serving for the rest of World War II. He was promoted to the newly created rank of Admiral of the Fleet on Dec. 17, 1944.

[15] Rear Admiral Samuel Eliot Morison. Historian. Lt. commander to rear admiral, U.S. Naval Reserve, 1942–1951; historian, U.S. Naval Operations, World War II. Recipient Columbia University Bancroft prize, 1949. Winner of two Pulitzer Prizes: *Admiral of the Ocean Sea*, 1942; *John Paul Jones*, 1959. Author also of *The Oxford History of the American People*, 1965, and numerous other works.

[16] Edward Lindley Bowles. Professor of electrical engineering at M.I.T. from 1937, consulting professor, School of Industrial Management, 1952–1963, professor emeritus, 1963– . A charter member of the Microwave Section, N.D.R.C., 1940–1942, he served as consultant to the Secretary of

War, 1942–1947; consultant, communications and radar, U.S.A.A.F., 1943; scientific consultant, U.S.A.F., 1947–1951; and scientific warfare adviser, Weapons Systems Evaluation Group, Office of the Secretary of Defense, 1950–1952.

[17] Rear Admiral Francis S. Low, 1894–1964. Appointed to conduct special survey of U.S. Navy's antisubmarine program. Vice Admiral, 1947. Deputy Chief of Naval Operations (logistics), 1951–1954. Commander, Western Sea Frontier, 1953.

[18] Admiral Karl Doenitz. Commanded the German submarine force until January 1943, when he was promoted to the rank of Grand Admiral and became Commander in Chief of the Navy. Served in that position until the end of the war. Tried by the Interallied Tribunal at Nuremberg and sentenced to prison, from which he was released in 1956.

[19] Rear Admiral Daniel Vincent Gallery. Commander USS *Guadalcanal* which captured German submarine U-505 off Cape Blanco, French West Africa, at sea, June 1944, the only warship captured at sea by U.S. Navy since 1815. Assistant Chief of Naval Operations for Guided Missiles, 1946. Commander, Carrier Division, U.S. Mediterranean Fleet, 1951, hunter-killer force, Atlantic Fleet, 1952. Retired 1960. Author of *Clear the Decks; Twenty Million Tons Under the Sea; Hear This; Eight Bells and All's Well; Stand By to Start Engines; The Brink.*

[20] Edward Mills Purcell. Associated with the physics department of Harvard since 1938. Group leader of Fundamental Developments Group, Radiation Laboratory, M.I.T., 1941–1945. Since 1960 Gerhard Gade University Professor, Harvard. With Felix Bloch, awarded the 1952 Nobel prize in physics "for their development of new methods for nuclear magnetic precision measurements. . . ."

[21] Philip M. Morse. Headed the N.D.R.C.'s Antisubmarine Warfare Operations Research Group (ASWORG, 1942, later renamed Operations Research Group with subgroups in other fields of warfare). Also chairman of the National Research Council's Committee on Sound Control in Combat Vehicles; supervisor, Sound Control Laboratory, Harvard, 1939–1945; director, Brookhaven National Laboratory, 1946–1948; deputy director, Weapons Systems Evaluation Group, National Military Establishment, 1949–1950. With M.I.T., Department of Physics, since 1931. From 1958, director of Operations Research Center.

[22] Warren Weaver. Mathematician, teacher, foundation executive, author. With department of mathematics, University of Wisconsin, 1920–1932, professor and chairman of department, 1928–1932. Director, Division of Natural Sciences, General Education Board, 1932–1937. Director of Division of Natural Sciences, Rockefeller Foundation, 1932–1955; vice-president for Natural and Medical Sciences, 1955–1959. He was chief of

the Applied Mathematics Panel of the N.D.R.C., and later was chairman of the Basic Research Group of the Research and Development Board, Department of Defense. Associated with the Sloan-Kettering Institute on Cancer Research from 1951–1959; vice-president, 1958–1959. Trustee, Alfred P. Sloan Foundation, 1956– ; vice-president, 1959–1964; consultant, science affairs, 1964– . Member, National Science Board, National Science Foundation, 1956–1960. Author, *The Mathematical Theory of Communications* (with C. E. Shannon); *Lady Luck—The Theory of Probability; Alice in Many Tongues; Scenes of Change*, and other works.

[23] John von Neumann, 1903–1957. Mathematician, principal architect of the "game theory" approach to economic behavior, social organization and strategy in war; leader in the development and construction of high-speed electronic computer. With Oskar Morgenstern wrote *Theory of Games and Economic Behavior*. Visiting professor, Princeton University, 1930–1933; professor of mathematics, Institute for Advanced Study, 1933–1945; director, Electronic Computer Project at the Institute, 1945–1955. Member of the United States Atomic Energy Commission from 1945 until his death in 1957. Also author of *Mathematical Foundations of Quantum Mechanics*.

[24] The Rt. Honorable, The Lord Blackett (Patrick Maynard Stuart Blackett). Fellow of King's College, 1923–1933, Honorary Fellow, 1949; professor of physics, Birkbeck College, 1933–1937; Langworthy Professor of Physics, University of Manchester, 1937–1953. Since 1965 professor emeritus and senior research fellow, Imperial College; deputy chairman and scientific adviser (part-time), Advisory Council on Technology, Ministry of Technology, since 1964. President of Royal Society since 1965. Awarded Nobel prize for physics, 1948.

[25] J. Robert Oppenheimer, 1904–1967. Professor of physics, University of California, 1936–1947. Director, Los Alamos Scientific Laboratory, 1943–1945; director and professor of physics, Institute for Advanced Study, Princeton, from 1947.

[26] General George Smith Patton, Jr., 1885–1945. Commanded U.S. Forces on West Coast, Morocco, North Africa, November 1942; Commander, Central Sector, American Forces in Tunisia, March 1943; commanded 7th Army in Sicily, July 1943; organized and commanded desert training center, March 1944; Commander 3rd Army in France, Belgium, Luxembourg, Germany, August 1944; Commander 15th Army, France, October 1945. Author of *War as I Knew It*, 1947.

[27] General Henry H. Arnold, 1886–1950. Pioneer in Army aviation. Became Chief of Army Air Corps, 1938; appointed Deputy Chief of Staff for Air, 1940; became Commanding General, Army Air Forces, 1942; General of the Army, December 1944. (The U.S. Air Force was established under the Armed Services Unification Act of July 1947.)

[28] Major General Stephen G. Henry. Graduate, Louisiana State University. Attended M.I.T., 1922. Attended Tank School, Infantry School, Command and General Staff School, Army War College. Advanced through grades to major general, 1943; retired, 1946. Established Armored Force School, Fort Knox, Kentucky, 1940. Established and operated Research and Development Division, War Department General Staff. Became Assistant Chief of Staff for Personnel.

[29] General Alfred M. Gruenther. Chief of Staff, Allied Forces Headquarters, 1942–1943; Chief of Staff, Fifth Army, 1943–1944; Chief of Staff, 15th Army Group, 1944–1945; Deputy Commander, U.S. Forces in Austria, 1945; Deputy Commandant, National War College, Washington, 1945–1947; Director, Joint Staff, Joint Chiefs of Staff, 1947–1949; Deputy Chief of Staff for Plans and Operations, Army General Staff, 1949–1951; Chief of Staff SHAPE, 1951–1953; Supreme Allied Commander in Europe, 1953–1956. Retired, 1956. President of American Red Cross, 1957–1964.

[30] General Jacob Loucks Devers, Chief of Armored Force, 1941–1943; Commanding General, European Theatre of Operations, 1943; Deputy Supreme Allied Commander, Mediterranean, 1944; Commanding General, Army Ground Forces, 1946–1948; Chief, Army Field Forces, 1948–1949; retired, September 1949.

[31] Major General Albert Whitney Waldron, 1892–1961. A graduate of the United States Military Academy, he advanced through the ranks, retiring as major general in 1946. At the outset of World War II, assigned to the South Pacific, where he was wounded in action December 1942. He was then reassigned to Washington and designated Chief, Ground Requirements Section at Headquarters Army Ground Forces. In February 1945 he was named to the executive committee of the short-lived Research Board for National Security established by the National Academy of Sciences. The citation that accompanied the Distinguished Service Medal awarded him in 1945 noted especially that "He coordinated the orderly development of the separate arms and of weapons and equipment, and recommended changes in warfare techniques, in accordance with this development." Following his disability retirement, brought on by wounds suffered in the Pacific engagement, he served as manager of the Veterans Hospital in Palo Alto, California, from 1946–1952.

[32] Lt. General Brehon Burke Somervell, 1892–1955. Chief of Construction Division, Quartermaster Corps, 1940–1941. Assistant Chief of Staff in Charge of Supply Division, 1941–1946; retired, 1946.

[33] Palmer C. Putnam. President and Chairman of Board, G. P. Putnam's Sons, 1930–1932. Research on wind turbine, 1935–1941. With O.S.R.D., 1941–1945; as technical aide to Division 12, N.D.R.C., had active role in the development of the Dukw, Weasel, and other devices. Assigned to various theaters of war, on loan to the Joint Chiefs of Staff, to help

develop techniques of amphibious assault; loaned, also, to Admiral Mountbatten, Supreme Allied Commander, SEAC, 1943–1944; special assistant to director, O.S.R.D., 1944–1945. Special consultant to the Atomic Energy Commission, 1949. Author, *Power from the Wind, Energy in the Future.*

[34] William Franklin Knox, 1874–1944. Newspaper publisher. Republican nominee for Vice-President of the United States, 1936. Named by President Roosevelt to be Secretary of the Navy, 1940–1944.

[35] Merle A. Tuve. Staff member, Department of Terrestrial Magnetism, Carnegie Institution of Washington, 1926–1946, director, 1946–1966; distinguished service member, 1967– ; on leave, 1940–1946, for war work. Chairman, Section T, O.S.R.D. (proximity fuze), 1940–1945. Director, Applied Physics Laboratory, Johns Hopkins University (Navy), 1942–1946. Member, Executive Committee, U.S. National Committee for International Geophysical Year; member of President's Science Advisory Committee, International Science Panel; member, U.S. National Commission for UNESCO; member, Committee on Growth, National Research Council; chairman, Geophysics Research Board, N.R.C. Home secretary, National Academy of Sciences, since 1965.

[36] Lawrence Randolph Hafstad. In the O.S.R.D., vice-chairman, Section T (proximity fuzes), and consultant to other sections and divisions of the N.D.R.C. From research physicist to director, Johns Hopkins University Applied Physics Laboratory, 1942–1947. Director, Nuclear Reactor Development Division, A.E.C., 1949–1955. 1955–1968, vice-president for research, General Motors Corporation. Since 1968, chairman, Committee on Undersea Warfare, National Research Council.

[37] Richard Brooke Roberts. Physicist, Applied Physics Laboratory, Johns Hopkins University, 1943–1946; staff member, Department of Terrestrial Magnetism, Carnegie Institution of Washington, 1947–1953; chairman, Biophysics Section, 1953–1963; staff member, 1963– .

[38] Sir John Douglas Cockcroft, 1897–1967. Jacksonian Professor Natural Philosophy, University of Cambridge, 1939–1946. Chief superintendent, Air Defense Research and Development Establishment, Ministry of Supply, 1941–1944; director, atomic energy division, National Research Council of Canada, 1944–1946; director, Atomic Energy Research Establishment, 1946–1958; member, Atomic Energy Authority, 1958– . Chancellor, Australian National University, 1961–1965. Awarded Nobel Prize in physics (with E. T. S. Walton), 1951.

[39] General Joseph T. McNarney. Appointed Deputy Chief of Staff, U.S. Army, 1942; appointed Deputy Supreme Commander in Chief, Mediterranean and Commanding General, U.S. Mediterranean Theatre of Operations, October 23, 1944; Acting Allied Supreme Commander in Mediterranean area, October 1, 1945; Commander U.S. Forces in Europe, November 1945. Appointed Army Air Force representative, Military Staff

Committee, United Nations, 1947; Commanding General, Materiel Command, Wright Field, Dayton, Ohio, 1947. Retired 1952. President, Convair, 1952; also, after merger in 1954, senior vice-president, General Dynamics Corp., holding both posts until 1958.

[40] General Walter Bedell Smith, 1895–1961. U.S. secretary, Combined Chiefs of Staff, and Secretary, U.S. Joint Chiefs of Staff, 1941–1942. Chief of Staff, Allied Forces Headquarters, North Africa, 1942–1944, SHAEF, ETO, 1944–1945; U.S. Forces European Theatre, June–December 1945; U.S. Ambassador, Union of Soviet Socialist Republics, 1946–1949; director of Central Intelligence Agency, 1950–1953; Undersecretary of State, 1953–1954; vice-chairman, American Machine and Foundry Co., 1954– .

[41] Samuel Abraham Goudsmit. Professor of physics, Northwestern University, 1946–1948. On leave to Radiation Laboratory, M.I.T. and England, 1942–1946. Senior scientist, Brookhaven National Laboratory, since 1948. Detailed to War Department as chief of Scientific Intelligence Mission in Europe, May 1944–December 1945. Author of *ALSOS*, 1947.

[42] Colonel Boris T. Pash. In World War II, chief Counter Intelligence Division, 4th Army and Western Defense Command, and in charge of security for Manhattan Project installations in western United States. In 1943 directed to organize and command War Department Scientific Intelligence Task Force for the purpose of obtaining intelligence on Nazi atomic capability (ALSOS). Chief, Foreign Liaison Section on General MacArthur's staff in Tokyo, 1946–1947. Following a detail to C.I.A. and other military assignments, retired from military service in 1957 because of wartime injuries. From 1958 to 1963, division chief, Department of Army Technical Intelligence. Author of *The ALSOS Mission*, 1969.

[43] Colonel Charles P. Nicholas. Commissioned 2nd lieutenant, Field Artillery, U.S. Army, 1925; advanced through grades to colonel, 1942; assigned U.S. and overseas stations, 1925–1942; General Staff Corps, 1942–1943; War Department General Staff, 1943–1946; deputy assistant director (member initial organizing group), Central Intelligence Agency, 1946–1948; member faculty U.S. Military Academy, 1930–1935, 1941–1942, 1948–1967; professor of mathematics, 1948–1967; head of department, 1959–1967.

[44] James Phinney Baxter III. President of Williams College, 1937–1961; president emeritus, 1961– . Director, research and analysis, for Coordinator of Information, Washington, 1941–1942; deputy director, Office of Strategic Services, 1942–1943; historian, Office of Scientific Research and Development, 1943–1946. Author of *Scientists Against Time*, 1946, winner 1947 Pulitzer Prize in history.

[45] Edward L. Moreland, 1885–1951. Graduate of Johns Hopkins University and M.I.T. Member of firm, Jackson & Moreland, 1919– .

Head, Department of Electrical Engineering, M.I.T., 1935–1938; Dean of Engineering, 1938–1946; executive vice-president, 1946– . Executive Officer, N.D.R.C., 1942–1945; expert consultant to Secretary of War, assigned G.H.Q., Armed Forces in the Pacific; chief, scientific survey in Japan, 1945.

Chapter IV

[1] Geoffrey Nathaniel Pyke, 1894?–1948. British inventor and educator about whom facts are rather scarce. During the Spanish Civil War, he conducted a secret survey of popular opinion in Nazi Germany. In World War II, with the backing of Lord Mountbatten, he pushed the development of the snow vehicle, Weasel, and a more ambitious scheme for the construction of mobile air bases out of a substance known as pykrete, a mixture of wood pulp and ice.

[2] Wilfrid Eggleston. Canadian journalist. Born in England, educated in Canada. During World War II, director of Censorship. Most recently, professor and director, Department of Journalism at Carleton University in Ottawa. Author of *Scientists at War*, *Canada at War*, *Frontier and Canadian Letters*, *The Queen's Choice*.

[3] Leaders in this notable group were Roderick Stephens and Roger Sherman Warner, Jr. Roderick Stephens, of the New York firm of naval architects, Sparkman & Stephens, Inc., had a key role in the design of the Weasel. He and his older brother, Olin II, also had a leading part in the design of the 2½-ton amphibious truck Dukw. Both brothers received citations from the United States Government for this work, and Roderick and Roger Warner, Jr., received a special commendation from Lord Mountbatten for their work in training the drivers of the British Army's Royal Army Service Corps to handle the Dukw.

Roger Sherman Warner, Jr., served as technical aide, project engineer, Division 12, N.D.R.C., 1942–1944; as coordinating engineer, division head, Manhattan Project, Los Alamos, 1944–1947; and as director of engineering, 1947–1949. Later he was member of the staff, Advanced Research Projects Agency, Office of the Secretary of Defense, and manager, Advanced Systems Astroelectronics Projects Division, RCA.

[4] Major General Earl Seeley Hoag, U.S.A.F., as 1st lieutenant, commanded Aviation Section, Signal Officers Reserve Corps, January 1918. He became captain, Air Service, U.S. Army, October 1918, and advanced through grades to major general, 1948. Member, War Department General Staff, July 1940–March 1943. Commanding General, Africa-Middle East Wing, India-China Wing, European Division, Air Transport Com-

mand, 1943–1945; Deputy Assistant Chief of Staff, G-4, January 1946; deputy director, Research and Development Division, War Department General Staff, October 1946; chief of Air Force Group, American Mission for aid to Turkey, 1947; special assistant to Chief of Staff, U.S.A.F., for Reserve Forces, 1950; director, Joint Air Transportation Board, 1951.

[5] Marshal of the Royal Air Force Sir Arthur Travers Harris. Air Vice-Marshal, 1939; Air Marshal, 1941; Air Chief Marshal, 1943; Deputy Chief of Air Staff, 1940–1941; head of Royal Air Force delegation to U.S.A., 1941; commander in chief, Bomber Command, 1942–1945; Marshal of the R.A.F., 1945. Managing director, South African Marine Corporation, 1946–1953.

[6] Ralph McAllister Ingersoll. Vice-president and general manager, Time Inc., publishing *Time*, *Life*, *Fortune*, and the *Architectural Forum*, 1935–1939. Resigned to organize and finance company to publish *PM*. Enlisted as private engineer, Amphibian Command, U.S. Army, 1942, advanced to lieutenant colonel, General Staff Corps; served overseas, 1943–1945, in Africa, England, Italy, France, Belgium, and Germany on staffs of General Jacob Devers, Field Marshal Montgomery, and General Omar Bradley.

[7] John Taber, 1880–1965. Member 68th–87th Congresses from 36th and 38th New York Districts. Chairman, Committee on Appropriations, 1947–1948 and 1953–1954. Member of Special Committee on Government Reorganization.

[8] Irving Langmuir, 1881–1957. American chemist long associated with the General Electric Company of Schenectady, New York. Developed gas-filled tungsten electric lamp, electron discharge apparatus, a high-vacuum pump; with Gilbert N. Lewis originated the Lewis-Langmuir atomic theory; awarded the 1932 Nobel prize for chemistry for his work in surface chemistry.

[9] Nelson Aldrich Rockefeller. Coordinator of Inter-American Affairs, 1940–1944; Assistant Secretary of State, 1944–1945; chairman, International Development Advisory Board (Point 4 program), 1950–1951. Governor of New York since 1958.

[10] Kenneth Douglas McKellar, 1869–1957. United States senator from Tennessee, 1916–1953.

[11] Admiral Chester William Nimitz, 1885–1966. Chief, Bureau of Navigation, Navy Department, 1939–1941; Commander in Chief, Pacific Fleet, 1941–1945; made Fleet Admiral, Navy, December 1944; Chief of Naval Operations, 1945–1947.

[12] Samuel Callaway. Joined the staff of the Carnegie Institution of Washington in 1919. Private secretary to three presidents of the C.I.W. successively: John C. Merriam, Vannevar Bush, and Caryl P. Haskins.

Retired from C.I.W. in 1958. Awarded the Presidential Certificate of Merit for an outstanding contribution to the nation during World War II as secretary to Dr. Bush in his capacity as director of the O.S.R.D.

[13] George Bogdan Kistiakowsky. With the chemistry department of Harvard since 1930, professor, 1937– . Chief of the explosives division, Los Alamos, 1944–1946. Returned to Harvard, 1946. Special assistant to the President for Science and Technology under President Eisenhower, 1959–1961. Vice-president, National Academy of Sciences, since 1965.

[14] Dr. Albert Baird Hastings. Professor of physiological chemistry, University of Chicago, 1926–1928; professor of biochemistry, Lasker Foundation for Medical Research, 1928–1935; Hamilton Kuhn Professor of Biological Chemistry, Harvard Medical School, 1935–1959; emeritus, 1959– ; member, Committee on Medical Research, O.S.R.D., 1941–1946; head, Laboratory of Metabolic Research, Scripps Clinic and Research Foundation, La Jolla, California, 1959–1966; member emeritus, 1966. Contributor to Oral History Program, National Library of Medicine.

[15] General Charles M. Mangin, 1866–1925. Commanded the French 10th Army in its counterattack at Villers-Cotterets (July 18, 1918), which halted the German advance.

[16] General Douglas MacArthur, 1880–1964. Commander in Chief, U.S. and Filipino Forces, during invasion of Philippines by Japanese, 1941–1942; Supreme Commander, Allied Forces in Southwest Pacific, 1942; appointed Supreme Commander to accept surrender by Japan, 1945; Commander Occupational Forces in Japan, 1945–1951; Commander in Chief, U.N. Forces in Korea, 1950–1951. General of the Army.

[17] Admiral William Frederick Halsey, 1882–1959. Graduated from the Naval Academy in 1904. After destroyer commands in World War I, served as naval attaché. Flight training thereafter. Commanded aircraft carrier *Saratoga*, 1935–1937, and in 1940 was placed in command Aircraft Battle Force, U.S. Pacific Fleet. In 1942 ferried General James H. Doolittle's Army bombers for their raid on Tokyo. Led forces defeating the Japanese in the Battle of Guadalcanal. Named Fleet Admiral 1944 and placed in command U.S. Third Fleet in the Pacific. Japanese surrender was signed aboard his flagship, U.S.S. *Missouri.* Retired 1947.

[18] Marc Andrew Mitscher, 1887–1947. Graduated from the U.S. Naval Academy, 1910; advanced through the grades to vice-admiral, 1944; associated with naval aviation since October 1915; served as pilot on first Navy transatlantic NC-4 flight, 1919. Commanding officer, U.S. carrier *Hornet*, 1941–1942, during Major General Doolittle's raid on Tokyo. In command of carrier *Midway* during Battle of Midway, June 1942; Commander, Air, Solomon Islands, April to July 1943; Commander Task Force 58 Pacific Fleet, 1944; Chief of Naval Operations for Air, 1945.

[19] Lt. Gen. Walter Krueger, 1881–1967. Joined U.S. Army as a private, 1898; commissioned 1901; advanced through ranks to lieutenant general, May 1941, and commander of 3rd Army. Appointed February 1943 to organize and command 6th Army in Australia.

[20] Admiral Thomas Cassin Kinkaid. Graduate of U.S. Naval Academy, 1908; advanced through grades to admiral. Commander cruiser group action off Bougainville, February 20, 1942; Salamua-Lae raid, March 10, 1942; Battle of Coral Sea, May 4–8, 1942; Battle of Midway, June 3–6, 1942; commander *Enterprise* carrier group, Guadalcanal-Tulagi landings, August 7–9, 1942; Battle of Eastern Solomons, August 25, 1942; Santa Cruz Islands, October 26, 1942; Guadalcanal, November 15, 1942; commander, North Pacific Force, in Aleutian campaign, January–October 1943; commander, 7th Fleet, and Commander Allied Naval Forces, Southwest Pacific Area, November 1943 to September 1945. Retired 1950.

[21] Admiral Jesse Barrett Oldendorf. Career naval officer; a graduate of the U.S. Naval Academy in 1909, he served with distinction in both World War I and World War II. During World War II he commanded forces, first in the Aruba-Curaçao area, and was later commander of the Naval Operating Base, Trinidad, West Indies. In 1943 he commanded the Task Force of the Atlantic Fleet and in January 1944 was ordered to the Pacific area as Commander, Cruiser Division 4. The following December he was designated Commander, Battleship Squadron One, with additional duty in command of Battleship Division Two. Admiral Oldendorf was honored by the Navy for his work in the Pacific and was decorated also by General Mark Clark "For service from 1 January to 15 February 1945 as Commander Battleships, U.S. 7th Fleet in the Luzon Operations . . ." He retired in September 1948 with the rank of admiral.

[22] Rear Admiral Clifton Albert Sprague, 1896–1955. Graduate of U.S. Naval Academy, 1917; advanced through grades to vice-admiral, 1951; commanded jeep carrier division at Leyte Gulf battle, October 1944; commander, Alaskan Sea Frontier; commandant, 17th Naval District; retired in 1951.

[23] Harvey Hollister Bundy, 1888–1963. Assistant Secretary of State, 1931–1933; special assistant to Secretary of War, 1941–1945. Member, Choate, Hall & Stewart, 1933–1941, 1945– .

Chapter V

[1] Orville Wright, 1871–1948. With his brother, Wilbur (1867–1912), made first successful flight in heavier-than-air mechanically propelled airplane on December 17, 1903, at Kitty Hawk, North Carolina.

[2] Warren Judson Mead, 1883–1960. Head, Department of Geology, M.I.T., 1934–1939; emeritus, 1949; distinguished consultant in economic engineering and geology.

[3] Joseph Christopher O'Mahoney, 1884–1962. Practiced law in Cheyenne, Wyoming, and Washington, D.C., 1920. First Assistant Postmaster General of the United States, 1933. United States senator from Wyoming, 1934–1953 and 1954–1961.

[4] Ralph D. Booth, 1899–1960. Electrical engineer long associated with the engineering firm of Jackson & Moreland, of which he became a partner in 1927, vice-president in 1955, and president in 1959. He was vice chairman of Division C (Communications and Transportation) of the N.D.R.C., as well as chairman of the subsection dealing with mechanical and electrical equipment.

[5] William R. Ransom. Professor of mathematics, Tufts College, 1900–1950, emeritus 1950– .

[6] Russell Grinnell, 1875–1948. With Grinnell Corporation, Providence, R.I., from 1898. President from 1925, director from 1906. Director, Automatic Fire Alarm Company, Firemen's Mutual Insurance Company, Gorham Manufacturing Company, Metals and Controls Corporation.

[7] Charles G. Smith. Research engineer, American Radio and Research Corporation, 1920–1921. Physicist, Raytheon Manufacturing Company, 1922–1966. Ikor, Inc., 1966–1967.

[8] Henry Sturgis Morgan. Began with J. P. Morgan & Co., Inc., 1923; partner, 1928–1935; treasurer, Morgan Stanley & Co., Inc., 1935–1941; partner, Morgan Stanley & Co., Inc.; director, Connecticut General Life Insurance Company, General Electric Co., Aetna Insurance Co. President, Pierpont Morgan Library; trustee, Morgan Memorial Park, Carnegie Institution at Washington, Metropolitan Museum of Art. Served on active duty as commander, U.S. Naval Reserve, 1941–1945.

Junius Spencer Morgan, 1892–1960. Director, J. P. Morgan & Co., Inc.; director, Continental Can Co., Inc., Morgan & Cie., Inc.; President and trustee, Pierpont Morgan Library.

[9] Charles Stark Draper. Associated with M.I.T. since 1922, where he has served as assistant professor, associate professor, and professor in the Department of Aeronautics and Astronautics. A pioneer in the development of inertial guidance systems, he is now institute professor emeritus and vice-director of the Charles Stark Draper Laboratory, formerly the Instrumentation Laboratory, which he established in 1939 at M.I.T. and directed for thirty years. He is best known for his work in developing guidance and control systems for the Thor and Titan ICBMs, the Polaris submarine, and the Apollo spacecraft.

[10] Lee De Forest, 1873–1961. Inventor, pioneer in wireless telegraphy, telephony. Sometimes called "the father of radio." Patented in U.S. and foreign countries 300 inventions in wireless telegraphy, radio, wire telephone, sound-on-film, high-speed facsimile and picture transmission and television.

[11] Amos Emerson Dolbear, 1837–1910. From 1874 professor of physics, Tufts College. Inventor of various telephonic and telegraphic devices. Received gold medal, London, 1882, for static telephone.

[12] Richard Hewitt. Now research assistant to the president, Carnegie Institution of Washington.

[13] Abram Fedorovich Ioffe (Joffe), 1880–1960. Doctor of Physics; full member, U.S.S.R. Academy of Sciences; full member, All-Union Academy of Agricultural Sciences (Lenin); professor; director, Leningrad Physico-Agronomy Institute; member Communist Party, from 1942. Principal work on quantum theory of light, mechanical properties of crystals, and electric properties of dielectrics and semi-conductors.

[14] Aleksandr Kerenski, 1881–1970. Russian revolutionary leader. After first revolution, February 1917, made Minister of Justice in provisional government; Minister of War, May to July 1917; succeeded Prince Lvov as Prime Minister; overthrown by Bolshevik Revolution, November 1917.

[15] Leon Trotsky. Real name Leib Davydovich Bronstein Trotsky, 1877–1940. Russian Communist leader. Following revolution of November 1917, became people's commissar for foreign affairs in Soviet Government; negotiated with Germans at Brest-Litovsk. Closely associated with Lenin during the latter's lifetime. After Lenin's death in 1924, defeated by Stalin for control of the party; expelled from party, November 1927; banished from Russia, 1929; sought refuge in Mexico in 1937 where he lived until murdered, August 21, 1940.

[16] Reinhold Rudenberg, 1883–1961. Electrical engineer, Siemens Schuchert Works, Germany, 1908–1923; chief electrical engineer, 1923–1936. Consulting engineer, General Electric Company, England, 1936–1938. Professor, electrical engineering, Harvard, 1939–1952; emeritus professor, 1952–1961.

[17] Hermann Minkowski, 1864–1909. Russian mathematician who taught at Königsberg, Zurich, and Göttingen. Credited with laying mathematical foundation for theory of relativity.

[18] Douglas Rayner Hartree, 1897–1958. British theoretical physicist; professor, University of Manchester, from 1929; known for work in solid-state physics, one-electron approximation (Hartree-Foch method).

[19] Blaise Pascal, 1623–1662. French scientist and philosopher; mathematical genius.

[20] Charles Babbage, 1792–1871. English mathematician. Devoted much of his life and fortune to development of a calculating machine for which he was unable to obtain acceptance. Author of *On Economy of Machines and Manufactures* (1832) and *Ninth Bridgewater Treatise* (1837).

[21] Harold E. Edgerton. Electrical engineer and inventor noted for the modern stroboscope which made stop-action and ultra-high-speed multiple-action photography possible. Graduate of University of Nebraska. With M.I.T. as student and teacher since 1926. Institute professor since 1966. Involved in underwater exploration since 1953, inventing special cameras and sound devices for ultra-sonar research.

[22] Herman Hollerith, 1860–1929. Inventor. Instructor in mechanical engineering, M.I.T., 1882. With U.S. Patent Office, 1884–1890. Invented tabulating machine which worked on principle of punched holes in non-conducting material. His machine used for U.S. Census of 1890.

[23] Robert A. Lovett. Secretary of Defense, 1951–1953. Special assistant to the Secretary of War, 1940–1941; Assistant Secretary of War for Air, 1941–1945; Undersecretary of State, 1947–1949; Deputy Secretary of Defense, 1950–1951. Since 1953, general partner, Brown Brothers Harriman & Co.

[24] Edwin H. Land. Inventor of Polaroid camera and other optical devices. President and director of research, Polaroid Corporation.

[25] Elihu Root, Sr., 1845–1937. American lawyer and statesman. Secretary of War, 1899–1904; Secretary of State, 1905–1909; United States senator from New York, 1909–1915. Awarded Nobel prize for peace, 1912. Trustee, Carnegie Institution of Washington, 1902–1937.

[26] Oliver Wendell Holmes, 1841–1935. Associate Justice, U.S. Supreme Court, 1902–1932.

Louis Dembitz Brandeis, 1856–1941. Associate Justice, U.S. Supreme Court, 1916–1939.

Benjamin Nathan Cardozo, 1870–1938. Associate Justice, U.S. Supreme Court, 1932–1938.

Chapter VI

[1] John Landes Barnes. Following undergraduate studies at M.I.T., he took his Ph.D. in mathematics at Princeton in 1934. Taught at Princeton, M.I.T., and Tufts, where he later became professor and chairman of the department of applied mathematics. During World War II and since associated with a number of industrial companies in electronics and systems engineering.

Murray Gardner. With Department of Electrical Engineering, M.I.T., since 1920; became full professor in 1944. Associated initially with Vannevar Bush, he later succeeded him as head of Electrical Engineering's Research Division. His course for graduate students, "Transients in Linear Systems," was discontinued upon his retirement because it was based upon his own knowledge and upon his book published in 1942 (with J. L. Barnes). Internationally known in the field of operational circuit analysis.

[2] Elihu Root, Jr., 1881–1967. Lawyer, associated with Root, Clark and Bird, 1909–1953; later counsel to Cleary, Gottlieb, Steen and Hamilton. Served as major in the Infantry in World War I and as a member of the U.S. Army Air Corps, Strategic Target Board, in World War II. Member of the United States and Panama General Claims Commission, 1932–1933. Trustee, Carnegie Institution of Washington, 1937–1967.

[3] George Wilhelm Merck, 1894–1957. Associated throughout his life with Merck & Co., Inc., manufacturing chemists. President and Director, 1925–1950; Chairman of the Board from 1949. Member, National Science Board; member of the Corporation of M.I.T.; member of the Munitions Board, 1939–1951; member of the Committee on Drugs and Medical Supplies, National Research Council, 1942–1945; director, War Research Service (in charge of biological warfare), 1942–1944. Special consultant to Secretary of War for biological warfare, chairman of U.S. Biological Warfare Committee, 1944–1945.

[4] Paul Vories McNutt, 1891–1955. Governor of Indiana, 1933–1937. U.S. High Commissioner to the Philippines, 1937–1939 and 1945–1946. Administrator of Federal Security Agency, 1939–1945; Director of Defense Health and Welfare Services, 1941–1943; chairman of War Manpower Commission, 1942–1945.

[5] Henry White Gadsden. President and chief executive officer of Merck & Co., Inc. from 1965– . Associated earlier with Sharpe & Dohme, Inc., as vice-president and later as director.

[6] Max Tishler. President, Merck, Sharpe & Dohme Research Laboratories, 1957– . Board director, Merck & Co., Inc., 1962.

[7] Freelan O. and Francis Edgar Stanley, identical twins, b. 1849. Inventors of Stanley dry plate which revolutionized photography, also of Stanley steam automobile, 1897. Their car made the fastest mile record (in that time) of 28⅕ seconds at Ormond Beach in 1906 and two miles in 59¾ seconds. Francis Edgar was killed in an automobile accident in 1918 and Freelan O. died in 1940. Francis Edgar produced first successful steam automobile, the Stanley Steamer, in 1897; began quantity production of car in 1898, but sold patent rights and business to J. B. Walker; repurchased patents (with his brother), organized Stanley Motor Car-

riage Co. (which produced famous Stanley Steamer), 1902; president, 1902–1912. Organized (with his brother Freelan) Stanley Dry Plate Co., 1883; sold firm to Eastman Kodak Company, 1905.

[8] Henry J. Kaiser, 1882–1967. Industrialist. Organizer and president, Columbia Construction Company, Consolidated Builders, Inc.; participant in seventy other major construction projects, 1941–1945. Chairman of the Board, Kaiser Industries Corporation. Managed seven shipyards on Pacific Coast during World War II.

[9] Gaston Planté, 1834–1889. French physicist, invented first practical storage battery (1860).

[10] Henri G. André, Montmorency, France. Electrical engineer and inventor. Member, Societé Française des Electriciens.

[11] John Ericsson, 1803–1889. Swedish engineer and inventor. While living in England, established reputation as an engineer through his introduction of the screw propeller on a commercial vessel. Later came to the United States, where his name is associated with the ironclad Civil War vessel, *The Monitor.*

[12] Robert Stirling, 1790–1878. Scottish clergyman and engineer. Stirling's cycle. Thermodynamics. A cycle for an air engine using a regenerator, having for its indicator diagram 2 isothermals and 2 lines of constant volume. Hence: Stirling engine.

[13] Samuel C. Collins. Associated with the mechanical engineering department of M.I.T. since 1930. Professor, 1949–1964, emeritus professor, 1964–   . Noted for research in cryogenics.

[14] Air Commodore Sir Frank Whittle. After service as pilot officer, Royal Air Force, and as test pilot attended Officers' Course on Engineering at Henlow, 1932–1934; Cambridge University, 1934–1937; posted to Special Duty List to continue work on Whittle jet-propulsion gas turbine, 1937–1946; technical adviser on Engine Design and Production to the Controller of Supplies (Air), Ministry of Supply, 1946–1948; retired RAF, 1948. Technical adviser to BOAC, 1948–1952. Consultant to Power Jets (Research and Development) Ltd., 1950–1953. Among other honors, first recipient of the Goddard Award of American Institute of Aeronautics and Astronautics, 1965. Author, *The Early History of Whittle Jet-Propulsion Gas Turbine* and *Jet: The Story of a Pioneer.*

[15] Charles Franklin Kettering, 1876–1958. Engineer and inventor. Organized Dayton Engineering Laboratories Company for manufacturing own inventions. Organizer of the Dayton Metal Products Company and Dayton-Wright Airplane Company, 1914. Served for twenty-seven years as vice-president and general manager of Research Laboratories, General Motors Corporation.

[16] Alexander Graham Bell, 1847–1922. Scottish educator and inventor. Came to the United States to teach systems of speech for the deaf. Naturalized, 1882. Obtained patents on the development of the telephone (1876–1877).

Chapter VII

[1] Eugene Joseph McCarthy. U.S. senator from Wisconsin, noted for his unexpected entry into the Democratic primary in New Hampshire in 1968 when almost immediately he attracted a wide following of young people throughout the United States. Defeated the late Senator Robert F. Kennedy in several primaries before being defeated by him in the California primary just prior to the latter's assassination in June 1968.

[2] Frederick G. Fassett, Jr. Long associated with M.I.T. as teacher and later as teacher and administrator. Sometime editor, *Technology Review*, and director, *Technology Press*. Director, Office of Publications, Carnegie Institution of Washington, 1945–1951.

[3] Francis Bitter, 1902–1967. Magnetician. Associate professor of physics, M.I.T., 1934–1951; professor of physics, 1951–1960. Associate Dean of Science, 1956–1960; professor of geophysics from 1960. Author, *Introduction to Ferromagnetism; Currents, Fields and Particles; Magnets; The Education of a Physicist*, and other works.

[4] Dugald C. Jackson, 1865–1951. Electrical engineer. Senior member firm D. C. and William B. Jackson, 1902–1918; Jackson and Moreland, 1919–1930. Professor, electrical engineering, University of Wisconsin, 1891–1907. Professor of electrical engineering and head of department, M.I.T., 1907–1935; professor emeritus, 1935–1951.

[5] Arthur Edwin Kennelly, 1861–1939. Electrical engineer. Principal electrical assistant to Thomas A. Edison, 1887–1894. Associated with Edwin J. Houston in firm Houston and Kennelly, consulting electrical engineers, Philadelphia, 1894–1901. Professor, electrical engineering, Harvard, 1902–1930. Professor, electrical engineering, M.I.T., 1913–1924; director, electrical engineering research and chairman of faculty, M.I.T., 1917–1918.

[6] Christopher Columbus Langdell, 1826–1906. Lawyer, educator. Graduated from Harvard Law School, 1853; practiced in New York until 1870. In 1870 became Dane Professor of Law and Dean, Law Faculty, Harvard. Author, *Selection of Cases on the Law of Contracts; Cases on Sales; Summary of Law of Contracts and Equity Pleading*, and others.

Chapter VIII

[1] Charles Michelson, 1896–1947. Director of publicity, Democratic National Committee, 1929–1940; director of public relations, NRA, 1933–1934.

[2] Dean Gooderham Acheson. Secretary of State, 1949–1953; long associated with the Washington law firm of Covington and Burling.

[3] Edward Grey, first Viscount Fallodon, K.G., F.R.S., 1862–1933. Diplomat, Secretary of State for Foreign Affairs, 1905–1916. Temporary ambassador to U.S.A., 1919. Author of *Fly-Fishing*, 1899; *Twenty-five Years*, 1892–1916; *Fallodon Papers*, 1926; *Charm of Birds*, 1927.

[4] Sir John Buchan, First Baron Tweedsmuir, 1875–1940. Scottish author; Governor General of Canada, 1935–1940; author of novels of adventure, including *John Burnet of Barns*, *Greenmantle*, *John Macnab*. Also author of biographical and autobiographical works.

[5] William Cameron Forbes, 1870–1959. Grandson of Ralph Waldo Emerson. Member, Philippine Commission, 1903–1916; Vice-Governor General of the Philippines, 1908; Governor General, 1909–1913; named by President Warren G. Harding in 1921 to serve with General Leonard Wood as special investigator of the Philippines; chairman of Commission to Haiti, 1930; ambassador to Japan, 1930–1932. Member Boston banking firm J. M. Forbes & Co.

[6] John Phillips Marquand, 1893–1960. Novelist of Boston society, noted especially for *The Late George Apley*, published in 1937, for which he was awarded the Pulitzer Prize.

[7] Cleveland Amory. Writer about high society in the United States, including *The Proper Bostonians*, *The Last Resorts*, *Who Killed Society?* Editor-in-chief, *Celebrity Register*, 1963.

[8] Walter Sherman Gifford, 1885–1966. Chairman of the Board, AT&T, 1948–1950; honorary chairman, 1950– . U.S. Ambassador to Great Britain, 1950–1953. Trustee, Carnegie Institution of Washington, 1931–1966.

[9] James R. Killian, Jr. Educator, administrator. Chairman of the Corporation, Massachusetts Institute of Technology, since 1959, having previously served for nearly ten years as president of the Institute, with which he had been associated since he became a student there in 1923. His participation in service to the government has been extensive, involving many assignments in varied fields, a primary one being his appointment in November 1957 as special assistant for science and technology to President

Eisenhower, the first to fill the newly created post, which he held, on leave from the Institute, until July 1959.

[10] Jerome B. Wiesner. Electrical engineer. Leader in development of microwave theory and communication sciences, Dr. Wiesner was associated with the Radiation Laboratory at M.I.T. during war years, and served also for a year with the Los Alamos Laboratory. He has been Dean of the School of Science at M.I.T., and is now Institute professor and provost. A member of the President's science advisory committee since its inception, Dr. Wiesner was named Science Adviser by President Kennedy on January 20, 1961, and served in that capacity for three years.

[11] Donald F. Hornig. Chemist. Served as a group leader at the Los Alamos Laboratory of the Manhattan District in 1943–1945 after association with the Woods Hole Oceanographic Institution. After serving as professor and administrator at Brown University, joined Princeton University in 1957 and in 1959 became Donner Professor of Science. He became a member of the President's Science Advisory Committee in 1960 and in 1964, as science adviser to President Johnson, its chairman, in which capacities he served until 1968. Vice-president, Eastman Kodak, 1968 until election in 1970 as president, Brown University.

[12] William Averell Harriman. Railroad executive and public official. U.S. Ambassador to Russia, 1943–1946; to Great Britain, April–October 1946; Secretary of Commerce, 1946–1948; U.S. representative in Europe under Economic Cooperation Act of 1948; rank of Ambassador Extraordinary and Plenipotentiary, 1948–1950; special assistant to the President, 1950–1951. Governor of New York, 1955–1958. Ambassador-at-large, 1961, 1965– . Assistant Secretary of State for Far Eastern Affairs, 1961–1963; Undersecretary of State for Political Affairs, 1963–1965. Chief, U.S. Delegation, Vietnam Peace Talks in Paris, 1968–1969.

[13] Admiral Harold Raynsford Stark. Chief of Naval Operations, 1939–1942; Commander, U.S. Naval Forces in Europe, 1942–1945. Retired 1946.

[14] Klaus Emil Fuchs. Notorious German spy, the son of a well-known German pastor, who came to England as a refugee in 1933 and became a British citizen. Sponsored by the Society for the Protection of Science and Learning, he obtained his Ph.D. from Bristol University in mathematics and physics and a Doctor of Science degree from the University of Edinburgh. He was interned in Canada in 1940–1942. Later, under Professor Peierls of the University of Birmingham, he conducted atomic research, first in England and later at Los Alamos. He served three years as head of the theoretical division of the British Atomic Energy Commission project at Harwell. He was arrested on February 2, 1950, for espionage, convicted and given a fourteen-year sentence on March 1, 1950. Released from Wakefield Prison in 1959, he returned to Leipzig in East Germany, having been stripped of his British citizenship. He was

made deputy director of the East German Central Institute for Nuclear Research. (See Rebecca West, "The Terrifying Impact of the Fuchs Case," in *The New York Times Magazine*, March 4, 18, 25, 1951.)

[15] Rt. Honorable Sir (Richard) Stafford Cripps, 1889–1952. British Ambassador to Russia, 1940–1942; Lord Privy Seal and Leader of House of Commons, 1942; Minister of Aircraft Production, 1942–1945; president, Board of Trade, 1945; Minister for Economic Affairs, 1945. Rector of Aberdeen University, 1942–1945. Chancellor of the Exchequer, 1947–1950.

[16] Sir John Anderson, first Viscount Waverley, 1882–1958. Diplomat, government officer, and civic leader. Governor of Bengal, 1932–1937; Lord Privy Seal, 1938–1939; Home Secretary and Minister of Home Security, 1939–1940; Lord President of the Council, 1940–1943; Chancellor of the Exchequer, 1943–1945. Chief representative of Churchill government on atomic energy matters.

[17] Major General Clarence C. Williams, 1869–1958. Graduated from the U.S. Military Academy in 1894 and advanced through the ranks to major general, Chief of Ordnance, July 16, 1918. He was called back to active duty in 1942 after twelve years of retirement. While serving in the Office of the Assistant Chief of Staff for Materiel (Army Service Forces), he was named War Department representative on the National Defense Research Committee.

[18] General Lucius D. Clay. In charge of defense airport program, Civil Aeronautics Administration, 1940–1941; Assistant Chief of Staff for Materiel Service of Supply; appointed deputy director for war programs, December 1944; deputy to General Eisenhower in 1945; Deputy Military Governor of Germany (U.S.), 1946; Commander in Chief, U.S. Forces in Europe, and Military Governor, U.S. Zone, Germany, 1947–1949. Retired from military service in 1949.

[19] Admiral William D. Leahy, 1875–1959. Chief of Naval Operations, 1937–1939; retired August 1, 1939. Appointed Governor of Puerto Rico, 1939; ambassador to France, 1940–1942; recalled to active duty as Chief of Staff to the Commander in Chief of the United States Army and Navy, 1942–1949; appointed Fleet Admiral, December 15, 1944.

[20] Clement Richard Attlee, 1st Earl; Viscount Prestwood, 1955. Leader of the Opposition, 1934–1940; Lord Privy Seal, 1940–1942; Secretary of State for Dominion Affairs, 1942–1943; Lord President of the Council, 1943–1945; Deputy Prime Minister, 1942–1945; Prime Minister and First Lord of the Treasury, 1945–1951.

[21] William Lyon MacKenzie King, 1874–1950. Prime Minister of Canada, 1921–1926, 1926–1930, 1935–1948.

[22] James Francis Byrnes. Elected U.S. senator, two terms, 1931–1941; appointed Justice, U.S. Supreme Court, June 1941; resigned from U.S. Supreme Court, October 3, 1942, to accept appointment as director, economic stabilization, resigned; appointed director of war mobilization, May 27, 1943, resigned, 1945; Secretary of State, July 1945–1947, resigned January 1947. Governor of South Carolina, 1951–1955.

[23] Benjamin Victor Cohen. General counsel, Office of War Mobilization, 1943–1945; counsellor of Department of State, 1945–1947; legal adviser, International Monetary Conference, Bretton Woods, N.H., 1944; member American Delegation, Dumbarton Oaks Conference, 1944; Berlin Conference, 1945; Council of Foreign Ministers, London, 1945; Moscow, 1945, 1947; Paris, 1946; New York, 1946; Paris Peace Conference, 1946; senior adviser, American Delegation, U.N. General Assembly, London, New York, 1946; member Delegation, Paris, 1948, 1951, New York City, 1949, 1950, 1952; U.S. representative on U.S. Disarmament Commission, 1952.

[24] Lester Bowles Pearson. Minister Counsellor, Canadian Legation, Washington, 1942–1944; M.P., 1944; ambassador to United States, 1945–1946; Undersecretary of State for External Affairs, 1946–1948; Secretary, 1948–1957; Prime Minister of Canada, 1963–1968.

[25] Tom Connally, 1877–1963. Member 65th–70th Congresses (1917–1929), 11th Texas District; U.S. senator, 1929–1953; elected 1946 for term ending 1953; chairman, Senate Foreign Relations Committee, 1941–1946, 1949–1953.

[26] Bernard Baruch, 1870–1965. Noted New York financier and adviser to top-level government officials and groups. During World War II adviser to James F. Byrnes, war mobilization director, 1943–1945; headed fact-finding commission on synthetic rubber, 1942; reported to President and James F. Byrnes on war and postwar plans, February 1944. U.S. representative, U.N. Atomic Energy Commission, 1946.

[27] General Carl Spaatz. Chief of the Air Staff, Army Air Force Headquarters, 1941; Commander 8th Air Force and Commanding General U.S. Army Air Forces in European Theatre, 1942; Northwest African Air Force, 1943; Commanding General, Strategic Bombing Force operating against Germany, 1944. Commanding General U.S. Strategic Air Forces in the Pacific, 1945, and supervised final strategic bombing of Japan. Commanding General, U.S. Army Air Forces, 1946–1947; Chief of Staff, U.S. Air Force, 1947–1948; retired 1948.

[28] Marshal of the Royal Air Force Lord Arthur William Tedder (Baron Tedder of Glenguin), 1890–1967. Deputy Air Officer Commanding in Chief, R.A.F., Middle East, 1940–1941; Air Officer Commanding in Chief, R.A.F., Middle East, 1941–1943; Air Commander in Chief, Mediterranean Air Command, 1943; Deputy Supreme Commander under General Eisen-

hower, 1943–1945; Chief of the Air Staff and First and Senior Air Member, Air Council, 1946–1950; chairman of British Joint Services Mission, Washington and U.K. Representative on the Standing Group of the Military Committee of the North Atlantic Treaty Organization, 1950–1951. Chancellor of the University of Cambridge from 1960.

[29] Marshal of the Royal Air Force Sir John Cotesworth Slessor. Air Officer Commanding 5 (Bomber) Group, 1941; Assistant Chief of the Air Staff (Policy), Casablanca Conference, 1942–1943; Air Officer Commanding in Chief, Coastal Command, 1943; Commander in Chief, R.A.F., Mediterranean and Middle East, 1944–1945; member of Air Council for Personnel, 1945–1947; Commandant, Imperial Defence College, 1948–1949; Principal Air A.D.C. to the King, 1948–1950. Chief of the Air Staff, 1950–1952.

[30] Professor Nevil Vincent Sidgwick, C.E.B., F.R.S., 1873–1952. Fellow and Tutor of Lincoln College, Oxford. Noted for chemical researches including electronic theory of valency.

[31] Louis Arthur Johnson, 1891–1966. Assistant Secretary of War, 1937–1940. Secretary of Defense, 1949–1950. Personal representative of the President in India, 1942.

[32] Clinton P. Anderson. Member of 77th–79th Congresses from New Mexico at large; Secretary of U.S. Department of Agriculture, 1945–1948; U.S. senator from New Mexico, 1948–

[33] John Steelman. Commissioner of conciliation, U.S. Conciliation Service, 1934–1936; director, 1937–1944. Public relations consultant, New York City, 1944–1945. Special assistant to the President, 1945–1946. Director, Office of War Mobilization and Reconversion, June–December 1946. Assistant to the President, 1946–1952. Appointed acting director, Defense Mobilization, acting chairman, National Security Resources Board, 1948–1950.

[34] Lewis Strauss. Associated with Kuhn, Loeb & Co., New York, from 1919 to November 1946, partner, 1929–1947. Member, U.S. Atomic Energy Commission, 1946–1950. Special assistant to the President on atomic energy matters, 1953; chairman, A.E.C., 1953–1958. Secretary of Commerce, 1958–1959.

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