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During the past quarter-century science and technology have brought mankind many benefits -- jet airliners, nuclear power stations, antibiotics, communications and weather satellites, transistors, television, and computers; and many burdens -- jet bombers, nuclear weapons, biological weapons, military satellites, and intercontinental ballistic missiles. Together these products of advancing technology have reshaped our world: bringing it closer together; altering the disposition of power and influence; promising health and comfort to many; threatening all with catastrophe. Technology has made the world taut -- so that striking a tiny state shakes the entire globe. And it has made it fragile -- so that a small spark can grow to consume us all. But science and technology have also made us neighbors -- events in Paris are no farther from my television screen than those in Chicago. And they have given us hope -- hope for the alleviation of disease and ignorance and poverty. Thus, we live with the recognition that the evolution of science and the progress of technology contain both the promise of beneficent peace and the threat of devastating war.

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It is natural then, that we have come to aspire to anticipate and to control the consequences of science and technology. Perhaps if we look ahead we will see dangers and avoid them, or sense opportunities and exploit them. The current advances in biology and computer science, for example, hold the potential for both good and ill. Where, we might ask, will our new knowledge of the DNA-RNA mechanisms in the gene lead us? To the promise of lives freed from the pain of genetic defects? Or to the threat of some new and more terrible biological weapon? What can our increasing capacity to process information lead to? Freer access to knowledge for all? Or tighter control by the few of the many. Can we by projecting what science and technology might give us, steer our way to a world that acquires the benefits, but rejects the burdens?

There are some who say "No."

They argue that by looking ahead and spelling out potential problems, we may actually make them more likely. We may publicize forms of evil that might otherwise have escaped notice. Our prophecy of danger may inspire its own fulfillment.

Moreover, they say, our perceptions of the benefits or burdens of a future development might be faulty. Many of the implements of peace were born as implements of war. Might we not cut off some threatening line of research before it could demonstrate its benefits to mankind?

Finally, they assert, even if our perception is true and we are confident of the goal, we may not be able to steer our way to it. Man's attempts to prevent war by limiting arms, for example, have a sad history. Though undertaken in the best of faith, they may have caused more conflict than they prevented. Would our attempts to control technology fare any better?

But while we must remain aware of these limitations and dangers of attempting to anticipate the social consequences of scientific evolution and technical progress, I hope you will join me in saying "Yes, the attempt must be made."

For while our prophecies might inspire some otherwise unthought-of evil, I think it more probable that they will show the way to unanticipated benefits. Man has displayed no absence of imagination when it comes to creating new destructive devices; but he has not always had the wit to grasp fully how science and technology can be used constructively.

Moreover, by attempting to anticipate evil consequences of scientific and technical development, we <u>can</u> avoid many of them. Our experience contains more occasions in which technology has given us unpleasant surprises that we could have avoided if we had thought ahead, than instances in which we would have seen danger in a development that proved beneficial. The danger of misperception, though real, is far less than the danger of nonperception. Our air, our water, our resources, our freedom and privacy all are threatened by witless exploitation of technology. Foresight can protect them.

Finally, though our competence at social cybernetics is low, it is not completely absent. We <u>have</u> developed mechanisms that guide technology in beneficial directions. We shall develop more. The dangers of unanticipated and undirected technology are so great, that we must spend the effort to gain control.

In the end it will not be these arguments that lead us to say "yes" or "no"; it will be the degree of our faith in reason and rationality.

Should these control man's affairs? Can they? For me, and I hope also, for you, the answers are "yes."

But how is society to anticipate the consequences of advancing science and technology? Where should the responsibility lie? How can the competence be developed?

Of course we must begin by recognizing that some effort is already made. Many industrial organizations, some government bureaus, a few university faculties, and several private organizations try in one way or another to comprehend future scientific and technical developments. But these efforts are often limited in scope, in perspective, in continuity, and in influence. The job that needs to be done is much bigger. Each nation needs an institution whose attention is concentrated on the needs of society and the prospects of technology. Each nation needs a "lookout" institution, which combines a broad perception of society's objectives with a deep knowledge of science's capabilities.

Where should such an institution be Not in any existing institutional framework.

Not in government, because it must be free to think beyond the immediate and beyond the politically acceptable.

Not in the university, because its udies must draw on combi ions of skills, seek types of relevance, and exist under distribution limitations that are incompatible with the goals of most universities.

Not in industry, because its knowledge and influence must depend on privileged relations with government, industry, and the universities.

The answer, it seems to me, is for society to employ a new class of institution -- the independent research institution, which will have close relations with government, industry, and the universities, but be independent of them; and whose responsibility will be to society, perhaps through a board of trustees selected to represent the public interest.

Therefore, I make the following

PROPOSAL: Every nation should have at least one independent research institution engaged in anticipation of the social consequences of scientific evolution and technical progress.

And in support of this proposal I should like to draw on the experience of one such institution that has been in existence for twenty years -- The Rand Corporation. By examining its experience we shall be able to form a clearer picture of what such an institution can be expected to achieve. But, of greatest importance, we shall be able to identify some characteristics that seem prerequisite to its success.

Let us start with Rand's charter. The Rand Corporation is a non-profit corporation formed "to further and promote scientific, educational, and charitable purposes, all for the public welfare and security of the United States of America."

Its history will tell us more. Rand was born in the aftermath of the Second World War. During that conflict the immediate relevance of scientific evolution and technical progress to a nation's security was strikingly demonstrated through the development and application of radar, sonar, jet propulsion, guided missiles, and the atomic bomb. Scientists had also learned how to apply their processes of systematic analysis to improve the ways that the new weapons were employed, developing what is now called "operations research." But the efforts of scientists and engineers had been mobilized primarily through a series of emergency organizations that were disbanded when victory had been won. A number of foresighted individuals, however, recognized that scientific evolution and technical progress would continue to have significant impact on the nature and technology of defense, and that some means of keeping a group of scientists and engineers interested in such problems would have to be found.

Foremost among those concerned was General H. H. Arnold, then Commanding General of the Army Air Forces. While he was determined to maintain the close and useful relations between the Air Force and scientists that had developed during the war, he recognized that the relationship would be most fruitful if it were not too close; scientists should have the independence to identify and investigate problems as they saw them, and not be bound by the problems as perceived by decisionmakers. This independence turned out to be crucial to Rand's success, as we shall see in a moment. Most of you will also recognize how unusual this simple freedom is, especially when granted by a military organization. To make the situation even more unusual, the scientists were to be given a broad charter, one that enabled them to examine most of the critical problems of assuring the nation's security. They were, moreover, enjoined to think about the longer-range problems and to avoid becoming enmeshed in the day-to-day problems of the Pentagon.

The difficulties of establishing an organization with such a charter either within government or a university were understood. So late in 1945, General Arnold arranged for a contract between the Army

Air Forces and the Douglas Aircraft Company to set up "Project RAND" (for "research and development"). He ordered that $10 million be allocated to the project to give it the base of support it would need to prove itself.

From 1946 through 1948 Project RAND was a virtually autonomous department of Douglas. But as it grew, so did the need to leave the industrial environment. Its style of work was more that of a university than an aircraft manufacturer; its special relationship with the Air Force was inappropriate for a single contractor or sponsor; and the validity of its basic idea had been established. So in November 1948 The Rand Corporation came into being. [This month we are celebrating our twentieth anniversary.] The Project RAND contract was moved to The Rand Corporation, which having no stock and no stockholders, could gain the trust and cooperation of industry, while maintaining its close association with government and the universities. In the subsequent twenty years, The Rand Corporation has gained sponsorship from other agencies of government; but it has not received from any of them the broad charter and freedom granted by the Air Force.

How true was General Arnold's vision? The idea has been put to solid test. According to Herman Kahn, a Rand alumnus who now heads The Hudson Institute, the period since Rand's inception has seen four revolutions in the technology of intercontinental warfare. They are described in Fig. 1.

In 1946, the atomic bomb had already created a fundamental change in the nature of conflict. But both bombers and fighters still depended on the piston engine, submarines were diesel-powered, and the skies were searched by individual, poorly coordinated radars.

REVOLUTIONS IN INTERCONTINENTAL MILITARY TECHNOLOGY

	1946	1951	1956	1961	1966
WEAPONS	Fission - 1	Fission - 4	Fusion	Fusion	Fusion
BOMBERS	Piston - 1	Piston - 2	Jet	Jet	Jet
FIGHTERS	Piston	Jet - 1	Jet - 2	Jet - 2	Jet - 2
MISSILES	———	———	———	Liquid	Solid
SUBMARINES	Diesel	Diesel	Nuclear	Nuclear-Missile	Nuclear-Missile
DETECTION	Radar	Radar	Radar	BMEWS	Satellites
CONTROL	———	Manual	Computer	Computer	Computer

Fig. 1

By 1951, fission bomb technology had entered its third- or fourth-generation. A second generation of piston-engined bombers with longer range had been acquired, and the first generation of jet fighters had appeared. A manual control system tied the radars together. It was then that the first true capability for intercontinental atomic warfare was in being.

By 1956, the potential destructiveness of such warfare took a vast jump. The era of thermonuclear weapons carried by huge jet-propelled bombers had begun. A second generation of jet defense fighters was being introduced and so was a computer-assisted control system. Nuclear submarines appeared in the oceans.

By 1961, intercontinental warfare began to shift from a technology of aircraft to one of missiles. The first generation of liquid-fueled, but vulnerably-based, ICBMs appeared. The far less vulnerable combination of nuclear submarine and solid-fueled missile was on the horizon. And the first steps toward anti-missile defense, the BMEWS early-warning radars, were taken. Intercontinental warfare now assumed an entirely different pace and character.

By 1966, missile technology had matured. Solid-fueled, heavily protected missiles entered the force in large numbers. The submarine-based force expanded. Nuclear warheads had become highly efficient and relatively cheap. And now satellites with apparent military purposes began to appear. Soon each missile-armed nation would have the dreadful ability to destroy the other, even after absorbing a direct attack. In such a balance of terror, many see a stable peace.

I recount these developments, not to impress you with man's over-developed competence in creating destructive technologies, but to establish that since its inception, Rand has had to concern itself constantly with the implications of revolutionary technological changes.

Rand's scientists have attempted to anticipate the consequences of each change -- and the consequences have been many. Each revolution has altered not only the instruments of warfare, but the nature and prospects of warfare itself.

Never before have military doctrines had to undergo such continuous and severe change in the shadow of peace, instead of the glare of war.

Never before have the consequences of ignorance or carelessness been so dangerous to a nation's security or to mankind's preservation.

Never before has the requirement to push technology to its utmost, lest an opponent reap its bounty first, been so severe.

Under this pressure, Rand's scientists have established, as I shall try to show, the value of the independent research institution concerned with societal implications of scientific evolution and technical progress. They have demonstrated the validity of General Arnold's vision.

In the next twenty years, with social and technological change in non-military pursuits gaining the speed and impact that have characterized the military ones, Rand and organizations like it will have an increasingly important role to play in the civilian sectors of society.

So that you may gain a better understanding of how such an organization operates, I shall describe two of Rand's early studies and try to extract the lessons from them that have guided Rand's research ever since. These lessons should, I believe, be heeded in any attempt to establish independent research institutions of the kind I am proposing.

The first major task suggested to the new Project RAND in the spring of 1946 was to study the feasibility and usefulness of an artificial earth satellite. This was at a time when the major interest in such objects was held by science-fiction writers. A preliminary design study was delivered in May 1946, which showed that a primitive satellite could be launched by 1952. In the letter of transmittal, Rand wrote that it considered

> the construction of a satellite to be technically feasible, the problems associated with instrumentation and guidance being more difficult of solution than those of building the vehicle itself. The scientific data which a satellite can secure and transmit to earth are extremely valuable and the vehicle has important military uses in connection with mapping and reconnaissance, as a communications relay station, and in association with long-range missiles.

No unusual methodologies were used in making this quite accurate projection. It was simply the result of asking highly-skilled technologists to estimate the state of the art. What is somewhat more surprising is the report's additional observation in 1946 that:

> Since mastery of the elements is a reliable index of material progress, the nation which first makes significant achievements in space travel will be acknowledged as the world leader in both military and scientific techniques. To visualize the impact on the world, one can imagine the consternation and admiration that would be felt here if the U.S. were

to discover suddenly that some other nation had
already put up a successful satellite.

What can we learn from this study?

Well, first, that while the state of the art can often be estimated with fair accuracy, the actual rate at which progress will occur is far harder to estimate. Technology is not the only constraint; the nation's interest and resources exercise strong control over technical progress. The satellite that might have been launched in 1952 did not achieve orbit until 1958, when the International Geophysical Year and Sputnik gave it the boost that was needed.

There is another, more important lesson. At about the time that Rand was asked to look at the feasibility of a satellite, another military service, which had no Project RAND, was also supporting a satellite project. But it approached the problem differently. It decided early that it preferred a single-stage hydrogen-fueled vehicle. It then went to a contractor and asked what the ratio of fuel to gross weight for such a vehicle would be. With that result in hand, it decided on a gross weight of 100,000 pounds; which gave a 1000-pound payload and 10,000 pounds for the vehicle. It decided that the powerplant should be roughly half the vehicle weight, and asked another contractor to design a suitable 5000-pound powerplant. Finally, it asked three other contractors to design satisfactory 5000-pound airframes.

The result of this study went before a scientific board together with the result of Rand's study, which had been inspired by a much simpler and broader question -- is a satellite feasible? Rand was able to show that the other service's vehicle would not work, since multiple staging would be needed to achieve orbital speeds with any practical technology. Because of Rand's work, the board accepted the Air Force's proposals.

The lesson, of course, is that freedom to define a problem is important. The other service's approach was for the decisionmaker to specify and divide the problem, and it failed. The Air Force succeeded because it gave Rand a simple work statement, with the latitude to explore those avenues that the technologists thought might be fruitful.

The satellite example holds one further lesson. The Air Force's early interest in satellites died during the struggle to adapt to the more pressing revolutions in aircraft and nuclear weapons technology that were occurring during the fifties. In fact, the budget squeezes of that period led all the services to give up support for satellite research. Had the Air Force specified Rand's research program, Rand's interest might have died as well. It did not. The original findings were the basis for a continuing program which, through the years, yielded hundreds of reports on space technology. In 1951 two Rand scientists proposed the creation of meteorological satellites. During the early fifties Rand several times urged the development of other useful satellites. And a study of the Soviet technical literature led to an educated guess in mid-1957 of the launching date for Sputnik I. It was wrong -- by two weeks.

Then in 1957 after Sputnik was launched, the Air Force reaped the benefits of the ten-year investment it had given Rand the freedom to make. Rand was able to pass its knowledge along quickly to those who now needed to know: to Air Force, Defense Department, and Congressional personnel. And it was able to help the Air Force design an effective program.

Freedom is especially important if one seeks to anticipate the future consequences of scientific and technical progress.

A second study that had strong influences both on Rand's sponsor and on Rand itself was one, begun in 1951, on the selection and use of strategic air bases. In this instance Rand was asked by the Air Force whether it could help to choose the sites for a series of overseas air bases that were to be built during the 1956-61 period. It was hoped that Rand might help to find a basing system that minimized the cost of the facilities to be built.

As posed, this would appear to be a relatively straightforward exercise in logistics planning. But when it came to Rand, the request for assistance was turned over to Albert Wohlstetter, a mathematical logician-turned-economist, who has subsequently gained prominence as a strategic theorist. Rand need not work on projects suggested by the Air Force, but the requests are usually considered carefully. Wohlstetter's considerations led him to believe that some potentially major issues were raised by the request, but that they were not the ones the Air Force had initially identified. He became concerned about a question that prior thinking on air base location had almost completely ignored: the vulnerability of the aircraft and bases to an opponent's first strike. While it may seem surprising seventeen years later, in 1951 the implications of atomic bombs, long-range bombers, and international antagonisms had not all been recognized. We were considering putting the major part of our strategic forces on a few overseas bases where they could easily be destroyed on the ground by a surprise attack.

Wohlstetter undertook to examine the basing of U.S. strategic forces, accepting overseas basing as only one of four alternatives that were to be compared. Moreover, he determined to compare them not according to the cost of facilities alone, but according to the costs of the entire system -- bases costs and aircraft costs, initial costs and operating costs -- and their relative effectiveness in fulfilling their strategic mission. The study, therefore, was broadened to consider questions of international relations: How would allied nations feel about bases? Of technology: What were the characteristics of future aircraft? And of national strategy: How best can the United States deter an attack on itself?

The results showed that the then-planned system of overseas operating bases was decidedly inferior to a system of bases in the United States, supplemented by spartan overseas refueling bases constructed so as to reduce vulnerabilities. The findings contributed to an Air Force decision to revise its strategic base structure, which, according to an Air Force estimate, saved $1 billion in proposed installation costs, while maintaining the same capability. The study also introduced a new mode of strategic thought, in which the ability to survive an assailant's initial blow with enough force to punish him became the cornerstone of America's deterrence policy. That idea has guided the construction of all new strategic forces since then.

What are the lessons of this study? First, we see again that projections of the implications of future technology must include consideration of many other factors. The basing system under examination was intended for aircraft that were not yet in operation and, in one case, were not even in existence. Their capabilities had to be projected. But so also did the economic, political, and strategic factors that determined the environment in which they were to be used. Rand has rarely engaged in the projection of science and technology in vacuo. Such studies take on true value only when they are part of the broader consideration of the ways of achieving some specific societal goal.

Second, we see how essential is the ability to rephrase the problem stated by the decisionmaker. Had Rand solved the problem as the Air Force first perceived it, the result would probably have cost a billion dollars more and been considerably less effective. It would have produced a less stable peace.

Third, the result of the study would have gone for nought if Rand did not have close and continuous relations with the Air Force. That service's trust in Rand and Rand's ability to gain the ear of high Air Force officers, led them to accept conclusions that in some ways contradicted existing Air Force doctrine. This close association, so vital for influence, is just as critical as the freedom and independence so crucial to success. Balancing the two has been the most important part of Rand's relationship with the Air Force. It will equally be the most difficult and the most essential part of the relationship of Rand-like institutes with their governments.

Finally, this study, like many others at Rand, benefited by its detachment from day-to-day problems. Wohlstetter and his team were able to devote over a year-and-a-half to a thorough thinking through of the problems of strategic basing. Had they had to conduct a similar study within the confines of government, they probably would have had one-third the time and far less eventual influence. It is doubtful that they could have developed their fundamentally different approach in that time.

These two studies give some idea of how Rand approaches the problem of projecting scientific evolution and technical progress.

The satellite study is an example of one major class of studies, in which the starting point is a particular technology and the purpose is to identify its prospective impact on society's objectives. Such studies might be called "technology-oriented."

The basing study is an example of the second major class, in which the starting point is one of society's objectives -- in that case, deterrence of strategic attack -- and the purpose is to identify the implications of advancing science and technology for its achievement. Such studies might be called "policy-oriented."

A third class of study focuses on the methodology of analysis in an attempt to make fundamental improvements in our ability to do technology-oriented or policy-oriented studies. Many such improvements, of course, are made in the midst of technology or policy studies, out of necessity. But since its inception, Rand has found it fruitful to have some individuals whose principal concern is methodology. Many of its mathematicians and computer scientists pursue such concerns.

One such product that may be familiar to many of you is the Delphi method. It grew from the interest of its developer, Olaf Helmer, a mathematician and logician, in the methodology of the inexact sciences: those in which expertise exists, but can not be asserted or established through formal argumentation. Most of the social sciences, especially those relevant to international relations, are inexact in that sense.

Helmer felt that if in such sciences the knowledge of a number of experts could be combined, the result would be better than the judgment of any single expert. He also believed that the traditional ways of combining experts' knowledge through the use of teams, panels, and committees produced distortions, because differences in prestige and interpersonal conflicts extraneously affected rational judgments. He therefore devised a procedure that employed a series of questionnaires, in which experts were asked their opinions anonymously, had the combined results fed back to them without identification of the source, and then were asked their opinions again. Helmer's belief was that the final judgments would converge to one that was better than most of the individual judgments. I must say that though some positive evidence has been obtained, this belief still has not been substantiated. At present, the application of the Delphi method is itself an inexact science.

While it was not developed specifically for that purpose, the Delphi method has gained its principal fame through its use in the projection of future social, technical, and political developments. One such study, conducted at Rand, has become widely known. Since then a number of corporations and other institutions have carried out even more ambitious studies.

The Delphi method has followed the route of many other methodologies in whose development Rand has played a large role: it has gained more extensive application outside of Rand than within, and it has spread across national boundaries.

In addition to Delphi, Rand has been instrumental in the development of the techniques of scenario-writing, in which a careful attempt to write an artificial future history is used to insure the examination of self-consistent and realistic events; and of gaming, in which opposing teams are used to insure that the actions of malevolent competitors are fully examined. Like Delphi, both these methods depend for their success on the participation of individual experts.

Another class of methods in which Rand has pioneered has been one comprising formal solution techniques for problems of policy and operations. Linear programming, which was first conceived by Kantorovich in Leningrad in 1939, was reinvented independently after World War II by George Dantzig, who came to Rand to develop and disseminate it. Dynamic programming was invented by Richard Bellman at Rand during an attempt, later judged naive, to solve a problem of bomber allocation. The technique proved far more successful than its first application, and is now widely applied. The theory of games, invented by John von Neumann, underwent intensive development at Rand, though it rarely found application in any real problems of conflict. The understanding of the basic concepts of conflict that it provided, however, proved valuable in many Rand studies.

The computer forms the base for a wide range of possible methodologies for technology and policy studies. However, their development demands the attention of experts in computer science. Thus, just as Rand has, from its inception, had a group of mathematicians devoted to studies of methodology, so also has it had a number of computer scientists developing new computer tools. From their efforts have come a technique in which the computer is used to simulate man's informal problem solving procedures, heuristic programming, and methods whereby man and computer may cooperate in the solution of problems by interacting through convenient consoles, some equipped with electronic pencils and visual display units.

But Rand's major methodological contributions have come in the area of policy analysis. Here, a group of cost analysts has led in the development of the technique of policy-oriented budgeting, called "program budgeting," that has been adopted by the Department of Defense and many other government agencies. By providing the framework in which total-system costs can be more easily associated with the individual objectives of an organization, it has opened the way for comparison of alternative means of achieving those objectives in terms both of cost and relative effectiveness. Thus, it has enabled government to adopt the methods of cost/effectiveness analysis pioneered at Rand. Though valuable, these methods still have deficiencies, and Rand continues to work on them, hoping to develop better methods to select among alternative future systems to carry out some function; hoping, that is, to improve the methodology of system analysis.

The final class of studies that Rand performs might be called science-oriented, since their motivation is the cumulation of knowledge. Many of them have been in the political and physical sciences. Because the military objectives, the economic vitality, and the political doctrine of other nations affect the national security of the United States, Rand has conducted international political and economic studies since 1948. Among the results have been books on:

- The Real National Income of Soviet Russia Since 1928
- How Nations Negotiate
- On the Game of Politics in France
- Divided Berlin
- Burma's Foreign Policy

- Middle East Oil Crises and Western Europe's Energy Supplies
- Communist China's Strategy in the Nuclear Era

The anticipation of technical progress demands careful examination of scientific evolution. Rand has had a continuing theoretical program in the physical sciences, although it has never had laboratories for experimental research. Among its results are books on:

- Invariant Imbedding and Radiative Transfer in Slabs of Finite Thickness
- The Structure of Field Space
- Human Color Perception

We have now had a chance to see how Rand operates. During this examination we have identified some lessons for the design of the Rand-like institutions that I proposed earlier. Before attempting to bring those lessons together, let me complete the picture of Rand by presenting its vital statistics.

This year its budget will be about \$25 million. That will go to support a staff of 1100 persons and seven computers. Half of the persons are members of the research staff, and one-third of that number hold doctorates. They belong to ten research departments, organized according to disciplinary lines: Economics, Mathematics, Social Science, Engineering Sciences, Logistics, Cost Analysis, Computer Sciences, Environmental Sciences, Physics, and System Sciences. And they work on one or more of about two hundred projects, staffed with many different disciplines, and ranging in effort from a fraction of one man to ten men or more. Work for the Air Force constitutes only 60 percent of the total. The rest is sponsored by several other federal government agencies, the

City of New York, the State of Arkansas, and several private foundations.

Now let us return to an examination of the lessons of Rand's experience for other institutions that would attempt to anticipate the social consequences of scientific evolution and technical progress. The major lesson, it seems to me, is that such an institution can follow no simple formula. Its success depends on a complex and delicate balancing of contrasting influences.

For example, a major portion of its studies should be policy-oriented. By addressing issues of policy concern, the institute stands the best chance of exercising beneficial influence. But the institute must also have a large program of research that explores science, technology, and methodology. Such studies can be influential in their own right, but they also create a solid base for policy studies. They establish the substantial understanding of scientific evolution and technical progress that is essential to the analysis of policy alternatives. In turn, the policy studies guide scientific and technical explorations into potentially fruitful directions. The methodology studies provide sharper tools for all the other studies and are, reciprocally, driven to further refinement by them. Rand has found no best way to reach the proper balance among these types of study. At present, no more than half its effort is devoted to policy-oriented research.

The second balance that must be struck is between independence and influence. The former requires a certain detachment and flexibility; the latter flourishes when contacts are close and structured. Both of them seem to take time to develop. It is difficult to offer advice on this matter, except to say that both are critical, but difficult to achieve. Much depends on organizational, and even national, traditions. Several foreign visitors to Rand have commented that they find it hard to imagine in their countries a situation, not uncommon to Rand, in which a young scientist explains to senior generals that they are mistaken on a military matter. Yet this possibility symbolizes the combination of independence and influence that a successful institution of the kind I have proposed must have.

The third balance is between studies that draw on the skills of many disciplines and those that stay within discipline boundaries. Most studies that anticipate the consequences of science and technology must face problems that cut across the concerns of many disciplines: physical science, engineering, social science, mathematics, and so on. To conduct them, interdisciplinary teams comprising specialists from many areas must be formed. Many of Rand's studies are carried out by such teams. Its success in forming interdisciplinary teams of high competence and of many different sizes and compositions, in accordance with study needs, has distinguished Rand from most other organizations. The teams have been essential to the success of Rand's studies. Yet, many of the most crucial studies, especially those that explore new directions in science, technology, and methodology, have remained firmly within discipline bounds. They still constitute a large portion of Rand's research program.

The fourth balance is between a research charter that covers a broad spectrum -- national security or urban problems, for example; and one that is limited to a narrow band of the spectrum -- military control systems or urban transportation, for example. This is really a problem of degree rather than one of balance. A narrow charter leads to research that is appropriate to immediate, anticipated, specified needs. But even within a broad charter, effective research is likely to concentrate on those narrower issues that are vital at the moment. The broad charter's advantages are, first, that it enables a line of research to follow its findings to new areas of relevance; and, second, that it enables interrelations among subjects as diverse as, for example, education and transportation to be exploited.

A recent Rand experience is illuminating in this regard. Several years ago, Rand undertook studies of the problem of distinguishing between decoy and real missile warheads as they reentered the atmosphere. This problem is critical to anti-ballistic missile defense, of course. Attempts to carry out the discrimination automatically were proving difficult, so some Rand researchers decided to explore the possibility of using a rather old-fashioned device: the human eye and mind. One problem was that much of the information needed for discrimination would not lie in the visible spectrum. So research was begun on methods for transposing it to visible form, preserving certain spectral differences as color differences. This led to a concern with human color vision and then, more generally, to a concern with the eye. As a consequence, the fluid dynamicist who had been originally concerned with ballistic missile reentry problems is now studying the flow of blood in the very small vessels of the eye. And the work of his group has led to proposals for a stroke-detection clinic, in which examination of the eye will help warn of potential strokes. Thus, under Rand's broad charter -- to work on the public welfare and security -- this group has progressed from missile warhead detection to stroke detection.

The fifth balance is between a concern with systems, such as those that protect a country against missile attack or transport passengers within a city, and a concern with their constituents, such as radars or missiles or buses or trains. Once again, we can only conclude that both kinds of study are essential; each gains immeasurably from being carried out in conjunction with the others. Rand's studies of broad strategy issues have always benefited from having studies of constituent military systems available, and vice versa.

Finally, we turn to a balance that addresses our fundamental concern: projection of future developments. Should the institute concern itself only with the future? Or should it limit itself to the present? Again the answer, it seems to me, is that a balance must be struck. Projections into the future should rest on a thorough comprehension of the present. And our grasp of the here-and-now is often improved by attempts to see where it is heading. Rand has, throughout its history, always conducted a range of studies, the most numerous devoted to short-range questions, with decreasing numbers associated with longer-range problems. The proper distribution here is exceedingly difficult to specify. But the tendency of organizations either to become rooted in immediate problems or to drift off into speculations on the distant future must be avoided. The institute should let the nature of the issues that it addresses and the character of developments in science and technology dictate its balance between short- and long-range studies.

We can return now to the proposal with which this discussion of Rand's experience began. I hope I have been able to demonstrate to you that independent institutions to study the consequences of scientific evolution and technical progress for society are feasible and that their influence can be substantial. I hope also that I have been able to show that their success depends on a delicate balance of competing influences. While in its central research themes the institution must be policy-oriented, independent, interdisciplinary, broad in scope, systems-oriented, and future-oriented; it must take care to support those themes with a network of studies comprising just the opposite approaches.

To build such institutions will be a difficult task, but the magnitude of the job is warranted by the size of the prospective benefits. Through such institutions society will be better able to anticipate the consequences of scientific and technical progress and, thereby, more likely to reap the benefits and avoid the burdens. I hope the time is not distant when we shall see such institutions serving every nation -- and the community of nations.