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What We Must Do

A large-scale mobilization of scientists may be the only way to solve our crisis problems.

John Platt

There is only one crisis in the world. It is the crisis of transformation. The trouble is that it is now coming upon us as a storm of crisis problems from every direction. But if we look quantitatively at the course of our changes in this century, we can see immediately why the problems are building up so rapidly at this time, and we will see that it has now become urgent for us to mobilize all our intelligence to solve these problems if we are to keep from killing ourselves in the next few years.

The essence of the matter is that the human race is on a steeply rising "S-curve" of change. We are undergoing a great historical transition to new levels of technological power all over the world. We all know about these changes, but we do not often stop to realize how large they are in orders of magnitude, or how rapid and enormous compared to all previous changes in history. In the last century, we have increased our speeds of communication by a factor of 10^7; our speeds of travel by 10^2; our speeds of data handling by 10^6; our energy resources by 10^3; our power of weapons by 10^6; our ability to control diseases by something like 10^2; and our rate of population growth to 10^3 times what it was a few thousand years ago.

Could anyone suppose that human relations around the world would not be affected to their very roots by such changes? Within the last 25 years, the Western world has moved into an age of jet planes, missiles and satellites, nuclear power and nuclear terror. We have acquired computers and automation, a service and leisure economy, superhighways, superagriculture, supermedicine, mass higher education, universal TV, oral contraceptives, environmental pollution, and urban crises. The rest of the world is also moving rapidly and may catch up with all these powers and problems within a very short time. It is hardly surprising that young people under 30, who have grown up familiar with these things from childhood, have developed very different expectations and concerns from the older generation that grew up in another world.

What many people do not realize is that many of these technological changes are now approaching certain natural limits. The "S-curve" is beginning to level off. We may never have faster communications or more TV or larger weapons or a higher level of danger than we have now. This means that if we could learn how to manage these new powers and problems in the next few years without killing ourselves by our obsolete structures and behavior, we might be able to create new and more effective social structures that would last for many generations. We might be able to move into that new world of abundance and diversity and well-being for all mankind which technology has now made possible.

The trouble is that we may not survive these next few years. The human race today is like a rocket on a launching pad. We have been building up to this moment of takeoff for a long time, and if we can get safely through the takeoff period, we may fly on a new and exciting course for a long time to come. But at this moment, as the powerful new engines are fired, their thrust and roar shakes and stresses every part of the ship and may cause the whole thing to blow up before we can steer it on its way. Our problem today is to harness and direct these tremendous new forces through this dangerous transition period to the new world instead of to destruction. But unless we can do this, the rapidly increasing strains and crises of the next decade may kill us all. They will make the last 20 years look like a peaceful interlude.

The Next 10 Years

Several types of crisis may reach the point of explosion in the next 10 years: nuclear escalation, famine, participatory crises, racial crises, and what have been called the crises of administrative legitimacy. It is worth singling out two or three of these to see how imminent and dangerous they are, so that we can fully realize how very little time we have for preventing or controlling them.

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Take the problem of nuclear war, for example. A few years ago, Leo Szilard estimated the "half-life" of the human race with respect to nuclear escalation as being between 10 and 20 years. His reasoning then is still valid now. As long as we continue to have no adequate stabilizing peace-keeping structures for the world, we continue to live under the daily threat not only of local wars but of nuclear escalation with overkill and megatonnage enough to destroy all life on earth. Every year or two there is a confrontation between nuclear powers—Korea, Laos, Berlin, Suez, Quemoy, Cuba, Vietnam, and the rest. MacArthur wanted to use nuclear weapons in Korea; and in the Cuban missile crisis, John Kennedy is said to have estimated the probability of a nuclear exchange as about 25 percent.

The danger is not so much that of the unexpected, such as a radar error or even a new nuclear dictator, as it is that our present systems will work exactly as planned!—from border testing, strategic gambles, threat and counter-threat, all the way up to that "second-strike capability" that is already aimed, armed, and triggered to wipe out hundreds of millions of people in a 3-hour duel!

What is the probability of this in the average incident? 10 percent? 5 percent? There is no average incident. But it is easy to see that five or ten more such confrontations in this game of "nuclear roulette" might indeed give us only a 50-50 chance of living until 1980 or 1990. This is a shorter life expectancy than people have ever had in the world before. All our medical increases in length of life are meaningless, as long as our nuclear lifetime is so short.

Many agricultural experts also think that within this next decade the great famines will begin, with deaths that may reach 100 million people in densely populated countries like India and China. Some contradict this, claiming that the remarkable new grains and new agricultural methods introduced in the last 3 years in Southeast Asia may now be able to keep the food supply ahead of population growth. But others think that the reeducation of farmers and consumers to use the new grains cannot proceed fast enough to make a difference.

But if famine does come, it is clear that it will be catastrophic. Besides the direct human suffering, it will further increase our international instabilities, with food riots, troops called out, governments falling, and international interventions that will change the whole political map of the world. It could make Vietnam look like a popgun.

In addition, the next decade is likely to see continued crises of legitimacy of all our overloaded administrations, from universities and unions to cities and national governments. Everywhere there is protest and refusal to accept the solutions handed down by some central elite. The student revolutions circle the globe. Suburbs protest as well as ghettoes, Right as well as Left. There are many new sources of collision and protest, but it is clear that the general problem is in large part structural rather than political. Our traditional methods of election and management no longer give administrations the skill and capacity they need to handle their complex new burdens and decisions. They become swollen, unresponsive—and repudiated. Every day now some distinguished administrator is pressured out of office by protesting constituents.

In spite of the violence of some of these confrontations, this may seem like a trivial problem compared to war or famine—until we realize the dangerous effects of these instabilities on the stability of the whole system. In a nuclear crisis or in any of our other crises today, administrators or negotiators may often work out some basis of agreement between conflicting groups or nations, only to find themselves rejected by their people on one or both sides, who are then left with no mechanism except to escalate their battles further.

The Crisis of Crises

What finally makes all of our crises still more dangerous is that they are now coming on top of each other. Most administrations are able to endure or even enjoy an occasional crisis, with everyone working late together and getting a new sense of importance and unity. What they are not prepared to deal with are multiple crises, a crisis of crises all at one time. This is what happened in New York City in 1968 when the Ocean Hill-Brownsville teacher and race strike was combined with a police strike, on top of a garbage strike, on top of a longshoremen's strike, all within a few days of each other.

When something like this happens, the staffs get jumpy with smoke and coffee and alcohol, the mediators become exhausted, and the administrators find themselves running two crises behind. Every problem may escalate because those involved no longer have time to think straight. What would have happened in the Cuban missile crisis if the East Coast power blackout had occurred by accident that same day? Or if the "hot line" between Washington and Moscow had gone dead? There might have been hours of misinterpretation, and some fatally different decisions.

I think this multiplication of domestic and international crises today will shorten that short half-life. In the continued absence of better ways of heading off these multiple crises, our half-life may no longer be 10 or 20 years, but more like 5 to 10 years, or less. We may have even less than a 50-50 chance of living until 1980.

This statement may seem uncertain and excessively dramatic. But is there any scientist who would make a much more optimistic estimate after considering all the different sources of danger and how they are increasing? The shortness of the time is due to the exponential and multiplying character of our problems and not to what particular numbers or guesses we put in. Anyone who feels more hopeful about getting past the nightmares of the 1970's has only to look beyond them to the monsters of pollution and population rising up in the 1980's and 1990's. Whether we have 10 years or more like 20 or 30, unless we systematically find new large-scale solutions, we are in the gravest danger of destroying our society, our world, and ourselves in any of a number of different ways well before the end of this century. Many futurologists who have predicted what the world will be like in the year 2000 have neglected to tell us that.

Nevertheless the real reason for trying to make rational estimates of these deadlines is not because of their shock value but because they give us at least a rough idea of how much time we may have for finding and mounting some large-scale solutions. The time is short but, as we shall see, it is not too short to give us a chance that something can be done, if we begin immediately.

From this point, there is no place to go but up. Human predictions are always conditional. The future always depends on what we do and can be made worse or better by stupid or intelligent action. To change our earlier analogy, today we are like men coming out of a coal mine who suddenly begin to hear the rock rumbling, but who have also begun to see a little square of light at the end of the tunnel. Against this background, I am an optimist—in that I want to insist that there is a square of light and that it is worth trying to get to. I think what we must do is to start running as fast as possible toward that light, working to increase the probability of our survival through the next decade by some measurable amount.

For the light at the end of the tunnel is very bright indeed. If we can only devise new mechanisms to help us survive this round of terrible crises, we have a chance of moving into a new world of incredible potentialities for all mankind. But if we cannot get through this next decade, we may never reach it.

Task Forces for Social Research and Development

What can we do? I think that nothing less than the application of the full intelligence of our society is likely to be adequate. These problems will require the humane and constructive efforts of everyone involved. But I think they will also require something very similar to the mobilization of scientists for solving crisis problems in wartime. I believe we are going to need large numbers of scientists forming something like research teams or task forces for social research and development. We need full-time interdisciplinary teams combining men of different specialties, natural scientists, social scientists, doctors, engineers, teachers, lawyers, and many other trained and inventive minds, who can put together our stores of knowledge and powerful new ideas into improved technical methods, organizational designs, or "social inventions" that have a chance of being adopted soon enough and widely enough to be effective. Even a great mobilization of scientists may not be enough. There is no guarantee that these problems can be solved, or solved in time, no matter what we do. But for problems of this scale and urgency, this kind of focusing of our brains and knowledge may be the only chance we have.

Scientists, of course, are not the only ones who can make contributions. Millions of citizens, business and labor leaders, city and government officials, and workers in existing agencies, are already doing all they can to solve these problems. No scientific innovation will be effective without extensive advice and help from all these groups.

But it is the new science and technology that have made our problems so immense and intractable. Technology did not create human conflicts and inequities, but it has made them unendurable. And where science and technology have expanded the problems in this way, it may be only more scientific understanding and better technology that can carry us past them. The cure for the pollution of the rivers by detergents is the use of nonpolluting detergents. The cure for bad management designs is better management designs.

Also, in many of these areas, there are few people outside the research community who have the basic knowledge necessary for radically new solutions. In our great biological problems, it is the new ideas from cell biology and ecology that may be crucial. In our social-organizational problems, it may be the new theories of organization and management and behavior theory and game theory that offer the only hope. Scientific research and development groups of some kind may be the only effective mechanism by which many of these new ideas can be converted into practical invention and action.

The time scale on which such task forces would have to operate is very different from what is usual in science. In the past, most scientists have tended to work on something like a 30-year time scale, hoping that their careful studies would fit into some great intellectual synthesis that might be years away. Of course when they become politically concerned, they begin to work on something more like a 3-month time scale, collecting signatures or trying to persuade the government to start or stop some program.

But 30 years is too long, and 3 months is too short, to cope with the major crises that might destroy us in the next 10 years. Our urgent problems now are more like wartime problems, where we need to work as rapidly as is consistent with large-scale effectiveness. We need to think rather in terms of a 3-year time scale—or more broadly, a 1- to 5-year time scale. In World War II, the ten thousand scientists who were mobilized for war research knew they did not have 30 years, or even 10 years, to come up with answers. But they did have time for the new research, design, and construction that brought sonar and radar and atomic energy to operational effectiveness within 1 to 4 years. Today we need the same large-scale mobilization for innovation and action and the same sense of constructive urgency.

Priorities: A Crisis Intensity Chart

In any such enterprise, it is most important to be clear about which problems are the real priority problems. To get this straight, it is valuable to try to separate the different problem areas according to some measures of their magnitude and urgency. A possible classification of this kind is shown in Tables 1 and 2. In these tables, I have tried to rank a number of present or potential problems or crises, vertically, according to an estimate of their order of intensity or "seriousness," and horizontally, by a rough estimate of their time to reach climactic importance. Table 1 is such a classification for the United States for the next 1 to 5 years, the next 5 to 20 years, and the next 20 to 50 years. Table 2 is a similar classification for world problems and crises.

The successive rows indicate something like order-of-magnitude differences in the intensity of the crises, as estimated by a rough product of the size of population that might be hurt or affected, multiplied by some estimated average effect in the disruption of their lives. Thus the first row corresponds to total or near-total annihilation; the second row, to great destruction or change affecting everybody; the third row, to a lower tension affecting a smaller part of the population or a smaller part of everyone's life, and so on.

Informed men might easily disagree about one row up or down in intensity, or one column left or right in the time scales, but these order-of-magnitude differences are already so great that it would be surprising to find much larger disagreements. Clearly, an important initial step in any serious problem study would be to refine such estimates.

In both tables, the one crisis that must be ranked at the top in total danger and imminence is, of course, the danger of large-scale or total annihilation by nuclear escalation or by radiological-chemical-biological-warfare (RCBW). This kind of crisis will continue through both the 1- to 5-year time period and the 5- to 20-year period as Crisis Number 1, unless and until we get a safer peace-keeping arrangement. But in the 20- to 50-year column, following the reasoning already given, I think we must simply put a big "✠" at this level, on the grounds that the peace-keeping stabilization problem will either be solved by that time or we will probably be dead.

At the second level, the 1- to 5-year

period may not be a period of great destruction (except nuclear) in either the United States or the world. But the problems at this level are building up, and within the 5- to 20-year period, many scientists fear the destruction of our whole biological and ecological balance in the United States by mismanagement or pollution. Others fear political catastrophe within this period, as a result of participatory confrontations or backlash or even dictatorship, if our divisive social and structural problems are not solved before that time.

On a world scale in this period, famine and ecological catastrophe head the list of destructive problems. We will come back later to the items in the 20- to 50-year column.

The third level of crisis problems in the United States includes those that are already upon us: administrative management of communities and cities, slums, participatory democracy, and racial conflict. In the 5- to 20-year period, the problems of pollution and poverty or major failures of law and justice could escalate to this level of tension if they are not solved. The last column is left blank because secondary events and second-order effects will interfere seriously with any attempt to make longer-range predictions at these lower levels.

The items in the lower part of the tables are not intended to be exhaustive. Some are common headline problems which are included simply to show how they might rank quantitatively in this kind of comparison. Anyone concerned with any of them will find it a useful exercise to estimate for himself their order of seriousness, in terms of the number of people they actually affect and the average distress they cause. Transportation problems and neighborhood ugliness, for example, are listed as grade 4 problems in the United States because they depress the lives of tens of millions for 1 or 2 hours every day. Violent crime may affect a corresponding number every year or two. These evils are not negligible, and they are worth the efforts of enormous numbers of people to cure them and to keep them cured—but on the other hand, they will not destroy our society.

The grade 5 crises are those where the hue and cry has been raised and where responsive changes of some kind are already under way. Cancer goes here, along with problems like auto safety and an adequate water supply. This is not to say that we have solved the problem of cancer, but rather that good people are working on it and are making as much progress as we could expect from anyone. (At this level of social intensity, it should be kept in mind that there are also positive opportunities for research, such as the automation of clinical biochemistry or the invention of new channels of personal communication, which might affect the 20-year future as greatly as the new drugs and solid state devices of 20 years ago have begun to affect the present.)

Table 1. Classification of problems and crises by estimated time and intensity (United States).

Grade	Estimated crisis intensity (number affected × degree of effect)		Estimated time to crisis* 1 to 5 years	5 to 20 years	20 to 50 years
1.		Total annihilation	Nuclear or RCBW escalation	Nuclear or RCBW escalation	✠ (Solved or dead)
2.	10^8	Great destruction or change (physical, biological, or political)	(Too soon)	Participatory democracy Ecological balance	Political theory and economic structure Population planning Patterns of living Education Communications Integrative philosophy
3.	10^7	Widespread almost unbearable tension	Administrative management Slums Participatory democracy Racial conflict	Pollution Poverty Law and justice	?
4.	10^6	Large-scale distress	Transportation Neighborhood ugliness Crime	Communications gap	?
5.	10^5	Tension producing responsive change	Cancer and heart Smoking and drugs Artificial organs Accidents Sonic boom Water supply Marine resources Privacy on computers	Educational inadequacy	?
6.		Other problems—important, but adequately researched	Military R & D New educational methods Mental illness Fusion power	Military R & D	
7.		Exaggerated dangers and hopes	Mind control Heart transplants Definition of death	Sperm banks Freezing bodies Unemployment from automation	Eugenics
8.		Noncrisis problems being "overstudied"	Man in space Most basic science		

* If no major effort is made at anticipatory solution.

Where the Scientists Are

Below grade 5, three less quantitative categories are listed, where the scientists begin to outnumber the problems. Grade 6 consists of problems that many people believe to be important but that are adequately researched at the present time. Military R & D belongs in this category. Our huge military establishment creates many social problems, both of national priority and international stability, but even in its own terms, war research, which engrosses hundreds of thousands of scientists and engineers, is being taken care of generously. Likewise, fusion power is being studied at the $100-million level, though even if we had it tomorrow, it would scarcely change our rates of application of nuclear energy in generating more electric power for the world.

Grade 7 contains the exaggerated problems which are being talked about or worked on out of all proportion to their true importance, such as heart transplants, which can never affect more than a few thousands of people out of the billions in the world. It is sad to note that the symposia on "social implications of science" at many national scientific meetings are often on the problems of grade 7.

In the last category, grade 8, are two subjects which I am sorry to say I must call "overstudied," at least with respect to the real crisis problems today. The Man in Space flights to the moon and back are the most beautiful technical achievements of man, but they are not urgent except for national display, and they absorb tens of thousands of our most ingenious technical brains.

And in the "overstudied" list I have begun to think we must now put most of our basic science. This is a hard conclusion, because all of science is so important in the long run and because it is still so small compared, say, to advertising or the tobacco industry. But basic scientific thinking is a scarce resource. In a national emergency, we would suddenly find that a host of our scientific problems could be postponed for several years in favor of more urgent research. Should not our total human emergency make the same claims? Long-range science is useless unless we survive to use it. Tens of thousands of our best trained minds may now be needed for something more important than "science as usual."

The arrows at level 2 in the tables are intended to indicate that problems may escalate to a higher level of crisis in the next time period if they are not solved. The arrows toward level 2 in the last columns of both tables show the escalation of all our problems upward to some general reconstruction in the 20- to 50-year time period, if we survive. Probably no human institution will continue unchanged for another 50 years, because they will all be changed by the crises if they are not changed in advance to prevent them. There will surely be widespread rearrangements in all our ways of life everywhere, from our pat-

Table 2. Classification of problems and crises by estimated time and intensity (World).

Grade	Estimated crisis intensity (number affected × degree of effect)		Estimated time to crisis* 1 to 5 years	5 to 20 years	20 to 50 years
1.	10^{10}	Total annihilation	Nuclear or RCBW escalation	Nuclear or RCBW escalation	✠ (Solved or dead)
2.	10^{9}	Great destruction or change (physical, biological, or political)	(Too soon)	Famines Ecological balance Development failures Local wars Rich-poor gap	Economic structure and political theory Population and ecological balance Patterns of living Universal education Communications-integration Management of world Integrative philosophy
3.	10^{8}	Widespread almost unbearable tension	Administrative management Need for participation Group and racial conflict Poverty–rising expectations Environmental degradation	Poverty Pollution Racial wars Political rigidity Strong dictatorships	?
4.	10^{7}	Large-scale distress	Transportation Diseases Loss of old cultures	Housing Education Independence of big powers Communications gap	?
5.	10^{6}	Tension producing responsive change	Regional organization Water supplies	?	?
6.		Other problems—important, but adequately researched	Technical development design Intelligent monetary design		
7.		Exaggerated dangers and hopes			Eugenics Melting of ice caps
8.		Noncrisis problems being "overstudied"	Man in space Most basic science		

* If no major effort is made at anticipatory solution.

terns of society to our whole philosophy of man. Will they be more humane, or less? Will the world come to resemble a diverse and open humanist democracy? Or Orwell's *1984*? Or a postnuclear desert with its scientists hanged? It is our acts of commitment and leadership in the next few months and years that will decide.

Mobilizing Scientists

It is a unique experience for us to have peacetime problems, or technical problems which are not industrial problems, on such a scale. We do not know quite where to start, and there is no mechanism yet for generating ideas systematically or paying teams to turn them into successful solutions.

But the comparison with wartime research and development may not be inappropriate. Perhaps the antisubmarine warfare work or the atomic energy project of the 1940's provide the closest parallels to what we must do in terms of the novelty, scale, and urgency of the problems, the initiative needed, and the kind of large success that has to be achieved. In the antisubmarine campaign, Blackett assembled a few scientists and other ingenious minds in his "back room," and within a few months they had worked out the "operations analysis" that made an order-of-magnitude difference in the success of the campaign. In the atomic energy work, scientists started off with extracurricular research, formed a central committee to channel their secret communications, and then studied the possible solutions for some time before they went to the government for large-scale support for the great development laboratories and production plants.

Fortunately, work on our crisis problems today would not require secrecy. Our great problems today are all beginning to be world problems, and scientists from many countries would have important insights to contribute.

Probably the first step in crisis studies now should be the organization of intense technical discussion and education groups in every laboratory. Promising lines of interest could then lead to the setting up of part-time or full-time studies and teams and coordinating committees. Administrators and boards of directors might find active crisis research important to their own organizations in many cases. Several foundations and federal agencies already have in-house research and make outside grants in many of these crisis areas, and they would be important initial sources of support.

But the step that will probably be required in a short time is the creation of whole new centers, perhaps comparable to Los Alamos or the RAND Corporation, where interdisciplinary groups can be assembled to work full-time on solutions to these crisis problems. Many different kinds of centers will eventually be necessary, including research centers, development centers, training centers, and even production centers for new sociotechnical inventions. The problems of our time—the $100-billion food problem or the $100-billion arms control problem—are no smaller than World War II in scale and importance, and it would be absurd to think that a few academic research teams or a few agency laboratories could do the job.

Social Inventions

The thing that discourages many scientists—even social scientists—from thinking in these research-and-development terms is their failure to realize that there are such things as social inventions and that they can have large-scale effects in a surprisingly short time. A recent study with Karl Deutsch has examined some 40 of the great achievements in social science in this century, to see where they were made and by whom and how long they took to become effective. They include developments such as the following:

- Keynesian economics
- Opinion polls and statistical sampling
- Input-output economics
- Operations analysis
- Information theory and feedback theory
- Theory of games and economic behavior
- Operant conditioning and programmed learning
- Planned programming and budgeting (PPB)
- Non–zero-sum game theory

Many of these have made remarkable differences within just a few years in our ability to handle social problems or management problems. The opinion poll became a national necessity within a single election period. The theory of games, published in 1946, had become an important component of American strategic thinking by RAND and the Defense Department by 1953, in spite of the limitation of the theory at that time to zero-sum games, with their dangerous bluffing and "brinksmanship." Today, within less than a decade, the PPB management technique is sweeping through every large organization.

This list is particularly interesting because it shows how much can be done outside official government agencies when inventive men put their brains together. Most of the achievements were the work of teams of two or more men, almost all of them located in intellectual centers such as Princeton or the two Cambridges.

The list might be extended by adding commercial social inventions with rapid and widespread effects, like credit cards. And sociotechnical inventions, like computers and automation or like oral contraceptives, which were in widespread use within 10 years after they were developed. In addition, there are political innovations like the New Deal, which made great changes in our economic life within 4 years, and the pay-as-you-go income tax, which transformed federal taxing power within 2 years.

On the international scene, the Peace Corps, the "hot line," the Test-Ban Treaty, the Antarctic Treaty, and the Nonproliferation Treaty were all implemented within 2 to 10 years after their initial proposal. These are only small contributions, a tiny patchwork part of the basic international stabilization system that is needed, but they show that the time to adopt new structural designs may be surprisingly short. Our cliches about "social lag" are very misleading. Over half of the major social innovations since 1940 were adopted or had widespread social effects within less than 12 years—a time as short as, or shorter than, the average time for adoption of technological innovations.

Areas for Task Forces

Is it possible to create more of these social inventions systematically to deal with our present crisis problems? I think it is. It may be worth listing a few specific areas where new task forces might start.

1) *Peace-keeping mechanisms and feedback stabilization.* Our various nuclear treaties are a beginning. But how about a technical group that sits down and thinks about the whole range of possible and impossible stabilization and peace-keeping mechanisms? Stabiliza-

tion feedback-design might be a complex modern counterpart of the "checks and balances" used in designing the constitutional structure of the United States 200 years ago. With our new knowledge today about feedbacks, group behavior, and game theory, it ought to be possible to design more complex and even more successful structures.

Some peace-keeping mechanisms that might be hard to adopt today could still be worked out and tested and publicized, awaiting a more favorable moment. Sometimes the very existence of new possibilities can change the atmosphere. Sometimes, in a crisis, men may finally be willing to try out new ways and may find some previously prepared plan of enormous help.

2) *Biotechnology*. Humanity must feed and care for the children who are already in the world, even while we try to level off the further population explosion that makes this so difficult. Some novel proposals, such as food from coal, or genetic copying of champion animals, or still simpler contraceptive methods, could possibly have large-scale effects on human welfare within 10 to 15 years. New chemical, statistical, and management methods for measuring and maintaining the ecological balance could be of very great importance.

3) *Game theory*. As we have seen, zero-sum game theory has not been too academic to be used for national strategy and policy analysis. Unfortunately, in zero-sum games, what I win, you lose, and what you win, I lose. This may be the way poker works, but it is not the way the world works. We are collectively in a non–zero-sum game in which we will all lose together in nuclear holocaust or race conflict or economic nationalism, or all win together in survival and prosperity. Some of the many variations of non–zero-sum game theory, applied to group conflict and cooperation, might show us profitable new approaches to replace our sterile and dangerous confrontation strategies.

4) *Psychological and social theories*. Many teams are needed to explore in detail and in practice how the powerful new ideas of behavior theory and the new ideas of responsive living might be used to improve family life or community and management structures. New ideas of information handling and management theory need to be turned into practical recipes for reducing the daily frustrations of small businesses, schools, hospitals, churches, and town meetings. New economic inventions are needed, such as urban development corporations. A deeper systems analysis is urgently needed to see if there is not some practical way to separate full employment from inflation. Inflation pinches the poor, increases labor-management disputes, and multiplies all our domestic conflicts and our sense of despair.

5) *Social indicators*. We need new social indicators, like the cost-of-living index, for measuring a thousand social goods and evils. Good indicators can have great "multiplier effects" in helping to maximize our welfare and minimize our ills. Engineers and physical scientists working with social scientists might come up with ingenious new methods of measuring many of these important but elusive parameters.

6) *Channels of effectiveness*. Detailed case studies of the reasons for success or failure of various social inventions could also have a large multiplier effect. Handbooks showing what channels or methods are now most effective for different small-scale and large-scale social problems would be of immense value.

The list could go on and on. In fact, each study group will have its own pet projects. Why not? Society is at least as complex as, say, an automobile with its several thousand parts. It will probably require as many research-and-development teams as the auto industry in order to explore all the inventions it needs to solve its problems. But it is clear that there are many areas of great potential crying out for brilliant minds and brilliant teams to get to work on them.

Future Satisfactions and Present Solutions

This is an enormous program. But there is nothing impossible about mounting and financing it, if we, as concerned men, go into it with commitment and leadership. Yes, there will be a need for money and power to overcome organizational difficulties and vested interests. But it is worth remembering that the only real source of power in the world is the gap between what is and what might be. Why else do men work and save and plan? If there is some future increase in human satisfaction that we can point to and realistically anticipate, men will be willing to pay something for it and invest in it in the hope of that return. In economics, they pay with money; in politics, with their votes and time and sometimes with their jail sentences and their lives.

Social change, peaceful or turbulent, is powered by "what might be." This means that for peaceful change, to get over some impossible barrier of unresponsiveness or complexity or group conflict, what is needed is an inventive man or group—a "social entrepreneur" —who can connect the pieces and show how to turn the advantage of "what might be" into some present advantage for every participating party. To get toll roads, when highways were hopeless, a legislative-corporation mechanism was invented that turned the future need into present profits for construction workers and bondholders and continuing profitability for the state and all the drivers.

This principle of broad-payoff anticipatory design has guided many successful social plans. Regular task forces using systems analysis to find payoffs over the barriers might give us such successful solutions much more often. The new world that could lie ahead, with its blocks and malfunctions removed, would be fantastically wealthy. It seems almost certain that there must be many systematic ways for intelligence to convert that large payoff into the profitable solution of our present problems.

The only possible conclusion is a call to action. Who will commit himself to this kind of search for more ingenious and fundamental solutions? Who will begin to assemble the research teams and the funds? Who will begin to create those full-time interdisciplinary centers that will be necessary for testing detailed designs and turning them into effective applications?

The task is clear. The task is huge. The time is horribly short. In the past, we have had science for intellectual pleasure, and science for the control of nature. We have had science for war. But today, the whole human experiment may hang on the question of how fast we now press the development of science for survival.