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ABSTRACT

This collection of 18 papers is concerned with the beliefs, methods, practices, and results associated with the type of forecasting which has become known in the last 10 to 15 years as "futures research." Topics discussed include: (1) forecasting methodology; (2) the validity of forecasting systems; (3) unforeseen developments; (4) forecasting in political science, sociology, technology, and economics; (5) normative forecasting; (6) forecasting for decisionmaking and policymaking; (7) professional issues in forecasting research; and (8) the future of futures research. Results of a survey of current forecasting efforts and a bibliography are appended. (Author/STS)

THE STUDY OF THE FUTURE: AN AGENDA FOR RESEARCH

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foreword

As we study our current situation and explore ways to improve it, we find that if we are to enhance human survival and well-being, we must be concerned about the future. For human survival and well-being depend upon our ability to anticipate and cope with future problems and threats, to perceive, evaluate and control the effects of our actions, and to imagine and create more desirable futures.

How we can improve our ability to pursue these goals through research is the subject of this volume.

If we are to enhance human survival and well-being, we need to improve our capacity to ask and answer questions about the future and implement the answers, increase our understanding and agreement about future problems and options in order to make possible effective action, and create a process to improve such capacity, understanding and agreement.

Important elements of this difficult and important process include actions to perform research concerning the future, to evaluate and revise the content and methods of such research, and to communicate and use the results. We need, for example, to improve the adequacy, clarity and precision not only of the concepts which we use to think about the future, but also of the symbols (the words, mathematical symbols and graphic/visual techniques) which we use to express, manipulate and communicate the concepts.

The following are some of the important questions which we need to address. What *aspects of the future* do we wish to consider, for example, climate, environment, resources, technology, the economy, social structure, and human values? What are the *characteristics* of these elements, for example, predictability, uniqueness, stability, and controlability? What *factual, policy, and value questions* should we ask about future events? For example, what future events and impacts are *possible*? How *probable* are they? What sorts of *contingency* are involved. That is, if we take a particular action, then what will be the impacts, or if we wish to achieve a particular goal, then what actions must we take? With what *degree of certainty* can we make statements about possibilities, probabilities and contingencies? How *desirable* and *popular* are the events and impacts, for what systems, groups, and values? What actions should we actually take? We need also to ask what intellectual tools, technologies, social processes, and institutions can we develop and use if we wish to address these problems and questions, and to implement the answers effectively?

This book was created in order to help us address many of the problems, questions, and needs outlined above. It contains an agenda for research which presents a challenge and an opportunity to those who perform, support, and use research on the future, and is the result of a project supported by the National Science Foundation's Research Applied to National Needs (RANN) Program in order to assess the state-of-the-art and needs for additional research in the field of forecasting. Although the conferences leading to this volume were held in January 1974, and the papers printed here were prepared primarily during 1973-1975, the volume is still timely since cumulative progress in this field is unfortunately slow.

It is hoped that, by helping to stimulate research on the future and raise its quality, foster an ongoing dialogue about subjects and methods for research, and strengthen the community of performers and users of research on the future, this book will help us more effectively to anticipate and cope with future problems, threats, and opportunities; to perceive, evaluate, and control the effects of our actions; and to imagine, design, and create more desirable futures.

Robert W. Lamson
Program Manager
Division of Intergovernmental Science
and Public Technology
National Science Foundation

November 1976

preface

This volume is concerned with forecasting. In particular, it is concerned with the various beliefs, methods, practices, and results associated with a kind of forecasting that has come to be referred to in the last 10 to 15 years as "futures research."

Most earlier works on these subjects—and there are many—have been philosophical or procedural, descriptive or exhortatory. They have tried to help us understand why we should study the future; how and to what extent this is possible; what the consequences may be if we fail to forecast responsibly or if we fail to use the best available forecasts; who is or should be involved in preparing and evaluating forecasts; where errors have been made; how forecasting can be made to complement planning and to strengthen decisionmaking; what subjects have been or should be addressed; how forecasting articulates with the larger needs and interests of society; and the like. But even the very best of these works has given us only slight guidance on the crucial question of what can and ought to be done to overcome current constraints on forecasting, constraints that prevent forecasting from making the kinds of contributions that could otherwise reasonably be expected of it.

Suggestions abound, of course. Lacking, however, has been an attempt to view and review such suggestions in context with each other or against a common backdrop of actual experience in forecasting. Also missing from earlier discussions has been a careful and systematic effort to single out those difficulties that can be dealt with only through research, as opposed to, say, political action, institutional change, or education. Finally, even where questions that best lend themselves to research have been identified, few authors indeed have tried to evaluate priorities among the candidates. In short, while most authors agree that forecasting is still one of the primitive arts and that research can make a difference in many instances, the field is without a research agenda. The specific concern and aim of this book is to describe the elements of such an agenda and then to organize and evaluate these elements.

The difficulties of such an undertaking are beyond the competence of a single individual, for account must be taken of issues of philosophy, theory, methodology, institutionalization, professionalism, implementation, and use, and each of these sets of issues must be seen in a way that cuts across many disciplines. Fortunately, with the support of a grant from the U.S. National Science Foundation, it was possible to draw upon the insight and experience of some of the world's outstanding authorities on forecasting in the course of preparing this volume. In addition to those who contributed chapters to this book, important assistance was received from the nearly 200 persons and organizations throughout the world that contributed to the survey of research in progress which is summarized in the Appendix. And, beyond that, there was also the direct participation of a number of U.S. specialists on forecasting who took part in conferences conducted by The Futures Group in January 1974—also sponsored by the U.S. National Science Foundation as part of this project—at which many of the chapters published in this book were first presented as papers and critiqued. The attendees included the following individuals:

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Norman C. Dalkey
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Jonathan Ward
CBS Radio
New York, New York

Roger Wescott
Drew University
Madison, New Jersey

John Wilkinson
Center for the Study of
Democratic Institutions
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This book obviously would not have been possible without the comments and cooperation of all of these persons—those who prepared papers, those who attended the conferences, and those who contributed to the world survey.

For a number of reasons, I hope that this book will reach many audiences—educators and students, planners and analysts, managers and policymakers, and the general public, or at least that portion of these audiences which shares William Faulkner's belief that man should not merely endure, but prevail. These pages came together at a time when it had long been fashionable in the West to attack science, to debunk planning, to excoriate bureaucracy, to lament tomorrow, and to applaud those who argue that sheer being is the antidote to the pains of becoming, that mysticism is the cure for the excesses of materialism, and that the higher morality is to be found in a warm and strangely interpersonal solipsism. One may or may not agree that these attitudes are in the end nothing more than a prescription for mere endurance. But it is difficult to maintain that they can in any way help mankind to prevail. In that task, where challenge and change are real, and where the process of understanding the one and controlling the other is laborious, it is impossible to ignore the future or to dismiss out of hand the only instruments that can bring the future to the service of man. On the contrary, these instruments, where useful, should be improved. No one quite knows how far this work can go, but whatever the limits—theoretical or moral—it is clear that many persons from many backgrounds will have to be involved. This book offers them the beginnings of a framework for their efforts.

Because most authors have avoided mathematics, the discussion should be accessible to everyone in the intended audiences. But a certain familiarity with forecasting is assumed by several of the contributors. Some readers may, therefore, find it advantageous to read this book in conjunction with one of the basic works in the field. Perhaps none surpasses Bertrand de Jouvenal's *Art of Conjecture* in the consideration of fundamental ideas and Erich Jantsch's *Technological Forecasting in Perspective* in the presentation of basic techniques, but references to many other useful sources, popular and technical, are provided in the bibliography.

I would like to acknowledge the help I received during this project from several colleagues at The Futures Group, particularly Theodore J. Gordon, who first proposed that such a study be undertaken and who contributed substantially at many points, and Janice Cohen, who assisted me in distilling out the various candidate research proposals (analyzed in Chapter 18). Special thanks are also owed to June Salisbury, who not only superbly typed a very difficult manuscript, but who also managed dozens of names, hundreds of publications, and thousands of other details professionally and with good cheer.

Claire Connelly, a consultant to the project, did the copy editing speedily and well.

Finally, I would also like to thank Robert Lamson of the U.S. National Science Foundation, who served (with extraordinary patience and cooperation) as administrator of the contract which supported much of the work reported here.

As editor, I would dedicate this volume to Edward S. Quade, who set the stage; to Theodore J. Gordon, who had the idea; to the contributors, who made it happen; and to Michele and Robert, who, among others, will judge its worth.

Wayne I. Boucher

October 1976

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Part II. Accomplishments and Prospects

Part III. Professional Issues

PART I
AN ORIENTATION TO FUTURES RESEARCH

Chapter 1

Introduction

Wayne I. Boucher

Astrologers, mystics, chiromancers, prophets, psychics, haruspices, and other kinds of soothsayers and seers, including priests and princes, have traditionally had the corner on forecasting. Even today these worthies have by far the greatest influence of all those who deal with the future, affecting (as they do) the expectations and actions of hundreds of millions of persons daily. This situation is not likely to change in the foreseeable future, but the twentieth century has witnessed the first concerted and sustained efforts to make it change. Indeed, these efforts—directed in part toward making forecasting less of an art and more of a science—have not only begun but have in fact become more and more widespread, organized, and sophisticated, particularly since the end of World War II, when, in Jacob Bronowski's words, the atomic bomb brought civilization "face to face with its own implications."

The conviction is now increasingly shared that it is no longer merely profitable or helpful to explore as carefully as possible the course of trends and the likelihood of potential events. It is necessary. The explanation—as this magnificent and yet terrible century has demonstrated more than once—is that without keener foresight, without a more responsible anticipation, some highly desirable possibilities will certainly be missed and some current decisions will have regrettable, perhaps disastrous, consequences in a tomorrow that could materialize so quickly that man would be powerless to do anything to protect himself from them.

Looking at the future has thus taken on a very serious side. And as a vocation or avocation it has also become something of a growth industry throughout the world, because accompanying this change in attitude toward the future, and to some extent causing it, has been the invention or rediscovery of new analytic capabilities, the creation of new institutional forms, and the development of new outlets for expression, as well as a steady growth in possible clients and actual funds. These are all dedicated in one way or another to gaining greater insight into the real opportunities for choice and into the implications of the use of these opportunities.

The Futures Movement in General

No one knows just how extensive this activity is, whether in number or type of institutions involved, number or background of practitioners, levels or sources of funding, number or kind of studies, or whatever. But various national and international surveys make the total number of individuals and groups strikingly large—so large, in fact, that it is possible to speak without exaggeration of a worldwide futures movement. This movement may be characterized roughly as embracing all of those persons, whatever their affiliation, engaged in forecasting or prediction with a view toward taming the future—leashing it where necessary and domesticating it where possible. They thus deny the implicit premises of the soothsayers that the future is totally outside of man's control and that, at best, it can merely be foretold as a sequence of necessary truths. And while their approaches to foreseeing or inventing the future vary across a spectrum that includes narrow economic or technological forecasting, the writing of science fiction, the establishing of communes, and rioting in the streets, they are as one in behaving as if they thought that the achievement of at least some desirable alternatives rests entirely upon conscious action, that action rests on decisions, that decisions rest on forecasts, and that forecasts rest on reasonable excursions into the plethora of imaginable tomorrows.

The growth, size, and composition of the futures movement is indicated, albeit only approximately, by the surveys mentioned before. For example, Erich Jantsch, in his pioneering study in 1966 which took him into the United States, Western Europe, and Israel, found about 100 organizations with a permanent forecasting activity, of which more than 60 percent, not unexpectedly, were in industry, with the rest distributed among government, academia, and independent research centers such as The Rand Corporation.[1] Extrapolating from these 100 cases, he estimated that just in the United States some 500-600 corporations had an in-house capability; additionally, he concluded that about 500 corporations (many of them included in the preceding group), plus an uncounted number of government agencies and foundations, drew upon the forecasting services provided by outside private or nonprofit firms.

Between 1968 and 1970, the Long-Term Planning and Policy Division of the Council of Europe surveyed forecasting in ten countries of Western Europe.[2] Of the 292 organizations which it identified, 101 were governmental, 66 were in universities, 12 were nongovernmental international organizations (e.g., Europe 2000), and the remaining 113 were private or independent nonprofit groups.

A global survey was undertaken in the spring of 1970 by De Houghton and others in England.[3] They identified and sent a questionnaire to 490 individuals and organizations (including ad hoc groups) that they considered to be part of the futures movement. Responses were received from 269 sources in academia, government, industry, foundations, and elsewhere. Another measure of international activity can be found in the annual surveys conducted for the past few years by the IRADES group in Rome. For example, the 1972 directory lists about 200 organizations, principally nongovernmental, that specifically claim to specialize in forecasting. In total, however, some 700-800 persons and organizations are listed—thus including many of the friends and acquaintances of the futures movement.[4]

John McHale of the University of Houston has conducted two important surveys of forecasting in the United States, the first in 1969-1970 and the second in 1971-1972.[5] The growth in the number of practitioners between the two studies is suggested by the data in the table below. In each study, government on all levels

PARTICIPANTS	NUMBER 1969-1970 Survey	1970-1971 Survey
Invited (institutions and individuals)	356	527
Responses	139	204

accounted for only about 5 percent of the responses. Most of the responses came from individuals (who may or may not have been affiliated with an organization concerned with forecasting), but after them industry was most important (with over 20 percent of the responses), followed by the universities (which went from 10 to 15 percent between the two surveys) and the nonprofits (with some 10 percent), and then, in much smaller percentages, associations and religious groups.

From time to time, W. W. Simmons of Applied Futures, Inc., in Connecticut, surveys organizations as well as ad hoc groups and publications throughout the free world that appear to be involved in what Simmons calls "exploratory planning," which he defines as the search for alternative futures in the period about 15 years from the present. These reviews are informal and lay no claim to being comprehensive, but the results are interesting in that they tend to confirm what other studies show about the growth of interest in studying the future and the distribution of the work among various sectors. The results from the first three reports are shown in the table below.[6]

SECTOR	NUMBER OF RESPONSES November 1972 Survey	October 1973 Survey	1974-1975 Survey
Corporations and business associations	25	44	55
Government agencies (U.S. and foreign)	16	34	59
Independent centers (private, nonprofit, academic, etc.)	44	79	99
TOTALS	85	157	213

Finally, there are the results from a survey made by The Futures Group in August-November 1973 during the course of research for this book. The aim of the survey was to determine the kinds of research projects currently being conducted around the world having to do just with the advancement of forecasting itself.[7] Because it was important to the objectives of the research project as a whole to contact as many organizations as could be found, the studies mentioned above, as well as a number of other sources, were reviewed carefully to extract names and addresses. Altogether, 937 institutions in 42 countries were identified, of which perhaps 850-900 appeared to have completed at least one substantive futures-oriented project within the last few years, assuming a very generous definition of what constitutes a futures project.

Of the 937 organizations invited to complete The Futures Group's questionnaire, 532 were based in the United States, 90 were from elsewhere in the Americas (mainly Canada), 224 were in Western Europe, 36 were in the USSR and Eastern Europe, and 55 were from other areas (primarily the Far East). About 15 percent of the invitees responded—a reasonable figure, considering the question that was asked—and for these respondents the percentage distribution among sectors was as follows:

Individual	5.4%
Industry	23.6
Government	8.1
Independent Research Center (private or nonprofit)	23.6
Association	9.5
University	29.8

In short, while the numbers vary widely from survey to survey, a few inferences can be made from these data. First, no less than 100 organizations in the United States, plus probably an equal or greater number in the rest of the world, are willing to associate themselves with the futures movement, and most of these organizations are actually practicing forecasting, at least part of the time. Second, the number of persons involved is no doubt many times greater than the estimate of organizations. Third, the number of individuals and organizations in the futures movement has grown rapidly and may still be growing.[8] Finally, most work is being performed in organizations, and of the organizations, about 50-80 percent worldwide are nongovernmental, that is, they are industrial groups, academic centers, private or nonprofit consulting firms, voluntary groups, and so on.

Many other signs provide additional evidence of the sudden burgeoning of a serious futures movement. Most dramatic perhaps is the post-World War II outpouring of books, articles, reports, newspaper stories, newsletters and other publications concerned with the future, either as an object of possible study (e.g., essays on the philosophy or practice of forecasting) or as a repository of potential developments warranting actual study (e.g., the future of the family, of ocean shipping, or of the Common Market). The 1971 IRADES directory, for example, contained a bibliography of 3,000 items; another effort in the same period led to the cataloging of more than 20,000 titles. Neither count, however, is even remotely indicative of the actual publication record since 1945.[9] But they do point in the right direction, and they also highlight the fact that "the futures literature" has begun to be recognized as a separate category. This is signaled as well by the appearance and increase over recent years of journals and newsletters specializing in articles on or about the future.[10]

Beyond publications, there are other signs. The upsurge in meetings and conferences explicitly devoted to the future could be mentioned, and if there were time series data on these affairs (which there are not), they would probably show at least a 10- or 20-fold increase over the past 30 years. Allowing for every kind of subject matter, from the future of printed circuits to the future of mankind, from the methodology of forecasting recidivism to the methods of modeling global dynamics, it is plausible to imagine that, on an annual basis, an average of two or three such meetings are held every day somewhere in the world and that this number is increasing.

Even more noteworthy is the fact that members of the futures movement have increasingly begun to train (and thus multiply) their successors. This is happening in nonacademic settings (such as the Industrial Management Center headed by James Bright)[11] and in the established forecasting organizations (which provide on-the-job training) and also in the educational system, on all levels. On the college and university level in the United States and Canada, where the pattern has been investigated, the results may well be representative of changes in other countries. In 1968, only about a dozen schools were offering courses for credit on the study of the future; in 1969, there were about 30; in 1970, there were 60; by 1972, the number had doubled or tripled again. Similarly, from 1970 to 1972, the number of courses offered at these schools increased from about 40 to nearly 200 (and perhaps as many as 400, if the data were extrapolated to the entire population).[12] Most disciplines appear to have their futurist now; as Eldredge puts it, "Practically every imaginable subject has been futurized, every problem stretched into the future in this quite amazing hodgepodge of courses."[13]

As suggested earlier, and as even the raw numbers presented so far would seem to indicate, "hodgepodge" may not be a bad description of the totality of aims, activities, and accomplishments within the futures movement itself, despite apparent agreement on one or two basic principles. Fortunately, it is not the purpose of this book to critique or to attempt to impose a rational structure on the entire movement, the very fecundity of which seems in fact to depend primarily upon its uniquely ebullient and probably unanalyzable diversity. Rather, the main concern in this volume is with a single small segment of the futures movement: the work of those persons and organizations engaged in what has come to be called futures research.

A Definition of Futures Research

Though the term "futures research" has achieved general currency,[14] it still is without an accepted definition. A few years ago, John McHale made an effort to discover what those who said they were practicing futures research meant by the phrase. Regrettably, the differences between the answers he received were so great that a synthesis was not possible.[15] Outcomes like this have led a number of observers to conclude that the issue is "not very important anyway" and that it would be far better to "get on with it"—whatever "it" is—the argument being analogous to the one expressed in the saw that "geometry is what geometricians do." Obviously, a person can be a successful futures researcher (or geometrician) without being able to define his field, and yet, just as obviously, it would be helpful to have a definition, particularly for the use of students, potential participants, and clients outside the field who are uncertain as to how futures research relates to other kinds of forecasting or who might wish to consider what it means to "do" it.[16]

The contributors to this volume who would call themselves futures researchers and who actually attempt to define the term in these pages are no more in agreement with each other than are similar authors elsewhere—or so it seems, if the words they use are taken literally. Nevertheless, there is a possibility, with a bit of history to support it, that the differences may be superficial and that a definition can be framed to which they and most other practitioners of futures research would assent. This possibility, which arises precisely through examination of contemporary papers and reports by futures researchers (that is, through study of what they "do"), lies in viewing futures research as a branch of systems analysis.

Of course, systems analysis itself is not easily defined, but E. S. Quade of The Rand Corporation, where it all started, has articulated one of the better definitions—one which is adequate for our purposes here. After reviewing (and dismissing) several narrow formulations, which amount to equating systems analysis to a particular method, a fixed set of methods, or a kind of report, Quade concludes:

> We would suggest that, properly speaking, [systems analysis] is a research strategy, a perspective on the proper use of the available tools, a practical philosophy of how best to aid a decisionmaker with complex problems of choice under uncertainty. In the absence of a good brief definition, systems analysis . . . can be characterized as *a systematic approach to helping a decisionmaker choose a course of action by investigating his full problem, searching out objectives and alternatives, and comparing them in light of their consequences, using an appropriate framework—insofar as possible analytic—to bring expert judgment and intuition to bear on the problem.*[17]

The 49 words in italics were written with great care; they compress much, and they clearly demand a close reading. But there is no mistaking five of the key ideas in this definition. First, systems analysis is emphatically not a particular activity; it is an "approach" to a particular activity. As such, it is not a task that is either done or left undone, but rather a "research strategy" that is either adopted or not adopted.[18] Second, systems analysis is policy-oriented: the analyst's principal role is to provide various kinds of information, all intended to help a manager ("decisionmaker") to act.[19] Third, systems analysis is problem-oriented: the only occasion for analysis is the recognition—perhaps tentative—of an existing or potential situation that may require action. Fourth, while systems analysis attempts to be as rigorous as possible in methodology, it is pragmatically oriented: analysis is impossible without the conscious use of models of one kind or another, but the models chosen should be "appropriate" to the problem and should take account of "judgment and intuition." Fifth, systems analysis is futures-oriented: as remarked earlier, decisions necessarily imply forecasts; additionally, to investigate the "full problem" is to see it and a range of alternative solutions in their substantive richness and complexity *through time.*

Now, if "futures research" is substituted for "systems analysis" in these five points, or in Quade's definition as a whole, we seem to have a good general statement of the major attributes mentioned by most futures researchers themselves in defining their occupation.[20] Futures research, they have said, is a new way of looking at the future (a new "approach"); it is a practical enterprise that focuses on effecting change (the invention and assessment of actionable policies). It cannot and does not tackle the future in its infinite possibilities, but rather suboptimizes to the level of issues that can be usefully pursued (the study of problems). Its techniques are varied, and the choice of one over another is determined in large part by the nature of the problem (pragmatism in methodology). And in forecasting and evaluating a spectrum of alternatives relevant to the problem, it is especially concerned to carry this analysis out to the end of the future period of interest (it is futures-oriented). If one grants these propositions, the question is no longer how futures research differs from other kinds of activities within the futures movement, but how it differs from systems analysis.

A wholly satisfactory answer is probably not possible, if only because it often happens that futures researchers find themselves working on the same problems, or at least in the same areas, that are being studied by systems analysts who would not consider themselves to be futures researchers. Thus the differences that exist between the two may be said to exist more in degree or relative emphasis than in kind.

As a branch of systems analysis, then, what sets futures research apart is first of all its special emphasis on making explicit the subjective impressions and "models" of reality that are shared by particular groups within society or within particular organizations and that underlie (and, in fact, often determine) the choices these groups make. If the future that ultimately materializes is likely to be shaped by a group process that involves a confused recollection of history or an unquestioned belief about the permanence of a certain way of doing things or a bizarre understanding of man or a dubious set of expectations about future change, it is exceedingly important to know that. ("If we believe absurdities," said Radhakrishnan, "we shall commit atrocities.") Getting at these opinions and subjective models insofar as specific problems are concerned offers some promise of seeing more clearly the meaning of proposed actions. It also can provide a firmer basis for anticipating what the consequences of particular actions might be before they are taken. At the very least, eliciting this information can make it possible to understand better the possible evolution of the world of greatest interest to the group and thus to minimize, to some extent, the uncertainties that will inevitably attend major choices or developments.

Futures research not only conducts investigations in what might be called policy-oriented social psychology, but also attempts to gather and organize other equally intangible kinds of information pertinent to decisions: the nature of potential value changes, of new patterns in the interaction of people and institutions, and of the beliefs that may be held by those who will inherit the effects of *our* choices. A second defining feature of futures research, therefore, is that it tries to describe and forecast these matters, sometimes by synthesizing information already available in the literature, sometimes by developing and testing a model, sometimes by conducting a poll or other judgmental kind of study involving a group, and sometimes by a personal conjecture on the part of the analyst. By itself, however, this information is of little value. The futures researcher makes it useful in the policy context when he interrelates it with other kinds of information—concerning, say, prospective developments forecasted by economists or technologists—for the practical purpose of synthesizing a reasonably comprehensive picture of what the world, or some aspect of it, might be like in the future if things relevant to the problem in hand continue as in the past.[21] The result is a "standard" or "surprise-free" world, against which, again, policies can be tested.[22]

The very essence of futures research, however, lies in emphasis on the use of its social-psychological data, forecasts, and special syntheses of forecasts to open the realm of choice by generating *alternative* futures. Such alternative futures are, in effect, variations on the standard world, and their use—as a policy-generation and assessment tool—is precisely the same. Writing in 1967, Andrew Kopkind had some very sharp criticisms of the "future-planners," as he called them.[23] His essay is still well worth reading, especially by futures researchers, but he erred when he attacked the creation of alternative futures as being, of all the activities of futures research, "the most fun, but also the least responsible." He was wrong in thinking it fun, and he was wrong in thinking it irresponsible, at least in principle. (Surely it is irresponsible if it promises too much, as has often happened. And surely it is irresponsible if the results are known to be inadequate to the particular need and the user is not warned, as has also happened more than once.) The point in principle hinges on the fact that the standard world is worth studying precisely because *it seems so probable* and because it is worth asking if this world, in all of its major features, is really the one we want. But this question can be posed and answered responsibly only with reference to specific, well-defined alternatives that are to an extent within man's power, within the time frame being considered.

Of course, since some aspects of all imaginable futures are now within our power, this effort could be endless. Nevertheless, some of the possibilities, particularly those that might evolve within or around our current institutional framework, are unquestionably important candidates for analysis before committing ourselves, out of mere inertia, to the standard world. Hence, at its heart, futures research can be said to place greatest stress on developing and applying methods for sorting out the possibilities deserving close examination and helping in the work of examining them, especially through the integration of judgmental and analytic data.

On this characterization, futures research adds a distinctive and important perspective to systems analysis. But futures research does not replace other kinds of forecasting, whether performed inside or outside of systems analysis. Rather, it builds upon the results they can provide, it extends these results systematically into the realm of judgment, and it seeks to synthesize the whole into self-consistent and policy-relevant images, highlighting possible issues, opportunities, and policy alternatives that can make a difference vis-a-vis the achievement of alternative goals.

The fragment of history that adds plausibility to the idea of subsuming futures research under systems analysis has to do with the early evolution of systems analysis.[24] As this approach began to define itself in actual experience, it was recognized that one of the most significant features of this new enterprise was its attempt to cope with what Quade, in the definition quoted earlier, calls the "full" problem. Unlike alternative approaches to problems involving decisions under uncertainty, which typically take the problems, as stated, and set about solving them, systems analysis insisted on another tack. To see problems in their entirety, in all of their dimensions, came to mean—among other things—that the analysts must be free to ask whether they are, in fact, problems or the "right" problems. This meant, in turn, that the scope of any important inquiry had to be substantially broadened. In such cases, however, since no one person could possibly have all of the information required to ensure that the investigation was indeed comprehensive, systems analysis demanded a multidisciplinary outlook, one which involved assembling teams of specialists from fields as diverse as engineering, economics, political science, mathematics, and philosophy.

The actual effort to bring different viewpoints to bear on the same complex problem and to do so in such a way that key assumptions were made as explicitly as possible, soon convinced most systems analysts that at some point standard analytic techniques must fail and that judgment—the mysterious process whereby values, attitudes, and beliefs are made to guide our expectations and preferences—must come into play, just as it must come into play in most of the established social and physical sciences.[25] Indeed, they realized that judgment had to be exercised from the very beginning, in the identification of factors, relationships, and goals that should or could be considered. As it turned out, this obvious perception about the centrality of judgment in the analysis of complex systems was to be of greater moment than anyone could have guessed then. The reason is that some analysts were dissatisfied with the conventional way of dealing with subjectivity, which at best was merely to admit its presence, to warn about its possible influence, and to recommend to the decisionmaker—the user of the systems analysis—that he somehow take it into account when and to the extent he thought appropriate. These analysts believed that there had to be better means to derive and incorporate judgment formally in the analysis and to communicate its influence to the policymaker. This hope led them to experiment with novel methods of eliciting, evaluating, and integrating human opinion in systems studies.

What happened as a result is not just that such techniques were devised—after all, as Dr. Johnson told us, nothing is closed to diligence and hard work—but rather that these techniques (scenario writing is an example) immediately proved useful in an application which was only dimly foreseen: forecasting itself. The examination of current and past relationships or values or situations is *likely* to be impossible without the use of judgment; the exploration and synthesis of future states of affairs, especially as one goes farther and farther out in time, is *certainly* impossible without judgment. These techniques provided a way. It is hardly surprising, therefore, that the most famous of them all was instantly dubbed the Delphi technique, after the Greek oracle.

When the history of futures research is written, its origins will almost certainly be traced to these innovations in methodology—not so much because of their intrinsic merits or even because of the truly remarkable interest they generated throughout the world as they became known, but primarily because these innovations now form the very core of the set of techniques that characterize the practice of futures research. Thus, in searching for means to grapple with "full" problems, systems analysis inadvertently but decisively transformed forecasting, making it possible (and obligatory, in many cases) to use these new opinion-gathering techniques to clarify, augment, and extend projections prepared through customary methods, such as mathematical trend extrapolation, and to obtain certain otherwise unobtainable kinds of information about the future. Thereby, it gave birth to futures research.[26]

As a distinct approach, futures research has been in the background of systems analytic work for about 25 years. It seems to have first come to the fore with the publication in September 1964 of the classic study by Gordon and Helmer on the Delphi technique.[27] Four years later, to the month, the Institute for the Future—the first organization in the United States, and one of the first in the world, formed to undertake futures research full time—came into existence. Since then, the number of organizations, advocates, practitioners, and publications has multiplied at a rate approaching that of the futures movement as a whole.

Purposes of This Book

Enough time has passed and enough research has now been performed to make it worthwhile to examine critically the claims and accomplishments of futures research, with a view toward assessing where the activity stands today, where it might be heading, and where further progress may be slowed or stopped entirely because of theoretical or practical difficulties. For a variety of reasons, it is particularly important to ask whether futures research has proven or can prove capable of "making forecasting less of an art and more of a science." Behind this phrase lies the possibility of recognizing futures research as a discipline—a possibility that has been mentioned with more frequency in recent years. Much more important, however, is the further possibility of realizing a vision first articulated in this century by H. G. Wells:

> I must confess that I believe quite firmly that an inductive knowledge of a great number of things in the future is becoming a human possibility. I believe that the time is drawing near when it will be possible to suggest a systematic exploration of the future. And you must not judge the practicability of this enterprise by the failures of the past. So far nothing has been attempted, so far no first-class mind has ever focused itself upon these issues. But suppose the laws of social and political development, for example, were given as many brains, were given as much attention, criticism and discussion as we have given to the laws of chemical combination during the last 50 years—what might we not expect?[28]

Wells urged his readers to keep in mind the example provided by the science of geology:

> . . . if it has been possible for men by picking out a number of suggestive and significant looking things in the present, by comparing them, criticizing them, and discussing them, with a perpetual insistence upon *why?* without any guiding tradition, and indeed in the teeth of established beliefs, to construct this amazing searchlight of inference into the remoter past—is it really, after all, such an extravagant and hopeless thing to suggest that, by seeking for operating causes instead of for fossils and by criticizing them as persistently and thoroughly as the geological record has been criticized, it may be possible to throw a searchlight of inference forward instead of backward and to attain to a knowledge of coming things as clear, as universally convincing and infinitely more important to mankind than the clear vision of the past that geology has opened to us during the nineteenth century?

His conclusion was as follows:

> I . . . suggest to you that, along certain lines and with certain qualifications and limitations, a working knowledge of things in the future is a possible and practicable thing.

Despite the historically important work of individual scholars like Pitirim Sorokin, William Ogburn, and S. Colum Gilfillan,[29] the proposal implicit in Wells's conclusion was hardly noticed again until midcentury. Writing in 1946, S. Lilley was thus able to point to some of the barriers that still had to be overcome before this "science" could be achieved:

> There has been as yet no important attempt to undertake in detail the fundamental analysis that would be required to found a science of prediction. The various attempts, though often made by men expert in the scientific method, have not yet been carried out on an essentially scientific basis. The best that can be said is that they are the products of acute intuition, modified by a rational, but far from systematic, survey of the available evidence. Again, there has been nothing in the nature of a "school of prediction"—no bringing together of a suitably balanced team of scientists, technologists, historians and sociologists to give those several years of work that are essential to the foundation of any science. Nor has there been any notable development of that other and looser form of co-operation that is frequently so fruitful, in which several workers in a field publish results, criticize and comment on the work of others, and by their mutual interaction advance the science they are interested in.

> [Thus,] the question is: could the teamwork of many, devoted to a really scientific analysis of the causation involved in technological progress, give the sort of result that is required? Could it make possible predictions of 95 or even 99 percent accuracy on which it would be possible to take social action?[30]

All these conditions, and others, have long been satisfied, at least in appearance, but Lilley's questions remain. Moreover, a good many additional questions have been raised and seek answers.

To provide these answers, it is first necessary to examine the present state-of-the-art of futures research, not only in methodology, but in every other dimension, as suggested by the outline in Table 1. Some authorities who have already attempted such a review have been dismayed by what they found. Dror, for example, has concluded that the state-of-the-art is, to put it politely, "undeveloped"; worse yet, endless repetition of accepted ideas has taken the place of real innovation:

> Careful perusal of futures studies literature and, even more so, of the proceedings of the four main international conferences,[31] leads me regretfully but unavoidably to the conclusion that the rate of innovation in futures studies, both in methodology and in substantive ideas, is on a marginally decreasing curve. Read three main books in methodology, read a dozen books of substantive ideas on the future, and look over the proceedings of the Kyoto Conference—and you have got it all! . . . Clearly, the urgent need is for innovative work, new ideas, better methodologies, new designs, rather than for sharing and resharing what has already been shared several times over.[32]

In effect, it is one purpose of the chapters in this book not only to repeat Dror's analysis, but also to widen it so that topics like those in the outline are at least touched upon.

Against this background, it is then necessary to explore the crucial issue of whether the strengths that are found can be enhanced and the limitations or weaknesses can be overcome. Has futures research, in whole or in part, gone as far as it can go? A second purpose of this book is to try to formulate answers to this question, to distinguish wish from reality, and to discern blind spots in the perceptions of futures researchers (or the users of futures research) about this vocation. Symbolic of the significance of this question is the publication by The Rand Corporation of an analysis of the Delphi technique which concludes that "conventional Delphi is basically an unreliable and scientifically invalidated technique in principle and probably in practice"—this, just 10 years after the publication of the original Gordon-Helmer report which was so influential in establishing futures research. But the author goes well beyond Delphi:

> Consumers of information on the future need far better advice and protection from contributing professionals than they have gotten to date. The future is far too important for the human species to be left to fortune tellers using new versions of old crystal balls. It is time for the oracles to move out and for science to move in.[33]

The language is harsh and the logic is flawed, but the thrust is clear. Similar ideas have been expressed elsewhere. Of the contributors to this volume, some agree, most disagree, but all cast further light on the issues involved.

Finally and most importantly, it is necessary to inquire how the changes in futures research that are in fact deemed possible and worthwhile might best be accomplished. Some of them, of course, simply could be allowed to occur; specific conscious action may not be necessary or desirable. For the rest, however, direct action of one kind or another would be required: conferences, new publications, wider dissemination of existing information, the creation of new institutions, original research, and so on. The major concern of the chapters in this book is the alternative of original research. What research projects are most needed? What problems can they overcome, or help to overcome, if these projects were completed successfully? What opportunities can be enhanced? To what extent would futures research be affected if these studies were performed?

It has been argued by Theodore J. Gordon, and many would agree, that "futures research is an embryonic discipline, a collection of techniques and estimates, with [only] a partially unifying philosophy. [As such,] its research agenda is immense."[34] This book arose principally from an interest in testing this argument.[35] And, as will be seen, the contributors overwhelmingly concur. Hundreds of research projects are suggested in the pages that follow; of these, a remarkably large number are directed at the most fundamental issues facing futures research. Taken together, they define at least the beginnings of an agenda that obviously warrants prompt, thoughtful,

TABLE 1

A POSSIBLE FRAMEWORK FOR EVALUATING THE STATE-OF-THE-ART IN FUTURES RESEARCH
(Six Working Categories of Interest, with Representative Types of Problems)

I. PHILOSOPHICAL AND CONCEPTUAL ISSUES
- A Possibility of "knowing" the future
- B The nature of time
- C Defining "rate of change"
- D The nature of probability
- E Futures research as a science
- F Criteria (accuracy, rationality, reproducibility, etc.)

II. SUBSTANTIVE ISSUES
- A. Subjects chosen for forecasting in the past
  1. Areas of most emphasis and the significance thereof
  2. Areas of least emphasis and the significance thereof
  3. Stated criteria for selection
  4. Possible unstated criteria (e g , ideological differences, attitudes in particular sectors, such as academia, government, and industry)
- B Reasons for success and failure
  1. In social forecasting
  2. In technological forecasting
  3. In forecasting value changes

III. METHODOLOGICAL ISSUES
- A. Possible improvements in types of methods
  1. Judgmental techniques (Delphi, games and simulations, polls and surveys, scenario writing, etc)
  2. Quasi-judgmental and mathematical techniques (trend correlation and analysis, computer models, matrix analysis, etc.)
- B. Possible improvements in monitoring systems
- C Problems of synthesizing forecasts from different domains

IV. PROFESSIONAL ISSUES
- A. Education and training in futures research
- B. Problems involving standards
  1. Ethical standards in the practice of futures research
  2. Publication standards (technical and popular)
- C Relationship to, and lessons on methodology or application to be learned from, similar activities
  1. Established disciplines (economics, sociology, anthropology, psychology, political science, biology, etc.)
  2. Multidisciplinary approaches (systems analysis, operations research, management science, policy science, risk analysis, research and development planning, corporate planning, technology assessment, social indicators, value analysis, etc.)
  3. Other areas (the arts, parapsychology, history, etc.)
- D. Institutionalization of futures research
  1. Academic centers
  2. Nonacademic centers (industry, government, etc.)
  3. Professional organizations
  4. International centers

V. ISSUES OF COOPERATION
- A. Influences on cooperation and collaboration
  1. Institutional form
  2. Sources of support
  3. Political factors, domestic or international
  4. Ideological factors
- B Problems of communicating research results
  1. Time delays
  2. Publication costs
  3. Language barriers

VI ISSUES OF IMPLEMENTATION AND USE
- A. Problems involving use of the literature
  1. Dissemination systems
  2. Storage and retrieval systems
  3. Handling of proprietary, classified, or otherwise sensitive materials
  4. Problems of minimizing duplication of work
- B. Use of and abuse of forecasts
  1. In monitoring systems
  2. In planning systems
  3. In actual decisionmaking
- C. Means of improving the use of forecasts

cooperative, and sustained attention by practitioners of futures research and by other individuals and organizations that would like to extend futures research to that uttermost limit where it can fairly be said that one has confirmed or denied the possibility that "a working knowledge of . . . the future is a possible and practicable thing."

FOOTNOTES

[1] Erich Jantsch, *Technological Forecasting in Perspective* (Paris: Organisation for Economic Co-Operation and Development, 1967). Despite its early date, this volume remains one of the few indispensible books on the philosophy, techniques, and uses of forecasting.

[2] "European Inventory of Long-Term Forecasting Activities." *Futures* Vol. 2, No. 4 (December 1970), p. 387.

[3] Charles De Hoghton, William Page, and Guy Streatfeild, *. . . And Now the Future: A PEP Survey of Future Studies*, Vol. 37, Broadsheet 529 (London, PEP, August 1971).

[4] *Social Forecasting Ideas, Men, Organizations, Activities* (Rome IRADES 1972) Incidentally, the directories published by IRADES are unquestionably among the best sources of information on the movement, even though the user must endure all of the problems that one would expect when editors whose native language is Italian receive questionnaire responses in many different languages and then issue them in English without benefit of professional translation.

[5] John McHale, *Typological Survey of Futures Research in the U S* (Binghamton, N Y Center for Integrative Studies, School of Advanced Technology, State University of New York, 1970), and *A Continuation of the Typological Survey of Futures Research, U.S.* (Binghamton, N Y Center for Integrative Studies, School of Advanced Technology, State University of New York, 1972). As illustrated in Chapter 16, these reports contain a good deal of additional detail of the movement, such as the academic training of the participants, areas of professional interest, size of their organizations, methods used, and so on. More recently, the results from an international questionnaire survey were published; see John McHale and Magda Cordell McHale, *Future Studies: An International Survey* (New York: United Nations Institute for Training and Research, 1975) In this study, 3,000 questionnaires were mailed and 926 returned, of which 414 were from institutions and 512 from individuals (254 of whom responded on their own behalf, while the remainder responded for their institutions).

[6] W W Simmons, *Exploratory Planning Briefs* (Greenwich, Conn W W. Simmons, November 1972 and October 1973), and W. W. Simmons, *1974-1975 Exploratory Planning Briefs* (New York: Center for Planning and Implementation of the American Management Associations, 1975).

[7] A summary of the survey results is presented in the Appendix.

[8] Allowance must be made, of course, for the fact that those who conduct the surveys are becoming better at identifying potential respondents. My own impression is that the growth in organizations has essentially stopped, except perhaps in academia, while the number of practitioners is continuing to rise, but at a much slower rate than in the 1960-1970 period.

[9] The bibliography in this book includes some 1,000 titles, which is merely a small subset of titles drawn almost exclusively from the English-language literature of just the last decade on the possibility of studying the future. Moreover, it omits the substantive work, which is easily the largest category. If a substantive futures publication can be defined as a self-contained document (book, news magazine article, or whatever), which includes at least one forecasted event that can be unambiguously confirmed after the projected date of occurrence as having occurred or not occurred (e.g., the GNP of the United States will reach $2 trillion in current dollars by 1982), I estimate that at least 100,000 such publications now appear *annually* in English alone But this is a very conservative estimate indeed, even for English-language items, because among other things, it leaves totally out of account all of those publications that project trends or that include those very interesting and sometimes very important forecasted events which are too imprecisely stated to be confirmed after the fact or which either posit desirable goals or declare intentions.

[10] It would, however, be a mistake to read too much into this development, important though it is to the establishment of forecasting as a discipline, since most popular, academic, and technical journals have always carried such articles. Proof of this observation, if proof is needed, can be found in Michael Marien's "A World Institute Guide to Futures Periodicals," *Fields within Fields*, No. 12 (Summer 1974), pp. 65-96, which describes, in addition to 18 non-English periodicals and 19 futures newsletters, the 100 English-language periodicals Marien judged to be "most relevant to the future." Not surprisingly, the list of 100 includes the three or four futures journals (*The Futurist, Futures, Technological Forecasting and Social Change*, and *Long Range Planning*); not surprisingly either, the rest of the list consists of publications that would be known to most intellectuals, but hardly be considered by them to be futures-oriented, despite an occasional article on forecasting, planning, change, or the future. This is not a criticism of Marien's "Guide," which can serve several purposes; but it is a reminder that one need only specify his criterion for identifying a "futures periodical" and such a list can fall readily into hand. (Marien's basic criterion was this: "If a periodical seemed to me to promote broad and creative thinking [about the future] that is also rigorous and responsible, and of potential benefit to mankind's interests, then it was included.") The significant point is that almost all journals and magazines have more and more frequently published articles on the future.

[11] In 1967, the Industrial Management Center offered what appears to have been the first course, outside of academia, on technological forecasting, and this activity has continued since then. The first course is documented in James R. Bright (ed.), *Technological Forecasting for Industry and Government: Methods and Applications* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968). Taken together with Jantsch's book (fn. 1), as well as the 1962 report by Ralph Charles Lenz, Jr., *Technological Forecasting*, 2nd. ed., Report ASD-TDR-62-414 (Dayton, Ohio: Wright-Patterson Air Force Base, Aeronautical Systems Division, June 1962), this volume captures very well the state-of-the-art in technological forecasting in the 1960's.

[12] These figures are from H. Wentworth Eldredge, "Education for Futurism in the United States: An On-Going Survey and Critical Analysis," *Technological Forecasting and Social Change*, Vol. 2 (1970), pp. 133-148; Billy Rojas, "Teaching the Future," *Change* (January-February 1971), pp. 17-19; and H. Wentworth Eldredge and Billy Rojas, "A Mark II Survey and Critique of Futures Research Teaching in North America," *Technological Forecasting and Social Change*, Vol. 4 (1973), pp. 387-407. A third survey, reporting much the same results for the period 1973-1974, was widely published. See, for example, H. Wentworth Eldredge, "Future Studies at the University," *Fields within Fields*, No. 14 (Winter 1975), pp. 69-80, or Eldredge, "University Education in Future Studies: A Mark III Survey," *Futures* (February 1975), pp. 15-30. It should be mentioned that these surveys go far beyond counting schools, courses, professors, students, and subjects. They provide what is perhaps the best account of current thinking within academia

about the value of studying (or teaching about) the future. Another excellent source of information is David C. Miller and Ronald L. Hunt *A Graduate-Level Survey of Futures Studies. A Curriculum Development Project* (Washington, D.C., Office of Education, U.S. Department of Health, Education, and Welfare, August 31, 1972); see, as well, Harold G. Shane, *The Educational Significance of the Future* (Bloomington, Ind.: Phi Delta Kappa, Inc., 1973).

[13] Eldredge and Rojas, "A Mark II Survey and Critique," *op. cit.*, p. 399.

[14] Over the years, a great many other names have been used: futurology, futuristics, futurism, future studies, future research, mellontology, and so on. Except as noted later in the text, these may be taken as rough synonyms for each other and for "futures research."

[15] For a sampling of the definitions, see Appendix G (pp. 69-72) of the first of the two studies cited in fn. 5. The sample is fascinating throughout. Consider, for example, just some of the genera used in the definitions: "the study of . . .", "the assessment of . . .", "the process of study, analysis and simulation of . . .", "the effort to . . .", "the study, forecasting, design, and realization of . . .", "an organized technique for . . .", "an investigation of probability that is . . .", "educated guesses about . . .", "the search for . . .", and so on. Predictably, the specific defining terms following these phrases are even more varied.

[16] It might also be useful to those introspective individuals within the field who must certainly wonder from time to time about what it is they are "doing."

[17] E. S. Quade, "Introduction," in E. S. Quade and W. I. Boucher (eds.), *Systems Analysis and Policy Planning: Applications in Defense* (New York: American Elsevier, 1968), p. 2 (Italics in the original.)

[18] That this distinction has a practical significance is illustrated by the fact that novice systems analysts, no matter how well trained academically, often find it difficult to discover where and how to begin their first independent work on a real-world problem. (The same is often true, of course, among novice futures researchers—including some who are experienced in systems analysis.) Colloquially, however, the distinction is constantly blurred, though not denied.

[19] Needless to say, the analyst and the manager can be the same person (or the same group). We can and sometimes do adopt a systems approach in generating our own policies, individually or collectively, as we seek to mollify our fate, enlarge our freedom, ensure our legacy, and "manage" our lesser affairs. But in the ordinary life of organizations and of society, the analyst and the manager are usually not the same. Indeed, it is frequently the case that policymaking is complicated or essentially blocked because the responsibility for one function or the other (or both) is too diffuse.

[20] For reasons considered at length in Chapter 12, this conclusion holds only for practitioners outside of the Communist orbit. While increasingly becoming enamored of Western-style systems analysis, the Communists define, practice, and rationalize "prognostics" in a manner that is still unique to them.

[21] In an impressively candid and thoughtful discussion, Bernard Cazes remarks that earlier studies of the future suffered "from one basic fault: their authors were *prophets* of the future; they had no notion of how society hangs together, which elements are more susceptible to change than others, how society as a system really works. Modern future research is trying to correct this deficiency." [Cazes, "Opportunities and Pitfalls of Future-Oriented Research," in G. R. Urban (ed.), *Can We Survive Our Future?* (New York: St. Martin's Press, 1971), pp. 351-367; the quotation is from p. 352].

[22] For a good presentation of the concept of a "surprise-free" world, see Herman Kahn and Anthony J. Wiener, *The Year 2000: A Framework for Speculation on the Next Thirty-Three Years* (New York: The Macmillan Company, 1967).

[23] Andrew Kopkind, "The Future Planners," *The New Republic*, Vol. 156, No. 8 (February 25, 1967), pp. 19-23.

[24] For greater detail on what follows, see in particular Bruce L. R. Smith, *The Rand Corporation: Case Study of a Nonprofit Advisory Corporation* (Cambridge, Mass.: Harvard University Press, 1966) and, in a less scholarly vein, Paul Dickson, *Think Tanks* (New York: Atheneum, 1971).

[25] On this essential point, see Olaf Helmer and Nicholas Rescher, "On the Epistemology of the Inexact Sciences," *Management Science*, Vol. 6, No. 1 (October 1959), pp. 25-52.

[26] Victor C. Ferkiss, in his admirable book, *Technological Man: The Myth and the Reality* (New York: George Braziller, Inc., 1969), offers a somewhat different lineage: "The techniques of systems analysis and the study of the future were born as twins" (p. 24).

[27] Theodore J. Gordon and Olaf Helmer, *Report on a Long-Range Forecasting Study*, Paper P-2982 (Santa Monica, Calif.: The Rand Corporation, September 1964); an abridgement of this study appears as an appendix in Helmer's *Social Technology* (New York: Basic Books, 1966). For historical perspective, it should be noted that the first work on Delphi and associated judgmental techniques began at Rand in 1948-1949.

[28] H. G. Wells, "The Discovery of the Future," *Nature*, Vol. 65, No. 1684 (February 6, 1902), pp. 326-331; the following quotations are from pp. 328-329.

[29] See Chapter 5.

[30] S. Lilley, "Can Prediction Become a Science?" in Bernard Barber and Walter Hirsch (eds.), *The Sociology of Science* (New York: The Free Press of Glencoe, 1962), p. 145. In 1949, Ossip Flechtheim formally called for the creation of "futurology," a science in the modest sense of being "not so different from some of the humanities (for instance, musicology) or from the social sciences (for instance, history or political science)." See Flechtheim, "Futorology—The New Science of Probability?" in Alvin Toffler (ed.), *The Futurists* (New York: Random House, 1972), pp. 264-276.

[31] Oslo (1967), Kyoto (1970), Washington (1970), and Bucharest (1972).

[32] Yehezkel Dror, "A Third Look at Futures Studies," *Technological Forecasting and Social Change*, Vol. 5, No. 2 (1973), p. 111.

[33] H. Sackman, *Delphi Assessment: Expert Opinion, Forecasting, and Group Process*, Report R-1283-PR (Santa Monica, Calif.: The Rand Corporation, April 1974), p. 73. The preceding quotation is from p. vi.

[34] Theodore J. Gordon, "The Current Methods of Futures Research," in Alvin Toffler, *The Futurists* (New York: Random House, 1972), p. 188.

[35] In fact, the original research agenda considered during the course of preparing this volume was the list of problems offered by Gordon in the paper cited above.

Chapter 2

A New Perspective on Forecasting Methodology

Herbert Gerjuoy

Commonly used morphologies of forecasting techniques are reflected in the outlines presented in texts on futures research such as those written by Jantsch, Bright, or Martino.[1] For the most part these morphologies are necessarily brief and are either serial descriptions of methods or are partitions of the "space" of possible forecasting methods that seem to be defined more by the existing methods themselves than by the universe of possible methods.

Although clearly it would be impossible to define this universe unambiguously and for all time, it is my hope in this chapter to set down a framework that points in this direction. I believe that essentially all methods for forecasting can be located within this framework, and the place for many not yet invented can be found. A classification scheme is presented here for forecasting methods in the hope that it will suggest new methods and will spark discussion which can lead to better organization and improved communication within the developing discipline.

The general structure on which this discussion is based is shown in Figure 1. In this scheme the methods of forecasting can be defined in terms of contrasting pairs of attributes. The presence or absence of particular attributes, singularly or in combination, uniquely defines method types. While such a list of facets can be extremely long, I focus here on what appear to be the principal pairs. Let us consider these in turn.

Intrinsic Assumption-Related Forecasting Methods

Intrinsic forecasting methods, as opposed to *extrinsic* methods, use forecasting criteria internal to the forecasting process itself. Thus, for example, they do not rely on criteria that depend on the use to which the forecasts will be put.

Assumption-related intrinsic forecasting methods, as opposed to *process-related* or *output-related* methods, are distinguished by their emphasis on the kinds of assumption that underlie the forecasting procedure. Clearly, forecasting methods may vary essentially independently with respect to their assumptions and the sort of output produced when these assumptions are embodied in a forecasting procedure. Moreover, a variety of different procedures are clearly compatible with a number of alternative assumptions or kinds of output. This is not to suggest that there are not natural affinities between particular assumptions, particular processes, and particular kinds of output. These affinities, however, reflect convenience, custom, or commonalities in outlook far more often than they reflect fundamental logical connections that compel the linkage of a particular process or kind of output with a particular set of assumptions. For example, the assumptions that underlie the method of forecasting called "judgmental" below would appear to be most compatible with assumptions that take into consideration unprecedented events (as in trend impact analysis). Nevertheless judgmental methods clearly could be used, for example, in the extrapolation of events by no means unprecedented. Furthermore, unprecedented events might be forecasted by an algorithm that depended upon other forecasts about the future (since, by definition, unprecedented events cannot be forecast from regularities in the past).

Periodic or Aperiodic

Forecasting algorithms usually assume, in effect, that the future will in some way repeat the past. One major class of algorithms generates forecasts that are the sums of simple periodic functions, such as sine functions, differing only in parameters such as their amplitudes, frequencies, and phases. For example, spectral analysis and related Fourier-component decompositions of past trend data may be used to "identify" the simple periodic functions whose sums are projected into the future. Autocorrelation and

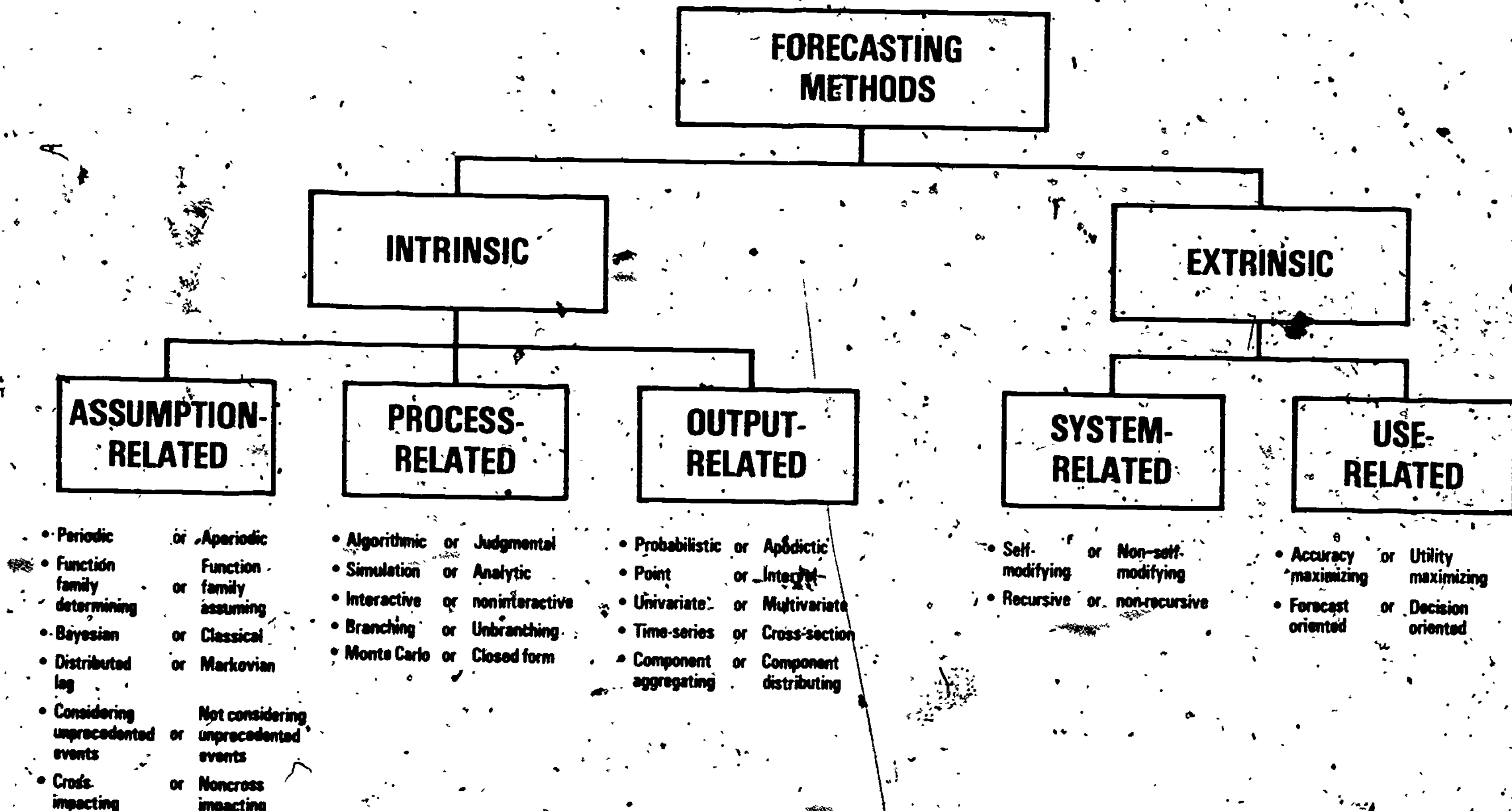


Figure 1. Structure of forecasting system attributes.

autocovariance methods also are used to identify the weights and durations of cycles of periodic components. If a sufficient number of past data components is "identified," then a perfect fit of the past data can always be accomplished and the forecast will be that the future trend repeats the past trend with a period corresponding to the time interval over which past data are available. Clearly, this is usually unreasonable, because the availability of past data is not a fundamental characteristic of the system about which forecasts are to be made.

Usually, what is done is to forecast using only a relatively small number of periodic components, the smallest number that conjointly "accounts" for the preponderance of the variability in the past data. There is a variety of goodness-of-fit statistical tests commonly used to determine which components to use and how many.

One trouble with such an approach is that selection of the set of components to be used for forecasting is made to depend on the goodness of fit of the forecasting formula to past data. Such an approach, unfortunately, neglects the possibility that a poor fit may be due to the influence of unique events in the past that uniquely perturbed the trend. A forecasting method that assumes that bumps or ripples in the past trend line must be reflected in the future by the reappearance of such bumps or by the presence of a periodic component in the future trend that echoed such a bump or ripple would erroneously predict that a unique past event would happen again and again. For example, the one-time-only achievement of escape (from the earth's gravitational field) velocity generated a sharp upward excursion in the trend curve representing the largest distance an object can be moved by human-generated force. There is no reason to believe that periodicity in the processes that led to this achievement would, for example, make achievement of escape velocity from the solar system and the galaxy predictable by periodic-function extrapolation from these data.

Another fundamental weakness of periodic-function trend extrapolation is that much trend data clearly show aperiodic secular trends, such as the trend upward in world population. Blind application of a periodic-function algorithm to such data might well generate a forecasted trend that was a sawtooth curve, falling back to base-line minimum or "starting" population at regular intervals.

Considerations such as these have led to the development of a variety of aperiodic forecasting techniques. Like the periodic techniques, they are in the main component-analysis methods that fit past trend data and then extrapolate the fitting function out into the future. The fitting function, however, is not the sum of periodic components but rather of aperiodic ones.

Like the Fourier analysis methods, such methods usually can, in principle, fit any set of past data to any desired level of approximation (i.e., they usually involve selecting a fitting function from a function family sufficiently general that a member of the family can always come as close as may be desired to any possible set of data).

Typical of such methods is polynomial curve-fitting or, more particularly, the method of orthogonal polynomial component anlysis. This method fits to the past data a series of components, each of a different pure algebraic degree—a constant, a first degree or linear term, a quadratic term, a cubic, and so on. It is well known that any continuous single-valued function can be approximated by such a polynomial to any desired level of accuracy. In general the greater the number of components used, the greater the accuracy that may be attained.

Usually preference is given to lower-degree components, and these are often "extracted" first. Thus past data may first be approximated by a constant, such as their mean. Then the "residuals" or deviations from this constant may be estimated by a trend line, the residuals from the trend line may be estimated by a parabola, and so on. With periodic-function estimation, there is no corresponding hierarchy of possible periodic components, although programs may rely on human judgments to give precedence to specified time intervals, such as day, week, month, or year. Analogously, it sometimes may be desirable to override the typical aperiodic-forecasting algorithm's preference for low-degree components. For example, suppose a trend curve could be fitted exactly by the function

$$y = x^{11}$$

Commonly used orthogonal polynomial component analysis algorithms would, instead, fit a series of lower-degree "components," so that by the time the eleventh degree was considered (if it would even be reached) its contribution to the total prediction variance would be quite small.

A fundamental weakness of polynomial curve-fitting methods is that they generate extrapolations that must go either to plus or minus infinity for the distant future (and also, retrospectively, for the distant past). Hence, if the trend is extrapolated far enough, it necessarily goes off scale. Moreover, if the fitted function is of odd degree, then the forecast about the future necessarily goes to the opposite infinity from that of the retrospective extrapolation. For example, if population growth is fitted by a cubic function then, according to the trend-fitting function, the remote future must trend toward infinite population and the remote past must have seen (absurd) infinitely negative population—or vice versa, with a positive infinity in the past and a negative infinity in the future.

On the other hand, if the function is of even degree, then the two infinities must be of the same sign. Even if the idea that in the very distant past and very distant future the trend must be to infinity is not too disturbing, it seems hard to accept the fact that choosing an odd or even degree for the equation determines whether the distant future is forecasted to be like the distant past or as different as possible. This is particularly disturbing because the degree used is often determined by a goodness-of-fit test that depends on the variability or turbulence of the past data, which hardly seems relevant to which sign infinity is approached in the limit.

This weakness of polynomial curve-fitting methods contrasts with the weakness of periodic methods, which keep resetting the forecasted trend line to the same old initial value at the start of each forecasted future cycle. Clearly what is needed is a combination of periodic and aperiodic methods, embedding them both in a more general class of estimation procedures, so that it will be possible to generate more reasonable estimates about the remote future, when this is relevant. (For short-term, as opposed to long-term, forecasting, the problem is rarely serious, because aperiodic forecasts do not have the time to go off scale and periodic ones do not get beyond a single cycle into the future.)

Finally, it must be emphasized that it is rarely reasonable to extrapolate using purely periodic or purely aperiodic techniques. There are usually both periodic and aperiodic components in a trend. Present techniques generally require some sort of precedence rule for extracting the components—either the aperiodic components are extracted first and then periodic components are extracted from some relatively undigestible residual or vice-versa. There is nothing in principle that precludes treating periodic and aperiodic components on a par with each other in competing for favor as components that "account" for trend data.

Function Family Determining or Function Family Assuming

As Karl Pearson pointed out, the choice of a particular estimation or forecasting algorithm rests ultimately on a purely judgmental selection of the function family for which the necessary parameter estimates are to be made. This judgment is clearly beyond the realm of statistics—an unfortunate fact that brings with it a certain sense of hopelessness about the rigor of the estimating process itself. Typically, the only recourse (and a very modest one) has been for the analyst to experiment with several different function families, ultimately selecting the function that gets the best score on some figure of merit, such as goodness of fit to past data.

A new approach to this problem can be envisioned which uses a new class of algorithms—function family determining algorithms. To do so, the notion of a *function space* can be introduced, a space whose elements are alternative possible functions. This familiar mathematical notion has not been used to deal with the "decision" problem, largely because there is a nondenumerable infinity of possible functions. However, this obstacle might be accommodated, as in the comparable problem of the nondenumerable infinity of possible subsets of the unit line segment dealt with in measure theory; by the construction of the equivalent of the Borel subset of this nondenumerable set.

A function will be thought of as consisting of a sequence of primitive or derived operations on operands in its domain. Each operation will be either unary or binary. The result in all cases will be single-valued. Successive operations are defined as acting on the result of all preceding operations, as if that result were enclosed in a single large parenthesis.

For example, a function may start with a domain, the variable x, which ranges from negative to positive infinity. The first operation might be the *arctan* function, a unary operator producing a value in the range $-\pi/2$ to $\pi/2$. The next operator might be the binary + operator, operating on the result of the first operation and (since it calls for two operands) the number 5.31. This would simply add 5.31 to the result of the first operation. The next operation might be the binary exponentiation operator, taking the result of the preceding step to a power specified by a second operand, which might be, for example, the original x, giving at this stage

$$y = [\arctan(x) + 5.31]^x$$

The process could start with some finite set of primitive operators, for example, the set of functions available in the standard FORTRAN library— +, −, *, **, SQRT, SIN, SINH, TANH, LOGE, etc. If complex numbers are included in the domain, then it is possible to so define the primitive functions that they are almost always defined. Alternatively, certain operations may be forbidden, e.g., arcsin(–3).

The function space now can be defined as the set of all functions that consist of a finite or denumerably infinite number of steps of defined application of the primitive operators. This space can be further reduced by considering only those functions that consist of no more than some small finite number of operations, e.g., two which would give rise to a set of functions, such as,

$(x + 5)*29.32$, SQRT($-x$), SIN[TANH(x)], etc.

The particular function to be used in a given case could be determined by applying a figure of merit to functions in a function sample drawn by random selection from the function space. Alternatively, where the space was finite, all functions in the space could be tested. Note that this approach treats numerical constants as a set of auxiliaries to the operators. Since they are infinite in number (or, in a finite computer, of very large number), it seems best that they should be drawn at random. In addition, the initial variable (x in the example), plus all preceding stages in the evolution of the function [e.g., SQRT(x)] should also be available for random or systematic selection. If the selection is random, there would be a two-stage choice: first, whether or not to choose x or a function of x as opposed to choice of a constant, such as 29.32; second, x or its function would be chosen or the constant would be chosen.

The greatest utility of this approach would be where the data are multivariate. In that case, not only values of x (the variable to be predicted) could be used, but also values of all other data variables. In any case, *time*, the time at which particular values of x are observed, would always be an available variable. Thus a possible two-operation function would always be

$$2t + x$$

where t stands for the time (e.g., in years) at which x is observed.

Where the limit on the number of operations was low, it would be possible to consider every combination. The function earning the best figure of merit in a given instance could then be selected.

At this point, the method appears to be simply a slight generalization of the procedure of having the algorithm try several different function families. It is only a slight advance to suggest that the function space may be sampled when the limit on the number of operations is not low.

Another innovation, however, is introduced at this point. The set of primitive operators may be termed the primitive *lexicon* of the system. The system may be designed to take note of each function derived from the primitive operators that earns a "best" figure of merit, and such a function may be added to the lexicon, as if it were a single-step function rather than a composite. Thus the lexicon can grow, and effectively more and more complex functions can be used, as if the limit on the number of operations had been extended.

This innovation is compatible with both complete coverage of all combinations of a small number of operators and sampling in the case where the number of operators that comprise a function is large or the number of primitive and derived operators is large itself. The probability of selecting a particular operator as the next step in construction of a function may be made to depend on the frequency of its appearance in "successful" functions—the more often an operator has been used, the more its probability of being drawn may be increased for additional use. Further, the context in which an operator appears may be taken into account. For example, the likelihood that an operator is drawn may be made to depend not only on how often it has contributed to a "successful" function, but also on whether, when it contributed, it was immediately preceded by a particular operator (i.e., two-term sequences of operators may be the unit of analysis, not just single operators). In the multivariate case, adjustment of selection probabilities could apply not only to operators but also to variables.

It is clear that this entire method could be iterated. Once applied to a set of variables to generate a set of forecasts about them, the entire procedure could be repeated, now using an expanded lexicon of operators taking into account the functions selected in the first cycle and also now using adjusted probabilities of drawing operators and variables in constructing functions. It is anticipated that this procedure would in many instances converge. Where the figure of merit for a function's selection was empirically based, e.g., depended on actual success as a *future* predictor, this would not be a mathematical question; it would itself be empirical whether or not the sequence of functions to be used to forecast the variables would converge with repeated iteration of the function-selection procedure with probabilities adjusted.

Note that this approach, in its essentials, follows the process whereby analytical procedures give rise to simulations. In effect, the program comes to "believe" in repeatedly useful operation combinations, and it comes to "regard" as "simple" operation sequences that work. Note that since the sum of the probabilities of alternatives must total 1.00, the approach effectively decreases the probabilities of use in forecasting efforts of noncontributory operations and variables.

Since with each addition of a derived operator to the lexicon (a derived operator is simply a "successful" function) the chances of fitting the data improve, a procedure is needed analogous to a test of goodness of fit that would take into account something like the "degrees of freedom" available when making the fit. Otherwise, the procedure would tend to converge to some more or less arbitrary complicated combination of derived operations that happened to fit the data perfectly.

The very Monte Carlo nature of the procedure would naturally provide such a test by providing a set of alternative fits to the same data. The obtained distribution of figures of merit for the alternative fits to the same data would provide a basis for statistical tests of the probability that a new candidate constituted a genuine, nonchance improvement in fit. There are very deep statistical and mathematical puzzles associated with development of a rigorous and highly efficient test of this kind, but it would not be too onerous to construct simple-minded ones, such as ones using Tschebycheff's inequality, that would almost certainly suffice.

Bayesian or Classical

A point of some controversy in the contemporary theory of statistical estimation has been the relative merits of Bayesian and classical estimation methodologies. This issue is somewhat less acute in connection with forecasting because forecasting is not just another estimation problem but is, rather, an extension of estimation.

Historically, Bayesian methods were developed with particular reference to retroprediction. Bayes's theorem provides a rule for calculating the probability of a particular *antecedent*, provided we know which consequences have occurred and what their probabilities would be if the particular antecedent did indeed occur. Thus, for example, we can conclude what the probability is that the butler killed Lord Heathingstoke, given that Lord Heathingstoke is dead, that he was shot in the wrist, and that he bled to death from that wound, and also given the probability that if the butler sought to kill him he would have been successful in his endeavor, the probability that he would have shot Lord Heathingstoke in the wrist, and the probability that, had he been so shot, Lord Heathingstoke would have let himself bleed to death.

In forecasting, our problem is quite the other way round. We have data about certain antecedents in hand, and we are interested in their consequences. We know, for example, that the butler intends to kill the Lord. We want to know now what his chances are of succeeding, say, tomorrow.

The possibility of using Bayesian methods in forecasting thus arises at one remove from the actual forecasting task. They are used, if at all, to estimate unknown population parameters to be used for forecasting. For example, we may wish to estimate a second-degree component of a polynomial function fitted to a past-data trend line. This will then be extrapolated out into the future in order to generate a desired forecast. Given the past data, and given the likelihoods of these data conditional on different possible values of the parameter to be estimated (the second-degree component), we may attach a probability to each of the different possible values of the parameter. Typically, we would then select the most likely.

Classical estimation methods do not ordinarily assign probabilities or likelihoods to possible values of unknown parameters. Rather, they select for their estimates those values of the unknown parameters that give rise to acceptable (often maximum) likelihoods for the given data, regarding the data as products of sampling from a population whose parameters are those to be estimated.

A possibly embarrassing weakness of both approaches is that the probabilities or likelihoods that are found to be "best" are often quite low. In the Bayesian approach, this may be because there is a wide range of possible alternative parameter values; in the classical approach, this may follow from a large number of alternative possible data samples.

In either case, the probabilities involved are best thought of as figures of merit rather than as frequency ratios. The probability or likelihood to be attached to a particular data sample depends critically on the statistical model that is used. A data sample simply is. In one sense, its only true probability is 1.00. But this comment applies equally to Bayesian and classical methods. It is brought into the discussion here to call attention to the fact that the distinction between the two methods is less profound than often appears, since both methods share important, and questionable, common assumptions.

A key consideration is that, for any given set of data, if a Bayesian approach can be used to generate any particular set of unknown parameter estimates, there must always exist a corresponding classical method that would give exactly the same estimates, and vice versa. The choice between methods generally should depend, therefore, on the convenience or naturalness of their application in a particular case; e.g., where a Bayesian approach is preferred, it should generally be because a classical model leading to the same estimate would be more complex or loaded with more *ad hoc* assumptions.

Experience has shown that Bayesian models seem to work better—i.e., generate more credible estimates—when external to the immediate data-gathering operation there is a substantial body of knowledge concerning the relationship between sample data and underlying population parameters. Although it is generally possible to make use of such information using classical methods, Bayesian methods seem to lend themselves more naturally to the exploitation of such information.

On the other hand, classical methods seem to lend themselves most readily to application in those cases where alternative conceptualizations of the relationship between data and underlying population seem equally credible. These considerations all must take second place, however, in questions of forecasting to consideration of the relationship between the past population (from which past data may be considered to have been drawn) and the future. Forecasting involves two sources of uncertainty: about the given data and about how the data relate to the future. (See "Point or Interval.")

If either Bayesian or classical methods are to be used in an effort to deal with this issue, they can only do so indirectly, with a heavy load of assumptions. For example, remote past data could be used to forecast more recent past data, and either classical or Bayesian methods could be used to estimate parameters of a forecasting function so as to maximize forecasting accuracy or utility. Either method could, in the limit, simply assume a particular function—in effect, impose it on the data. In either case a key assumption would be that the relation of the future to the immediate past parallels the relation of the immediate past to the more remote past. The meaning of "parallels" would depend on the forecasting model used, and here both methods would have considerable flexibility.

There is a subtle advantage for the classical approach in that the algorithms whereby the forecasts are related to the past are likely to be more similar to the algorithms used for estimation of past-data parameters using classical methods than to the corresponding algorithms for Bayesian methods. Thus there would be more opportunity for savings and efficiency in high-speed computation.

More fundamentally, we tend to think of the future as contingent on the past, and not the reverse. But Bayesian methods make our conception of the past depend on the data shadows the past casts forward. This is both a strength and a weakness of the Bayesian method. It is a strength because it is in fact true that our only knowledge of the past (as well as the future) comes from present data. The past is as much a hypothetical construct as the future is. It is a weakness, because it leads us to think about the past as if it were a sort of future, and this is not a comfortable way to think of the past.

In sum, then, the choice between Bayesian and classical methods seems less crucial in forecasting than in other statistical realms. It is overshadowed by issues concerned with the inaccessibility of the future about which we forecast and by the uncertainty of the relationship of the past to that inaccessible future. For the purposes of the present study, it would seem best to regard Bayesian and classical procedures as alternative approaches whose relative merits should not be prejudged.

Distributed Lag or Markovian

Distributed lag forecasting procedures allow past events in a time series to influence future events over several different time intervals. For example, the events at time t_5 may depend not only on the events at time t_4, but also on the events at times t_1, t_2, and t_3. Autocovarance and autocorrelation models are of this type. The most important development of distributed lag models has been for transfer function applications.

Markovian methods make the next step into the future depend only on the immediate present. In effect, prediction is done by a function of a single variable—the present state. However, since the present state may be a variable in a phase space of any number of dimensions, there is a formal equivalence between the class of distributed lag and Markovian models. All characteristics of the past can be considered to be implicit in the present.

Thus the fundamental distinction is between those models in which the path to the present state is irrelevant and those models in which it matters. When the path matters, a distributed lag formulation is generally preferable, even though a Markovian method may be formally possible. A Markovian formulation is preferable when there is no inertia, for instance, that tends to make a trend line continue upward if it has been rising of late.

Box and Jenkins[2] have developed a general formulation in which the distinction between the distributed lag or Markovian approach is demonstrably a formal one. In most cases the choice should probably be made on the basis of data available. For example, where human judgment would estimate more comfortably transition probabilities among alternative states of a system, a Markovian model is a natural way to extrapolate into the future. On the other hand, where judgment would more readily estimate trend lines or impacts or some other distributed lag data format, there is usually little point to converting the data into a form suitable for Markovian analysis.

A particularly interesting case arises when autoregressive methods reveal that the immediate past is less influential in determining the next step in a series than are events at some distance in the past. For example, the same month last year may be more predictive than last month this year. Such findings may strongly favor a distributed lag model.

Considering Unprecedented Events or Not Considering Unprecedented Events

By definition, unprecedented events cannot be anticipated on the basis of past data. To the extent that such events are to be forecasted, or to the extent that such events impact upon other events or trends that are to be forecasted, recourse must be to something other than past data. That something has been and will almost certainly continue to be expert human judgment. It may be argued that experts have no magical source of knowledge about such "unprecedented" events; they must depend on past data, and their seeming ability to make predictions of such events simply means that the events are not as unprecedented as they may appear to be. Therefore, one possible direction for further research directed toward improving forecasting technology would be to look into the bases used by experts to forecast seemingly unprecedented events.

One approach to such research would be to ask the judges what data they consider crucial to their forecasts. It has been observed, however, that experts are often unable to make explicit all or much of what enters into their judgments. Sometimes some light is thrown on the process by which experts make such judgments by the circumstances in which they disagree. Disagreements about the likelihood of future unprecedented events often go with differences in the experts' overall scenarios for the future or with differences in their professional backgrounds, i.e., kinds of expertise.

It may be particularly relevant to analyze the kinds of arguments that seem effective in leading to changes in expert opinion. For example, it might be possible to classify comments made by experts in connection with their forecasts; such a classification could be used, in turn, to semi-automate the forecasting of possible impacts, since comments that have been observed to be peculiarly effective in influencing other judges could be taken into account so as to anticipate the results of further rounds of Delphi interaction among panelists. In other words, the beginnings of a theory of convergence of Delphi judgments might be sought, making possible prediction of the final convergence values to be expected from a Delphi panel, given the initial judgments and the reasons stated for them. (See "Algorithmic or Judgmental" for a discussion of how trend impact analysis uses human judgments to take into account unprecedented events.)

Cross-Impacting or Noncross-Impacting

A *cross-impacting* forecasting approach takes into account the mutual impacts of different events or trends. A *noncross-impacting* approach does not. There are four kinds of cross impacts that should be distinguished:

1. Time series or trends may interact. Multiple covariance methods are among the many methods used to deal with this kind of cross-impact.
2. The probabilities of different unique events may interact. For example, development of cheap fusion power may preclude the development of cheap geothermal power.
3. Events may impact trends. Trend impact analysis (TIA) takes this into account.
4. Trends may affect the probabilities of events.

There are a variety of ways in which this list may be extended. Either trends or events themselves may affect each of these kinds of impacts. For example, in TIA the magnitude of an impact of an event upon a trend may be made to depend on the then-current expected trend value. Impacts may cross-impact. For example, the impacts of different events on the same trend line may rarely be expected to be additive. This is easiest to see when the trend falls between natural limits, e.g., it is a percentage. Surely, then, no number of positive impacts may be expected to raise the forecast value above 100 percent; consequently, the joint effect of these impacts often is less than their sum.

In addition, there may be any number of higher-order interactions among impactors or impacts. For example, two trends together may have a nonadditive effect on a third. Temporal order effects also may be significant; which of two events occurs first may be all-important in determining the impact on a third event or on some trend.

Current multivariate methods lend themselves to the development of sophisticated trend interaction models. Models that take into account interactions among event probabilities have encountered the difficulty that when such events are considered unique or unprecedented their probabilities must be estimated by judgments rather than by computational algorithms. It seems likely, though it is not strictly logically necessary, that the interactions among their probabilities also must be estimated by human judges. This leads, unfortunately, quite rapidly to a very large number of judgments. Where there are E events, there are $E(E-1)$ impacts of one on another to consider. Thus, unless the number of events considered is kept quite small, the number of interaction judgments needed soon becomes onerous and finally becomes unfeasible.

Sometimes, this problem can be dealt with by incomplete sampling schemes that assign to each of several judges only a portion of the necessary judgment load. However, this calls for a number of more or less interchangeable judges, an unlikely condition where high levels of expertise are called for. Alternatively, the judgmental task may be made easier by reducing its information content. For example, judges may be asked merely to trichotomize each of the $E(E-1)$ possible impacts into negative, neutral (or null), and positive impacts. Presumably, this is less difficult for the judges than estimating the magnitudes of the impacts. Alternatively, the judges might be asked to select some small number of high-magnitude impacts or only those whose magnitude exceeds some specified value. All these methods, however, still require the judge to consider, at least implicitly, each of the $E(E-1)$ possible impacts. Sometimes a further reduction can be achieved by assuming that the impacts are symmetrical, i.e., that the impact of event A on event B is the same as the impact of B on A so that only $E(E-1)/2$ impacts need be considered.

Among significant problems remaining for attack in this area are

1. Generalizing the solution of higher-order impact combinations.
2. Including in a general sense the effect of event temporal sequence.

Intrinsic Process-Related Forecasting Methods

Process-related forecasting methods are distinguished from each other, of course, by the process used. However, selection is necessary in our enumeration of these methods, since clearly any human activity may be claimed to be an appropriate precursor for a forecast. We are restricted here to algorithmic or judgmental, simulation or analytic, etc., techniques that share the common characteristics of being generally consistent with scientific method or, more broadly, with the modern unmystical approach to forecasting. Again, as noted above, we regard the various alternative process-related dichotomies as essentially independent choices. For example, we can readily conceive of an algorithmic analytic interactive unbranching Monte Carlo process.

Algorithmic or Judgmental

An algorithmic forecasting technique generates its forecast automatically, according to a set of operating rules—a program—that in principle can be followed by a computing machine, and often is. The operating rules constitute the forecasting algorithm. (As the term is used here, unlike some mathematical usage, an algorithm may include a random, not necessarily reproducible, step such as reference to a random number generating program or device.) By contrast, a judgmental forecasting technique depends on human input. For example, the forecast may be the average of estimates provided by a number of expert judges.

It should be noted that even algorithmic techniques ultimately descend from human judgments—about the particular algorithm to be used. Moreover, judgmental techniques usually rely on some computing algorithm (such as calculating an average) to generate useful forecasts from raw products of human judgment. This point is not trivial. It indicates that the distinction between algorithmic and judgmental forecasting has only the most remote bearing on choice between human and robot judgment; rather, the distinction pertains to what kinds of human judgments will be used, and at what points in the forecast-generating process they will enter. The forecasting process should be seen as one that requires certain tasks to be accomplished. At issue is which of these should be left to humans and which to machines.

One task is the consideration of available data. These may be data about the very variables whose values are to be forecasted, or they may be about other variables possibly related to them. All such data necessarily come from the past or present. Humans do better than machines when the quantity of data is either very small or very large. Machines can process and concurrently manipulate masses of information that overload humans, who must resort to what amounts to data sampling techniques. However, when the quantity of data exceeds machine storage capacity (presently large and rapidly growing larger), it is often difficult to program machines to select from among available data as adroitly as do expert humans. A comparable problem has been noted in connection with attempts to program chess-playing computers. The game is in principle subject to a closed, analytic solution, but the quantity of data that must be processed transcends the capacity of present machines. Therefore, machines, like humans, must "satisfice"[3] rather than optimize when they select a chess move. However, to do so, the machines, like humans, must ignore some available information while considering other data, and here it has proven difficult to provide machines with the so-far unprogramable intuition that enables a chess master to select, before he has thought through to the end, which lines of play to analyze fully and which to ignore. See also "Considering Unprecedented Events or Not Considering Unprecedented Events" for possible research to determine the sources of expert judgment.

Another task is choice among alternative forecasts. Many, though by no means all, forecasting procedures involve, at least implicitly, alternative forecasts among which there must be a choice. When the alternatives stem from explicit differences in algorithm, it is generally possible to apply the alternative algorithms to the generation of retrospective "forecasts" about data already available. Then a basis for choice among alternatives may be the accuracy with which the alternative forecasting algorithms "predict" what is already known.

Here, too, when the quantity of information available exceeds the rather small amount that humans can handle, machines have a general advantage, unless the quantity of information becomes so large that they are overwhelmed, and then humans' superior ability to sample and select particularly relevant information once more gives them the advantage. However, a fundamental objection to evaluating a forecasting procedure in terms of its accuracy when used retrospectively is that this neglects the possibility that a poor fit may be due to the influence of unique events in the past that uniquely perturbed the trend.

The judgmental-algorithmic dichotomy can enter at a variety of levels in the consideration of the state-of-the-art in forecasting. A forecasting procedure may be algorithmic, for example, but the procedure for evaluating its accuracy may be judgmental. This is often the case when a time series is extrapolated using a "canned" extrapolation package. Such packages generally generate a variety of alternative extrapolations, depending on various algorithm options, such as the degree of polynomial to be used or whether mean error or mean squared error is to be minimized. Often the human users of such programs examine displays of alternative extrapolations and select the most "reasonable-looking." Clearly, this procedure is only marginally algorithmic or objective. The computer's function is essentially that of idea generator; it generates a variety of possible forecasts, and the human judges select from among them. Clearly, the greater the number of forecasts considered, the more the whole procedure is judgmental and the less it is algorithmic.

Unfortunately, when the "reasonableness" of forecasts is the basis for the selection or rejection of a forecasting procedure, the human judges who must decide whether particular forecasts are reasonable face a dilemma. On the one hand, an "unreasonable" forecasts is necessarily a forecast that something will happen that the judges do not expect will happen. But if all forecasts are rejected that predict something unexpected, then the forecasting method used can never teach the judges anything new—and its chief use would seem to be to lend a coloration of science to their preconceptions.

On the other hand, a forecast can serve as a kind of stimulus to the imagination of human judges that prompts them to invent a scenario that makes the forecast probable. It thus can serve as a kind of psychological test of creativity that all too many "experts" pass with high marks. Research on the validity of the Delphi process, for example, has shown that expert judges, given "forecasts" that are falsely stated to be the consensus of other experts (but really are deliberately deflected off scale from the expert consensus), will sometimes abandon their previous estimates (which actually were close to the group consensus) in favor of the false "consensual" predictions.[4]

Considerations like the foregoing were among many others that contributed to the development of the trend impact analysis forecasting system by The Futures Groups, Inc. It was recognized that for the quantities of data most typically available for trend extrapolation, computers do better than humans in generating extrapolations. It was also recognized, however, that humans do better in taking into account the impacts (see below) of possible unprecedented events. Therefore, human judgments about the likelihoods, impacts, and impact time lags of events are permitted to modify the algorithmic extrapolations. As usually used, TIA deals only with possible future unprecedented events; however, a more consistent use would be also to take into account unprecedented past events. Their judged impacts could be subtracted retrospectively from historical trends before the trends are projected into the future.

Simulation or Analytic

In the sense used here, a *simulation* is a process that steps sequentially through a series of intermediate states starting with an initial or boundary condition state and terminating with a final or predicted condition state. The intermediate states that intervene between the initial and final states are taken to represent states of the system under investigation that occur in the (possibly zero) time between the initial state and the final state. The actual system is considered to pass through corresponding states in the same order.

An analytic model also may involve a series of computational steps that intervene between the initiation of the algorithm with the initial data and its generation of the final forecast, but the intermediate steps are not thought of as corresponding to intermediate temporal states of the system under investigation. Thus the distinction between a simulation and an analytic model turns on the interpretability of intermediate steps in the process of deriving a final forecast from initial input: if the intermediate steps are interpreted as corresponding to physically realizable intermediate states of the system under consideration, the model is a simulation; if not, it is not.

Considering the relative merits of simulation and analytic approaches is like consdering the relative merits of time-series or cross-section forecasting. A time-series forecast can be thought of as a limiting case of a simulation in which the simulation is nearly vacuous because only one characteristic of the system is tracked—the time-series variable. Simulations characteristically give rise to multivariate time series because several variables are dealt with concurrently.

When interest focuses on a particular future time, obtaining forecasts (through simulation) about intermediate times may nevertheless be of value by virtue of their relevance to the "reasonableness" of the simulation. Since simulations are multivariate, they also typically give more of a "feeling" for the process of emergence or evolution of future states over time than does a simple univariate time series. Thus a simulation leading up to the particular future time of interest may generate an implicit scenario about how the future state may come to pass, and this may suggest important insights about aspects of that future state that would not so readily come to mind if the forecast about the future state were obtained from an analytic model.

Forecasting techniques that start off as analytical methods have a way of evolving into simulations if they survive the test of continued use. This is a point about the social anthropology of futuristics rather than its methodology, but it is nonetheless relevant in this context.

An analytical method that continues to be used comes to be familiar to its users. Its computational steps, which may have seemed abstruse and strange at first, tend to become familiar and less puzzling. Since the same computation generally can be performed in a variety of ways, there is a tendency to try different arrangements of the computational steps, perhaps in order to reduce processing costs, reduce rounding error, or eliminate the need for the intermediate storage of data. If a particular computational step involves a formula that is similar to one used in some other step or in some other model—a formula that may apply to altogether different data—this tends to be noticed. Similarities are sought between the objects of similar computational operations. There is also a tendency to assign physical meaning to such computations. Thus, if an exponentially attenuating sine function is a computational step in a particular analytical forecasting model, there is a strong tendency to think of this step as reflecting the attenuation of some sort of periodic process. In this way, recurrent computational patterns tend to accrete the sorts of physical or theoretical interpretations that gradually convert the analytical model into a simulation.

The distinction between simulation and analytic models parallels a familiar distinction in the methodology of social science—the distinction between *hypothetical constructs* and *intervening variables.* As MacCorquedale and Meehl pointed out,[5] a hypothetical construct is an intervening variable (or the value assigned to a particular computational step) that is thought of as corresponding to some reality. Bergmann has argued quite convincingly that it is redundant to assume that an intervening variable is a hypothetical construct, since it is rather the task of the scientist to discover whether this is the case for any and all variables.[6] Seward has also pointed out that the utility of an intervening variable and the desirability of its being regarded as a hypothetical construct stems largely from the recurrence of the same computational pattern in a variety of contexts.[7] Thus, belief in the reality of, say, the neutrino, stems from encountering the corresponding computational form in a variety of different settings. Since the thing keeps turning up in equations, one begins to suspect that there is something really there.

This process of coming to believe in the physical realizability of an analytic model thus would appear to be central to the process whereby scientific theories evolve or, more generally, the way that scientific paradigms evolve.[8] Surprisingly, I know of no effort to make explicit and more or less routine this process, although it seems to be the sort of process for which an algorithm could be developed. It should be possible to enhance the state-of-the-art in forecasting methodology by incorporating in certain forecasting algorithms a procedure for searching through a body of different forecasting formulas, developed for the relations among various future variables and past data in order to discover parallel or isomorphic computational patterns. Such parallels may be brought to the attention of the program users, since they may well suggest unguessed theoretical and actual connections in the processes under study. In addition, each frequently occurring computational pattern can, in effect, be given its own name, so that it may thereafter be considered a building block in the development of yet more elaborate computational patterns. Thus algorithms can evolve from analytic to simulation models. They will do so in a way that should hasten this seemingly desirable process and should, in addition, render explicit the steps and assumptions involved. A first approach to this is discussed above, under "Function Family Determining or Function Family Assuming."

Interactive or Noninteractive

An *interactive* procedure permits a human operator to respond to intermediate output in such a way as to influence the final output. A *noninteractive* one does not. For example, the procedure may present itself to the human user as a game, in which he "competes" with the computer. Computer output is responded to by the user in such a way that he seeks to optimize some figure of merit. Such an approach is natural where the forecasting procedure is a simulation of some social system which naturally involves competing elements. Here one or possibly several different human operators may represent various competing interests. If such a game is played over and over again, using, probably, different teams of participants, a sort of quasi-Monte Carlo method may be possible, where the distribution of results from different plays of the same game may be used as the basis for estimation of future values that are considered to correspond to, say, averages of the individual game outcomes.

Sensitivity analysis often involves formidable mathematical obstacles to a closed-form solution. In this case an interactive approach may provide a convenient short-cut. The user may try various changes in the parameters of the different boundary variables or predictor variables in order to determine how sensitive the resultant solution is to the changes in their values. In this case, any one forecasting run may, in itself, be noninteractive, but a succession of runs using different values for the boundary and predictor variables may constitute an interactive unit for a heuristic sensitivity determination. This approach is generally feasible for any forecasting system, but the way the computational and procedural steps are set up may either favor or discourage it by, for example, reducing the cost of the repeated runs by saving the results of common intermediate calculations.

From a general standpoint, an interactive approach presumes that the user may learn something from an intermediate output that will lead him to make a decision about further processing that he would not have made without the availability to him of that intermediate output. Thus the conditions for the suitability of an interactive as opposed to a noninteractive approach parallel the conditions for the suitability of time-series and simulation methods as opposed to cross-sectional and analytic methods. Nevertheless, it should be clear that these are logically independent characteristics of a prediction system; we may have cross-sectional interactive simulations, time-series analytical noninteractive procedures, etc.

It is useful in programing to provide a certain minimum amount of interactive capability for essentially all programs. At the very least, a user can be provided with interrupt options that enable him to abort any job; he should also be able to intervene and change values of system variables. In effect, any major prediction run should give users the same sort of options that are generally given users who are operating a program for debugging purposes, where standard program packages permit interruption, resetting of variables, the display of intermediate results, a step-by-step tracing of the flow of variable changes, and a return to preset intermediate checkpoints.

Branching or Unbranching

When a forecasting procedure provides the opportunity for interaction within a single prediction "run," as opposed to no interaction opportunity or opportunity only between "runs," then each interaction point ordinarily constitutes a point at which the flow of computational operations may branch, depending on input from the human operator, judge.

A *branching* forecasting procedure is one that has the capacity to follow alternative computational sequences, depending on events that occur in the course of a computational "run." Such events need not involve interaction with a human; the branch may depend on the value of some intermediate computational result. For example, the procedure may elect to pool several different past-data trend lines into a single synthetic variable if certain criteria for their mutual colinearity are met. This, in turn, may convert a multivariate prediction problem to a univariate one.

An *unbranching* procedure does not have such a capability. All decisions about the computational steps have to be made in advance of the "run," using whatever data are available and interpretable at that time.

Of course, it is a fact that no sequence of computations can ever add to the body of information contained in the raw data input to a forecasting program. Thus, in principle, any decision that need be made by branching somewhere downstream in the operation of an algorithm could also be made at a single grand branchpoint at the headwaters or starting point of the program. However, even when interaction with humans does not enter the picture, it is often preferable to use a branching algorithm than to use one in which all choices are made prior to the start of computation.

The issues involved here parallel those that involve use of a decision tree as opposed to a single-decision strategic model in the analysis of a mathematical game. A single grand-strategic decision at the start of the "run" may overload the processing capacity of the system, be it human or machine capacity that is involved.

Branching may be particularly desirable when a simulation is involved, and the branches correspond to actual decision points as they are expected to arise in the real situation. For example, it is not natural to say that today we are deciding what sorts of fuel to use for aircraft of the year 2000, although the decision will surely be made before that year arrives. A simulation of the future of aircraft fuel technology would do well to make the decision *inside the simulation* depend on the judgments of human operators interacting with the simulation at appropriate downstream points. Most likely, the decision would be dissected into a series of decisions extending over a period of time.

Monte Carlo or Closed Form

Whether or not the computational procedure is a simulation or is analytical (in the sense that this distincton was made in an earlier section), it may also be a Monte Carlo or a closed-form procedure. A *Monte Carlo* procedure generates a sample of outputs selected by some randomizing process from a population of possible outputs; then the actual predicted values constitute sample statistics of the generated sample. For example, a future expected value of a variable may be estimated by generating a sample of possible future values and calculating the sample mean. A *closed-form* procedure calculates the desired forecast directly.

Monte Carlo procedures tend to be associated with simulations, while closed forms tend to be used for analytic models. This is not logically or mathematically necessary but is a consequence of the fact that simulations tend to involve more complicated structures of relations among the variables involved (and generally also involve more different variables) than do analytical models. Consequently, computation of desired results by closed-form methods tends to be relatively formidable for simulations, and Monte Carlo methods tend to be relied upon.

Generally, Monte Carlo methods are considerably more costly in computational expense than are closed-form methods of dealing with the same forecasts, since Monte Carlo methods require many "runs" of the same forecasting procedure, each generating one value for the sample of values from which the sample statistic will be computed. Often the computation necessary for a single one of the many Monte Carlo runs is of the same order of magnitude of costliness as an entire closed-form solution.

However, closed form solutions often call for much greater human time and effort in developing the computational algorithm. This extra cost, unlike the extra cost of Monte Carlo runs, is incurred only once, however. Therefore, choice between Monte Carlo and closed form approaches to forecasting generally turns on how often the particular forecast will be needed. More often, the more a closed form approach is favored.

A generally less important advantage of closed-form methods is that they provide precise values of the forecasts, while Monte Carlo methods give only approximate values, since they rely on sampling. However, because the law of large numbers is one of the more reliable laws of nature and mathematics, this consideration is generally a quibble, except for the rare instance where peculiar distributions are generated, such as the distribution of values of the tangent of rectangularly distributed angles, where the reliabilities of expectancies do not improve as sample size increases. Unfortunately, when a model is sufficiently complex, it may not be immediately obvious that a nonconvergent case has been encountered. It is desirable, therefore, to include in the development of any Monte Carlo method a test of whether the results do converge as sample size increases.

Intrinsic Output-Related Forecasting Methods

Classification of methods by kind of output constitutes the third and last basis for distinguishing among intrinsic forecasting methods. We are concerned here not with the subject matter of the output, but rather with its form or format. This includes the nature and specificity of qualifying or quantifying detail, such as whether the output is probabilistic or apodictic. In addition, we include in this set of distinctions the notion of whether a single forecast is generated or a set of forecasts, and the sorts of time and contingency relations among the various forecasts when more than one is generated.

Probabilistic or Apodictic

Probabilistic forecasting assigns a likelihood, cumulative probability, or conditional probability to the forecasted event or events. In *apodictic* forecasting, the forecast is simply an assertion, without probabilistic qualification. Clearly, probabilistic forecasting is preferable whenever the probabilities or likelihoods generated are reasonably accurate.

This is not an easy condition to meet, if it is required that the accuracy be demonstrable. Since a given future event either occurs or does not occur, it is not immediately obvious how a probability other than 0.00 or 1.00 can be assigned to it. Perhaps it would be better to call the "probabilities" or "likelihoods" assigned in probabilistic forecasting "quasi-probabilities" and "quasi-likelihoods." The term "quasi" is intended here to have no pejorative connotation whatsoever.

These quasi-probabilities and quasi-likelihoods, whatever their interpretability as actual expected frequency ratios or whatever their measure-theoretical status, do serve to rank order forecasted events with respect to their judged or estimated probability or likelihood. While there may be a question about whether a future event assigned a likelihood of 0.20 is truly half as likely as one assigned 0.40, there can be no question that the lower likelihood assignment points to a judgment or estimation that the likelihood is lower. In short, the quantitative values used in probabilistic forecasting can more reasonably be said to be an order-preserving transformation or function of the true probabilities or likelihoods than they can be said to equal them.

A bit more than this can be said. The quantitative probabilities generated in forecasting may also be thought of as constituting a kind of quality rating of the forecasts involved. It may be argued that an apodictic assertion that a future event will occur that is assigned likelihood 0.20 in a corresponding probabilistic forecast has half the quality of an apodictic assertion that a future event will occur whose probabilistic rating is 0.40.

Ultimately, however, the matter hinges on the source of the probability or likelihood values that appear in probabilistic forecasts. If they represent the consensus or average among judges' guesses, then they are Keynes's consensual probabilities or likelihoods, and it may seriously be questioned whether they do indeed predict frequency ratios. Here is a question that almost cries out for empirical study. What is needed is to collect a number of probabilistic forecasts, all of which share the same probability level, e.g., 0.50. Then, after the forecast times, the proportion of the forecasts that proved accurate should be counted. If the probability numbers in the probabilistic forecasts accurately correspond to frequency ratios for various probability levels, then it would be justifiable to regard such probability numbers as probabilities, or at least good estimates of probabilities.

Harold Schiffman[9] actually conducted such a study, using the very extensive data that are available for pari-mutuel horse-race betting. He found that the actual proportion of horses for whom the pari-mutuel odds were, say, 1:10 that actually won was, indeed, one in eleven, and so on for all other odds. In other words, if the proportion of money bet on a horse was p, then the horse did indeed have a probability p of winning. More precisely, the proportion of the time that a horse on which a proportion p of money was bet actually won turned out to be p. He found that this law applied very accurately.

Point or Interval

Point forecasting specifies a single particular forecasted value. *Interval* forecasting specifies an interval within which the value is forecasted to fall. Point forecasting is not infrequently apodictic, and interval forecasting is almost always probabilistic. Often a probability distribution function is defined for the interval. A somewhat lower level of precision is involved in the case where a probability is assigned to the forecasted value falling in the interval. The latter characterizes fiducial probability forecasting.

The language used in the foregoing paragraph is borrowed from the language of statistical estimation theory. Statistical theorists are familiar with the distinction between point and interval estimation, with fiducial estimation, and so on. Forecasting can be thought of as a generalization of estimation, in which the estimated value is not a parameter of a population from which a sample has been drawn, as in the standard estimation problem, but rather is a parameter of a population in some way related to the population from which the data are drawn. The forecasting task is thus an extension of the estimation task. In addition to estimation of unknown population parameters, in forecasting it is necessary to determine the relation of the population from which data are drawn to the population of interest. This is considered above under "Bayesian or Classical."

This point is generally relevant to forecasting. It is mentioned here because it has special bearing on certain common practices in interval estimation. In general, the wider the interval that is estimated or forecast, the less confidence is attached to a corresponding point estimate or forecast that the predicted value will be in the middle of the interval or, more precisely, at the interval centroid.

Interval width, in many common algorithms, is determined by two factors: the unreliability of the source data from which the interval estimate or forecast is derived and the unreliability of the relation between the source data population and the population about which the estimate or forecast is made. Unfortunately, common techniques do not permit the user to distinguish between these two sources of uncertainty in the resultant forecast. For example, where common regression methods are used, a low correlation coefficient may reflect either low reliability in the historical data or a weak relationship between these data and future values of the dependent variable.

In practice, improving forecasting accuracy calls for different approaches, depending on which source of uncertainty is involved. When the uncertainty stems from unreliable past data, then clearly it will be of little avail to seek to improve the characterization of the relationship between the future data and the given past and present data and, conversely, when the uncertainty stems from a feeble estimate of the relationship between future and given data, it will be of little value to seek to improve the reliability of the predictor-variable values. In short, the accuracy of forecasting is no greater than the accuracy of the least accurate contributor to the forecasting process, be it the historical data or the relation of the historical to the future to be predicted.

It seems possible to develop forecasting algorithms that decompose the uncertainty of forecasts into separate components reflecting the unreliability of the historical data and the weakness of the dependence of the values to be predicted on the historical data. If point estimates are used, then the commonly used standard error of estimate could be decomposed into the two components; if interval methods are used, then in addition to the interval reflecting the composite of the two components, an interval could be reported reflecting each component's separate contribution to uncertainty of forecast. Where a probability distribution function is defined over the interval, the function can be similarly decomposed.

Univariate or Multivariate

Multivariate techniques are both more sophisticated and more costly than the corresponding univariate methods. In general, where a number of different variables are all strongly interrelated, it is preferable to use multivariate methods *in advance* of the forecasting effort in order to elucidate the interrelations among the variables so as to reduce the number that need be dealt with by taking advantage of their interrelations to select representative ones or to combine them to form representative composites. Techniques such as hierarchical cluster analysis, multidimensional scaling, and factor analysis are used appropriately to this end. These techniques are not equally powerful. In general, if cluster analysis produces a satisfactory reduction in the number of variables, no other data-reduction technique is required; similarly, successful data reduction using multidimensional scaling will render factor analysis unnecessary.

Time Series or Cross-Section

Compare time-series forecasting with estimation limited to a particular point in future time. Clearly, the two approaches are limiting cases on a continuum that deals with the extent to which a particular point in time is of sole or predominant interest, as opposed to there being a time range over which different time points have parity in interest.

Clearly, in general a forecasting procedure should be so designed that it optimizes (or satisfices, if need be) with respect to the very time or times for which forecasts are desired and not for other times. Thus, if only a single point in time is of interest, then one would generally disregard times other than the time of interest. To do otherwise would usually waste computational work or human effort. This assumes, of course, that *by considering only the time of interest, a better forecast can be generated for the time of interest.* In general, this last proviso is no obstacle to focusing on a single point in time, since typically forecasting involves some kind of distributive or algorithmic effort with respect to the various times for which forecasts are made, so that a procedure that makes forecasts for more different times generally does less well for a particular time.

There is a difficulty here, however. Ordinarily, there is no good way of assessing the accuracy of a forecast about an as-yet unobserved future event or set of events.[10] This is what has motivated reliance on goodness of fit to the past data or on "reasonableness" of extrapolation. If all the past data have equal bearing on all of the future, then there is no way that goodness of fit to past data can be used as a basis for differential emphasis on a particular future time. However, if certain past values are more related than others to the particular future time or times of interest, then accuracy with respect to goodness of fit for the particularly relevant past data can be given greater weight. For example, events that occur at certain intervals or in connection with particular unique past occurrences may be considered more relevant. Thus advertising revenue in election years would be particularly relevant to extrapolation about advertising revenue in a future election year.

As an example of how such a consideration may be incorporated into a forecasting method, trend impact analysis permits giving greater weight to particular past data. A particular year may be designated for maximum weight, and the weights given other past years are reduced roughly as a reciprocal function of the number of years they are from the maximum weight year. As yet, the method does not permit as much flexibility in weighting as might be desirable; for instance, it does not permit extra weights for events occurring every four years.

However, a forecast that is unreasonable for some time other than the one which is directly of interest may justifiably be rejected on several grounds. It could, for example, be rejected if there is no reason to believe that the forecast is pertinent only to the time of interest.

This principle is simply an extension of the criterion of goodness of fit to past data. A "forecast" about known past data is used to determine whether a forecast about the future shall be made according to a particular prediction formula. Similarly, then, a forecast about some other future time, for which some other good basis for forecasting may be available, may be used to select the prediction procedure to be used for a future time whose indices are less readily estimated.

In concept, it would be possible to generalize the forecasting algorithm so that any set of past or future data points with any set of weights could be used to determine an extrapolation formula to be applied to any set of other data points, either past or future, with any weights. This very general approach would transcend and obviate concern with the time-series or cross-section forecasting issue, except insofar as cross-section forecasting involves multivariate as opposed to univariate forecasting.

Component Aggregating or Component Distributing

Component aggregating forecasting techniques perform operations directly on some variable without considering the various parts of that variable. *Component distributing* techniques, on the other hand, generally develop forecasts for the factors or components of some more general variable. These forecasts are then combined to produce a forecast for the more general variable. Several factors enter into the decision on the degree of disaggregation necessary to produce the best forecast. One obvious consideration is time. More computer time and human effort is usually involved in producing disaggregated than aggregated forecasts. This effort may not be worthwhile unless the forecasts of the disaggregated parts of the more general variable are also of importance to the user. In that case the decision may rest with the utility of the subforecasts compared with the extra effort and cost necessary to derive them. The same type of tradeoff exists between the accuracy of the general variable forecast and the effort spent on the subforecasts. If the accuracy of that forecast can be sufficiently improved by disaggregation, the effort may be well spent even if the subforecasts have no other use.

Finally, the level of disaggregation may depend on the ability of the experts who supply necessary judgmental inputs to the forecasting process to make meaningful statements concerning the aggregated variable. A judge, for example, may be able to estimate the impact of a certain new piece of legislation on the use of small private planes but may not be able to estimate reliably its impacts on total air passenger miles in the United States. In such instances it becomes imperative to disaggregate to at least the highest level that can be meaningfully handled by expert judges.

Sometimes, for example in a watch industry sales forecast developed by The Futures Group, aggregated and distributed forecasts about the same data may be used as mutual checks and to uncover contradictory assumptions about relations among components and their trends.

Extrinsic System-Related Forecasting Methods

When we distinguish among forecasting methods by *extrinsic* rather than *intrinsic* characteristics, we mean of course to distinguish them with respect to characteristics of the larger system in which the forecasting method is used. It is convenient, further, to distinguish between those cases where it is in terms of use that we determine how we classify the method as opposed to some other aspect of the system. The system, here, may be thought of best in terms of a process flow chart that would show how the information that the forecasting method processes in order to generate forecasts moves through the system. For example, in a self-modifying method, the flow chart necessarily includes a feedback loop, while without self-modification capability, there is no such necessity. A recursive capability is a more subtle form of looping than occurs with self-modification. As explained below, a recursive forecasting system makes forecasts about itself. This provides a special opportunity for self-modification. However, again, we must caution that the two dichotomies, *self-modifying* vs. *non-self-modifying* and *recursive* vs. *non-recursive*, are logically independent. For example, a non-recursive method may be self-modifying in some other way, or a recursive method may not make use of forecasts about its forecasts for purposes of self-modification but for some other purpose, such as to sharpen a particular forecast being made on a particular single occasion. Note that a self-modifying forecasting system does not merely modify its forecasts; rather, it modifies the ways whereby it generates them.

Self-Modifying or Non-Self-Modifying

A *self-modifying* forecasting system automatically modifies itself on the basis of its experience as a system. It contains, in effect, an algorithm that enables it to learn. This requires feedback to the forecasting system of the results of its operation. This, in turn, requires some way of assessing the utility or value of what the system does or produces. Various kinds of criteria may be used; important distinctions with respect to use or output are discussed below under "Accuracy Maximizing or Utility Maximizing" and "Forecast Oriented or Decision Oriented." In addition, there may be criteria that do not depend on output; for example, the system may take into account how complex or time consuming its forecasting operations are and modify itself so as to forecast more simply or more quickly.

The complete science of forecasting may be thought of as a grand self-modifying system. To be useful, the distinction in this section, however, must pertain to an automatic self-modification algorithm built into a forecasting system so that the modifications take place internal to the system's operation, without calling for fresh human judgments that deal directly with the modifications themselves. Of course, human judgment must have been used somewhere upstream in the development of the self-modification algorithm.

Recursive or Non-Recursive

A *recursive* forecasting system routinely makes forecasts about itself. That is, it forecasts its own forecasts, or it forecasts some other aspect of its operations. That a system may forecast its own forecasts is not just a play on words. For example, suppose a regression algorithm is used to forecast future values of a historical trend. Suppose, also, that the historical trend values are themselves uncertain and subject to repeated correction. A trend may be noted in the corrections of the historical trend, and this trend may itself be extrapolated, leading to a forecast of what the forecast generated by the regression model will be some time in the future, after further corrections have been made in the historical data. If this forecast of the forecast is itself based on use of the regression algorithm, then we have a recursive application of the algorithm.

The capability to be used recursively is very important if a system is to be self-modifying (q.v., above), because an important way that a system may modify itself is by extrapolating a trend in improvements. Therefore it is generally desirable that systems automatically monitor changes in themselves so that trends in these changes may be noted. Note, also, that another interesting possibility is the use of one approach to forecasting to forecast trends in another;

for example, an aperiodic trend toward increased reliance on spectral as opposed to autoregressive methods in periodic forecasting may be used to help predict what periodic forecasting methods will be preferred in the future.

Extrinsic Use-related Forecasting Methods

Again, we have two independent dichotomies. There is considerable customary association between the two. Decision-oriented systems are usually utility-maximizing, and forecast-oriented systems tend to be accuracy-maximizing, since this throws into sharpest relief whether or not the system is taking into account the criteria most closely related to its use. Nevertheless, again, we caution that the association is not logically necessary. An accuracy-maximizing system might be decision-oriented; a utility-maximizing one might be forecast-oriented, For example, we might have a utility-maximizing system that produces forecasts that will have a wide variety of uses, say, because they are generated by a futures-oriented information system for storage in a large data bank of statements about the future. Such statements might be used by many different users for many different purposes. Here, utility maximization would require some statistical model of the distribution of user purposes. In such a context, a forecast-oriented rather than decision-oriented utility-maximizing system would be quite natural.

Accuracy Maximizing or Utility Maximizing

Forecasting is rarely an end in itself. Rather, it is usually a means to an end, and that end is typically a correct decision, whose achievement is to be facilitated by the forecast. *Note*: The decision is normally in the *present*, at the time that the users or consumers of the forecast receive or consider the forecast.

When a single forecast will have many applications for many different decisions, it may make sense to evaluate the forecast by its accuracy (insofar as this may be estimated). Most often, however, this is not the best index of forecast quality. Since forecasting is rarely an end in itself, the costs or benefits of a forecast rarely are intrinsic to the forecast itself; rather, they usually are consequences of the decisions that hinge on the forecast.

This is an argument for a utility-maximizing, rather than an accuracy-maximizing, approach to forecasting. When forecasting is *accuracy maximizing*, the choice among alternative forecasts or forecasting system is in terms of their anticipated accuracy. When forecasting is *utility maximizing*, the choice is in terms of the costs/benefits of the decisions to be made on the basis of the forecasts.

Since the relation between decision and forecast is usually complex, the most accurate forecast in general will not be the most useful. In particular, if decisions will change sharply when a critical level of some forecasted variable is reached, then clearly it is crucial to develop a forecasting system that maximizes accuracy in the critical range, even at the expense of a general reduction in accuracy, averaging over all possible predictions (including ones not in the critical range).

Development of a utility-maximizing approach requires that the forecast users know at the time they use the forecasting system just how their decisions will relate to the possible forecast results. This is often not the case. However, clearly it would in nearly every instance be an instructive exercise if users were required to think about the relation between possible forecasts and the decisions they would then make. It is possible to design interactive forecasting programs so that they solicit such information (e.g., in the form of designated cut-off or critical values) from the users. Where this information is provided, the programs could then seek to maximize utility by maximizing accuracy where it is most critical. Where the information is not available, the programs could proceed with the familiar accuracy-maximizing approach.

Utility maximization requires that the utility or value of alternative futures be specified and compared. This requirement is difficult to meet. Often evaluating the differences among the consequences of alternative decisions calls for comparing apples and pears, highway plans and old-age assistance programs. However, the difficulty of such a task is not a good basis for seeking to avoid facing up to it. It would seem generally beneficial for the development of futures research if forecasters were encouraged to ask themselves how and how much fundamental value presuppositions influence the utilities they assign to alternative possible outcomes.

Forecast Oriented or Decision Oriented

Another and very similar distinction is between forecast-oriented and decision-oriented forecasting systems. *Forecast-oriented* systems provide forecasts. *Decision-oriented* systems provide decisions. To accomplish the latter it is necessary that the system be instructed by the user on the relation between the decisions that might be made and the forecasts that might be generated.

Forecasted-oriented methods are much more common than decision-oriented ones. Decision-oriented forecasting has been used in computer-controlled real-time operations, such as the operation of a petrochemical plant, in which forecasts about the short-term future state of the system generate decisions about computer-controlled operation parameters of the system. Users probably should have the option to select any forecasting approach either in a forecast-oriented or in a decision-oriented mode.

Another use of decision-oriented forecasting would be where the same sorts of decisions are made over and over again on the basis of the same sorts of forecasts. For example, forecasts of success or failure of college applicants generate more or less routine responses to their applications. It would be feasible, therefore, to develop a decision-oriented program that constructed an appropriate letter of acceptance or rejection in response to each possible configuration of application data.

Of course, there is nothing in this discussion that precludes display of both forecast and recommended decision. Since decision orientation requires input about the relation between decision and prediction, a decision-oriented system would generally be compatible with a utility-maximizing one, but the two dichotomies are nevertheless distinct since, for example, a utility-maximizing system must also have available the cost/benefits of the various possible decisions in addition to information about how decisions relate to forecasts.

FOOTNOTES

[1] Erich Jantsch, *Technological Forecasting in Perspective* (Paris: Organisation for Economic Co-operation and Development, 1967); James R. Bright (ed.), *Technological Forecasting for Industry and Government: Methods and Applications* (Englewood Cliffs, N.J.: Prentice-Hall, 1968); Robert U. Ayres, *Technological Forecasting and Long-Range Planning*, (New York: McGraw Hill, 1969) and Joseph P. Martino, *Technological Forecasting for Decisionmaking* (New York: American Elsevier, 1972).

[2] G. E. P. Box and G. M. Jenkins, *Time Series Analysis* (San Francisco, Calif.: Holden-Day, 1970).

[3] H. A. Simon, *Administrative Behavior*, 2nd ed. (New York: The Free Press, 1957), p. xxv.

[4] M. Scheibe, M. Skutsch, and J. Schofer, "Experiments in Delphi Methodology," in H. A. Linstone and M. Turoff (eds.), *The Delphi Method: Techniques and Applications* (Reading, Mass.: Addison-Wesley Publishing Co., 1975), p. 270.

[5] K. MacCorquodale and P. E. Meehl, "On a Distinction Between Hypothetical Constructs and Intervening Variables," *Psychological Review*, Vol. 55 (1948), pp. 95-107.

[6] G. Bergmann, "Theoretical Psychology," *Annual Review of Psychology*, Vol. 4 (1953), pp. 435–458.

[7] J. P. Seward, *Psychological Review*, "The Consistency of the I-V; Critique of Intervening Variables," Vol. 62 (1955), pp. 155-168.

[8] Thomas S. Kuhn, *The Structure of Scientific Revolutions, 2nd. ed.* (Chicago, Ill., University of Chicago Press, 1970).

[9] H. Schiffman, *Successive Interval Scaling of Money at the Race Track*, Research Memorandum 63-8 (Princeton, N.J.: Educational Testing Service, 1963).

[10] An attempt to accomplish this objective is presented in Chap. 3.

Chapter 3

Validity of Forecasting Systems

Herbert Gerjuoy

The story is told that once upon a time a chicken-packing company was having trouble with its frozen chicken production process. For some reason, rather than seek help from the appropriate refrigeration engineers or processing specialists, they brought in as a consultant a physicist whose specialty was thermodynamics. He listened to their problems and shook his head: it all seemed so simple. Confidently, he strode to a blackboard and said: "First, assume that a chicken is a perfectly spherical ball . . . ." Then followed an hour and a half of moderately high mathematics, for which he charged a moderately high consulting fee.

Of course, the falsity of an assumption does not necessarily render invalid the conclusions that depend on it. A false assumption may be benign rather than malignant: it may facilitate important correct conclusions, while the wrong conclusions it leads to may be unimportant. We all know that real chickens pack more densely than spherical chickens because real chickens are cubical, but perhaps the important aspects of chicken freezing are largely unaffected by how they pack. However, when we suspect that an assumption is false, this should certainly alert us to its impact on conclusions we care about.

I am not a physicist. The problem addressed in the present paper includes chicken processing only as a very special case. The mathematics I shall use will be moderately low, not high. Nevertheless, watch the assumptions. I suspect that some of them are equivalent to assuming that chickens are spherical.

This chapter presents three approaches to estimating the validity of a forecasting system. The first uses reliability to set an upper limit to validity. The second estimates validity of long-range forecasts by extrapolation from the validity of short-range forecasts. The third redefines validity to make it directly measurable.

"Validity", here means average, overall, or typical accuracy. A valid forecasting system is an accurate one. (Note that validity is defined as a property of a forecasting system, not of an individual forecast.) If for each forecast generated by a forecasting system it is possible to measure the (possibly zero) magnitude of the forecasting error, and if the error values so obtained are all mutually comparable, i.e., fall on the same (not necessarily unidimensional) scale, then the system's lack of validity ("invalidity") and consequently its validity can be measured, i.e., quantified. In practice we usually meet these requirements by using the same scale (dimensions and units) for forecasting that we use to measure the actual situation. As a result, the magnitude of the discrepancy between a forecast and the actual situation can be obtained by subtraction or its multidimensional, e.g., Pythagorean, equivalent. For example, forecasts may be of the year of man's first landing on the moon and the scale used may be time measured in years. The absolute values of the differences between the forecasted years and the actual year than may be used to measure the invalidity and hence the validity of the forecasting system.

Any measure of the degree of correspondence between forecast and actual values might do. A natural one is the Pearson product-moment correlation, $R_{(PO)}$, between predictions, P, and actual observations, O. This measure takes into account possible arbitrariness in units of measurement. In effect, it sets the unit of measurement of the forecast values equal to their standard deviation (root mean square about the mean or average) and the unit of measurement of the observed values, similarly, as their standard deviation.

When forecasting system validity is measured directly, it is necessary to wait until the forecasted events occur. Such assessment of the validity of a forecasting system works only when there has been long-term experience with it and when it has remained the same over the period of experience, so that the validity of the forecasting system in the more distant past for forecasts about the recent past and present may estimate fairly the validity of the system today. So-called scientific forecasting is so new and forecasting systems are in such flux that it is rarely, if ever, possible or useful to estimate validity directly. We must rely, instead, on indirect estimates that enable us to estimate a forecasting system's validity with respect to forecasts about what is still the future. A nice trick, if it can be done.

A quantified validity is not necessarily better or more valid than one that is not quantified, nor is a prediction system whose validity is easily quantified necessarily better or more valid than one whose validity is measured less readily. However, validity measures or estimates do provide one more thing to conjure with, and they may be useful or impressive. We can, for example, use validity indices to compare forecasting systems, and this comparison may be useful if we remember to take into account differences in how their validities were calculated, how or how much their validities are valued, and how expensive they are. Similar considerations apply to assessment of possible changes in a forecasting system.

Validity is hardly ever an end in itself; rather, validity is desirable only because the more accurate a forecast is, the more likely it is that decisions based on the forecast will have a favorable ratio of benefits to costs. This suggests, for example, that a less expensive and less valid forecasting system may be preferable to a more expensive and more accurate one—when the greater accuracy does not lead to sufficient improvement in decision benefits or reduction in decision costs to pay for the added expense.

An Upper Limit to Validity May Be Estimated from an Estimate of Reliability

A companion term to validity is reliability. "Reliability" means consistency. If a system always generates the same forecast given the same data, then it is perfectly reliable. But systems that call for human judgments or random computational steps (e.g., Monte Carlo processes) typically generate different, alternative forecasts in different "runs" using the same data to answer the same question about the future. Such alternative forecasts will be termed "parallel" below.

Like validity, reliability can often be quantified, and under essentially the same conditions: when parallel forecasts can be compared quantitatively, i.e., when we can assign magnitude values to the differences among parallel forecasts.

Reliability, or rather unreliability, sets the upper limit for validity. However, validity may fall anywhere below that limit. A perfectly reliable forecasting system—one that always gives the same forecast—may be completely invalid. An example would be prediction of the date of a future event by random selection from a set of dates in an old calendar—but a very small set, containing only one date, February 30, 1973. The forecast would always be the same, so it would be perfectly reliable. It would always be wrong, however, because February 30, 1973 can never come again. (The use of February 30, 1973 in the illustration exemplifies the kind of thinking used in this paper, and the strengths and weaknesses of this kind of thinking should be noted.)

Thus, a perfectly reliable forecasting system may be perfectly invalid, or it may be perfectly valid, or anywhere in between. However, a perfectly unreliable forecasting system must be perfectly invalid.

For psychologists and others interested in testing, Harold Gulliksen some 25 years ago rather elegantly mathematized a formal system whose key application is to the study and measurement of reliability and validity.[1] Quite independently, using very different and much more formidable mathematics, the information theorists have developed a parallel formal system.[2] Gulliksen, writing about mental tests, speaks of a test *score* that may be partitioned into its valid or *true* component and its invalid or *error* component. In information theory analysis of signal detection and message transmission, a *message* is comparably partitioned into *signal* plus *noise*.

The information theory approach uses more powerful mathematics and is better tied in with the broad body of scientific knowledge. For example, noise is related to entropy, a key notion in thermodynamics. But test theory has the kind of modest mathematics I prefer to use when it suffices. Note, by the way, that not every important corresponding theorem has been derived in the two systems, and so at least two eminent careers are possible for a clever translator.

Where there is a single signal, message, or forecast,

$$X = T + e, \qquad (1)$$

where X is obtained score, T is true score, and e is error. When there is a single true score (a single signal, a single future event to be forecasted),

$$s_X^2 = (1/N)[S(X - \overline{X})^2], \qquad (2)$$

where s_X^2 is the variance of the obtained scores, N is the number of obtained scores, Σ is the summation operator, and $\overline{X}$ is the mean of the X scores.

In the formula for s_X^2, each of the discrepancies, e, between forecast and actuality may be thought of as having two independent components: first, a constant bias component, which would be reflected by the average forecasting error over a long run of forecasts; second, a variable component (to be identified below with unreliability) whose long-run average value approaches zero.

It is assumed that no bias afflicts the scores (forecasting system), i.e., X converges to T as N becomes larger and larger, so that the mean e converges to zero. This assumption requires a moment of comment and trepidation. A verbal trick ought not lead us to disregard the real likelihood of bias. Psychologist test theorists have generally gotten away with such verbal ploys as "intelligence is what intelligence tests measure" because there is rarely any way to observe directly the true scores that their obtained scores seek to estimate. Since they rarely have to face the reality of bias, they can generally forget about it. This suggests that we can more comfortably neglect bias in long-term forecasting than in short-term forecasting.

Note, further, that the nature of the model makes any bias an additive constant, so that bias cannot affect differences in the reliability of alternative forecasting systems, but only affects the baseline about which their forecasts vary.

In any case, if it is assumed that there is no bias, then we may replace $\overline{X}$ with T, whenever N is reasonably large.

For convenience (this is *not* an assumption with real meaning; it is a *convention*) we assume that the measuring scale used for X and T has its zero point at T. Then,

$$s_X^2 = (1/N)[\Sigma(X - T)^2] = (1/N)[\Sigma(X^2]. \quad (3)$$

But, by equation (1),

$$s_X^2 = (1/N)[\Sigma(T + e)^2] = (1/N)[\Sigma(T^2 + 2Te + e^2)] = s_T^2 + 2cov(T,e) + s_e^2, \quad (4)$$

where cov(a,b) is the covariance of a and b. By assumption, s_T^2 is zero. Therefore, when only a single forecasted event is involved,

$$s_X^2 = s_e^2. \quad (5)$$

Here, a new nonvacuous assumption surfaces: e correlates zero with T. This is a very powerful assumption. It rules out the possibility, for example, that very large future increases in the value of an indicator may tend to be underestimated in forecasting, perhaps because of forecaster timidity, or perhaps because of forecaster social perceptiveness about what a forecast user will accept.

We are more interested, however, in a system that forecasts a *set* of events.

Now, s_T^2 is no longer in general zero.

$$s_X^2 = s_T^2 + s_e^2 + 2cov(T,e). \quad (6)$$

With the assumption, $r_{Te} = 0$,

$$s_X^2 = s_T^2 + s_e^2 \quad (7)$$

we can then define validity as r_{XT}, the correlation between X and T.

$$r_{XT} = [cov(X,T)] s_X s_T. \quad (8)$$

$$cov(X,T) = (1/N)[\Sigma(X - \overline{X})(T - \overline{T})] = (1/n)[\Sigma(XT)], \quad (9)$$

using the convention that $\overline{T} = 0$, and the assumption that $\overline{X} = \overline{T}$.

$$\Sigma(XT) = \Sigma [T + e)T] = \Sigma(T^2) + \Sigma(eT) \quad (10)$$

$$= \Sigma(T^2) \quad (11)$$

using the assumption that e and T have no linear relationship. Therefore,

$$cov(X,T) = s_T^2. \quad (12)$$

and

$$r_{XT} = s_T^2/s_X s_T = s_T/s_X \quad (13)$$

The most immediate problem with this nice formula for r_{XT} is that s_T is no more available than the T scores are. However, there is a relationship between reliability and validity that can be used to estimate r_{XT}.

By analogy with the definition of validity as r_{XT}, we can define reliability as $r_{X_1X_2}$, where X_1 and X_2 are alternative forecast sets generated by the *same* forecasting system. For example, where judges are used, we may regard $r_{X_1X_2}$ as the correlation between judgments by different judges. There are two kinds of correlation that have to be distinguished here. One is the correlation between judgments by two different judges, each making judgments about a number of different events. The other is the correlation between judgments about each of two different events by a number of different judges. These two correlations in general will be the same only if we regard all judges' judgments as mutually interchangeable, i.e., drawn randomly from the same population of judges (or, similarly, if we view the different events as mutually interchangeable). Formally, the model simply requires a source of data for r_{XX}, and if there is more than one source we simply run the risk of more than one conclusion. That can be an advantage, by providing a basis for estimating whether judges (or events) are, in fact, interchangeable. Thus,

$$r_{X_1X_2} = [\mathrm{cov}(X_1,X_2)]\, s_{X_1} s_{X_2} \qquad (14)$$

$$\begin{aligned} \mathrm{cov}(X_1,X_2) &= (1/N)[\Sigma(X_1X_2)] \\ &= (1/N)[\Sigma(T+e_1)(T+e_2)] \\ &= (1/N)[\Sigma(T^2+e_1T+e_2T+e_1e_2)] \\ &= s_T^2, \end{aligned} \qquad (15)$$

assuming, again, that e is uncorrelated with T, and assuming now for the first time that e_1 is uncorrelated with e_2.

With X_1 and X_2 interchangeable,

$$s_{X_1}^2 = s_{X_2}^2 = s_X^2 \qquad (16)$$

and

$$r_{X_1X_2} = s_T^2/s_X^2. \qquad (17)$$

But, then

$$r_{XT} = (r_{X_1X_2})^{1/2}. \qquad (18)$$

This approach really only gives a maximum value for r_{XT}, because what has been done here has simply been to do formally and in detail what was stated in words above when it was noted that reliability sets an upper limit to validity. However, since we have pointed toward ways of estimating the reliability (by looking at agreements among judges, for example), we do have some basis now for judging the validity of a forecasting system without waiting.

Validity May Be Estimated by Extrapolating Trends in Validity Estimates

Another approach to estimating the validity of a forecasting system may be based on the method that has been used to investigate the personality characteristics of individuals who successfully commit suicide. It is not easy to administer personality tests to such persons after they have killed themselves. What has been done, instead, has been to study the personality characteristics of persons who were unsuccessful in their attempts to commit suicide, and look for trends in these characteristics depending on how close the individual came to succeeding. The characteristics of successful suicides are then found by extrapolating such trends.

Similarly, we may estimate the not-directly-obtainable validities of long-range forecasting systems by noting trends in the validities of short-range forecasting systems as the range grows longer.

We May Redefine Validity So That It Pertains to Forecast Utility, Not Accuracy

Finally, we may cut the Gordian knot: Forecasting, after all, is usually done as a means to an end rather than as an end in itself, except for the presumably small band of futurists who see forecasting as an abstract art form. This may be clarified by contrasting the status of the future, for futurists, with the status of the past for historians, or the status of the mind for psychologists.

The futurist cannot view the future directly. But neither can the historian directly view the past, nor can the psychologist directly view the mind. The historian uses the notion of "the past" to help organize and direct his professional activities, which necessarily always take place *in the present*. Thus, history may be considered an empirical science. The historian may, for example, use his ideas about the past to *predict* that in the short-range future, if we dig in a particular place, we will find the body of a particular Roman. Similarly, the psychologist may use notions about what unobservable processes are going on inside another person to make predictions about that person's observable behavior. The utility of the past for the historian is that the notion helps him better to predict. Similarly, the notion of the mind helps the psychologist.

Obviously, futurists do not use the notion of the future simply to help improve their predictions. On occasion, of course, this is the case: A judgment about the nature of the long-range future may be used to help generate a short-range forecast. More fundamentally, futurists use the notion of the future to help generate forecasts that are useful for those who consume those forecasts.

Futurists provide forecasts that are used by individuals and organizations in decisionmaking. Therefore, it may be argued that the utility of a forecast is determined by how favorably it affects the decisions made by the forecast user. This approach makes it possible for a wholly inaccurate forecast to be a very good forecast. For example, Jonah says, "Nineveh will be destroyed!" But Nineveh repents, and the city is not destroyed. Was Jonah therefore incompetent? No. He provided a very useful forecast, for it had the desired effect upon the users—the rulers and people of Nineveh.

Since the validity of a forecasting system, defined as its accuracy, as has been shown above, cannot be measured directly and indirect approaches seem likely to give only very gross estimates, and since it may be argued that the accuracy of a forecasting system is much less important than its influence on the users of the forecasts it generates, it may be suggested that the definition of forecast-system validity be changed to reflect the utility of the forecasts generated rather than their accuracy. This may be done while retaining the general definition of validity as the correlation of the forecast with its criterion by redefining the criterion. The criterion need not be based on the future event that is forecasted. Rather, it may be based on the response of the user of the forecast.

We may imagine the following. When a futurist makes a forecast professionally, he is required by the code of ethics of his profession to place in a sealed envelope the anticipated effect of the forecast on the anticipated user. This, of course, is itself a prediction. It is the accuracy of this prediction that is used to assess the validity of the forecast.

FOOTNOTES

[1] H. Gulliksen, *Theory of Mental Tests* (New York, Wiley, 1950).

[2] E.g., F. M. Reza, *An Introduction to Information Theory* (New York: McGraw-Hill, 1961).

Chapter 4

The Nature of Unforeseen Developments

Theodore J. Gordon

Introduction

Picture yourself 20 years from now, after a satisfying career in futures research, checking back on the forecasts you and others made in the last few decades. The array of scientific, technological, economic, societal, and political events and trends would be very large indeed. Many of the forecasted developments would certainly have occurred, and from this you would draw some professional satisfaction (especially if they were desirable); others originally expected during the interval you are examining would not have occurred, and this would cause you to search for reasons for the incorrect forecasts. But despite the richness of the forecasts, inevitably you would find that some of the really important developments of the last few decades were not represented at all in the forecasts.

For example, the RAND long-range forecasting study, conducted using the Delphi method in 1964, included a forecast of important scientific breakthroughs expected in the next 30 years or so.[1] About two-thirds of the events forecasted to occur by 1970 had actually occurred by then. But more importantly, perhaps, many scientific breakthroughs had occurred which were not included in the original forecast. The yearbooks of the Encyclopedia Americana, the *New York Times Index*, and other sources were consulted to form a list of significant scientific developments of about the same level of generality as items included by the original Delphi respondents. The list of omissions includes discovery of quasars; growth of holography; applications of lasers to industry and medicine; measurement of the big bang site temperature; the moon-like surface of Mars; effective use of hybrid seeds by many developing countries; establishment of cigarette/cancer relationship; theoretical identification of a faster-than-light particle; synthesis of DNA; the linking of criminal behavior to abnormal chromosomes; discovery of pulsars; discovery that the rotation of Venus is in resonance with the Earth; confirmation of continental drift; and the synthesis of enzymes.

The nature of such unforeseen developments, not only in science but in other fields as well, is the subject of this chapter. Can the nature of such developments be described *a priori*, in general terms at least? Can methods be invented to reduce their number? Can planning respond to unprecedented events? I am indebted to Mitroff and Turoff for pointing out a parable of Carl Sandburg's which seems appropriate to this line of inquiry:[2]

> The white man drew a small circle in the sand and told the red man: "This is what the Indian knows," and drawing a big circle around the small one, "This is what the white man knows." The Indian took the stick and swept an immense ring around both circles. "This is where the white man and the red man know nothing."

This chapter presents a first, tentative exploration of the space between what we think we know and the outer boundary defined by that immense ring.

Some Definitions

An *unforseen development* is an event which in retrospect is seen to have been important but which has been omitted from forecasts.

The relative number of unforeseen developments is a function of the time between the present and the forecast horizon. As the forecast time horizon approaches the present, information on which to base forecasts improves, forecast accuracy improves, and the relative number of unforeseen developments diminishes. This observation is probably true even for "random" events. For example, a lightning bolt, before it strikes, is manifested by the buildup of a static charge; terrestrial strains probably can be associated with earthquakes; whether or not the bullet will hit the target can change from a problem in statistics to ballistics after the bullet is fired.

While it is probably true that almost all unforeseen events can be forecasted immediately before they occur, the problem is to accomplish the forecast in time for action. Suppose that a man built an electric charge detector that could forecast the stroke of a lightning bolt. If its warning time were only a second, evasive action would not be possible and, depsite being "foreseen," the forecast of lightning would be useless. Therefore, moving this forecast from the "unforeseen" to the "foreseen" had little practical value.

There appear to be at least two types of unforeseen developments: we call them extrapolative and mutant. An *extrapolative unforeseen development* is one which conceptually could have been forecasted given adequate historical data, expertise, or appropriate simulation models. On the other hand, a *mutant unforeseen development* is one which could not have been forecasted; history simply held no clues to its emergence; simulation models, no matter how ingenious, would not have hinted at these developments. Such developments are discontinuities, breaks with the past. To put it in other terms, most developments are steps in long chains of casuality, and conceptually, at least, future events in the set can be inferred from past vectors of change. But at times, developments occur which are not only unanticipated but which could not have been anticipated. These are mutant.

The purpose of this chapter is to begin to explore the nature of the unknowable, the unexpected, and the unprecedented—concepts which hinge on the nature of causality - and what links one kind of development to another.

Causality

If we have enough knowledge of present and past events, will we, in concept at least, be able to specify future events? Are all events rooted in some antecedent events? Is the cause of every event a preceding event without which the event in question would not have occurred? Many philosophers have argued the point; from Aristotle, who assumed that causes produce effects, to Descartes, who called cause substance; and to Mill, who justified belief in universal causation by tracking it back to an ultimate causation.

But many philosophers deny the validity of universal causality. Hume found cause only a fiction of the mind. Bergson believes that ultimate reality is not bound to exact causal sequences; some elements of growth are uncaused and, he holds, therefore are unpredictable. The point of view accepted in this paper is that cause and effect relationships exist most of the time but, occasionally, spontaneous uncaused events emerge.

Extrapolation in Forecasting

Most forecasted developments consist of statements about future conditions which grow out of historical and contemporary activities. For example, most forecasts of scientific achievements anticipate the consequence of research in progress or planned research; economic forecasts deal with the momentum of economic change, and technological forecasts relate to the adaptation, perfection, or diffusion of existing or emerging capabilities.

In a real sense, then, most forecasts are extrapolations, be they derived from extensions of historical trends, expert opinion, or simulation models. Some forecasting techniques are clearly labeled as extrapolative procedures. For example, if time-series data are obtained about these various technological or societal measures (such as peak aircraft speed, population density in urban cores, life expectancy, or engine efficiency), forecasts can be produced by extending the historical trends into the future using well-known curve-fitting approaches. But most other forecasting techniques have strong extrapolative elements or are completely extrapolative as well.

To be somewhat more specific, suppose a group of executives is interacting through Delphi and addressing a subject such as, say, external political changes likely to affect their corporation. They would almost certainly list changes currently in existence, at least in embryonic form, and debate their future course. It would be unreasonable to expect the executives through any process to invent tomorrow's analog of consumerism or antitrust; they would merely carry such current trends to conclusions that seemed reasonable to them at the time. An econometrician attempting to describe the relationship between energy availability and unemployment would probably utilize linear programing, multiple regression, or input-output techniques to formulate his model; all would rely to a greater or lesser extent on the past relationships among the variables. Econometricians would find it difficult, if not impossible, to include credibly in such analyses developments which if they occurred would greatly distort the economic system being described, including, for example, drastic fuel rationing, vastly higher fuel prices, or even free energy.

As another example, even Forrester's system dynamics is a case of extrapolation: here it is assumed that a chosen systemic structure remains fixed with time and that the variables included in the model interact according to rules that can be defined through analysis of historical data. The forecasts produced presume the structure remains unchanged and the variables continue to relate to one another as they have in the past. Of course, future developments can be tested one at a time (the model is simply rerun with the new development), but it is usually presumed that such developments act within the old structure, and the result is still extrapolation but under different initial assumptions.

The business of most forecasters most of the time is extrapolation. Extrapolation requires allegiance to the notion that forces at work in the past will continue to be at work in the future. This assumption is bound to be wrong eventually.

Extrapolative Unforeseen Developments

Why would developments rooted in history which are later recognized as important be omitted from forecasting studies when in concept at least they could have been anticipated? Assume first that the prospective development is in hand during the forecasting study and a decision is being made about whether to include it. A decision matrix could be constructed as follows:

		Development is thought to be:	
		Important	Trivial
Probability of the development is perceived as:	High	Included	Excluded
	Low	Might be included	Excluded

Only developments seen to be both important and highly probable would certainly be included. If the development were seen as trivial it would be excluded, independent of its probability; if it were seen as having low probability it might or might not be excluded, independent of its importance. So the reasons for extrapolative omissions can be listed as follows:

1. A development recognized as prospectively important but having low probability turns out to occur anyway because the probability judgment was wrong or because, as sometimes must happen, the unexpected development occurred.
2. A development thought to be trivial has, through an unanticipated chain of causality or coincidence, unexpected consequences.

An example of the former might be the embargo of petroleum by the OPEC states in an attempt to influence Middle East politics; many of the consequences of such an event could have been forecasted, but the probability of such an event would have been judged vanishingly small a year before it occurred. Therefore, it had been discounted in most energy studies. An example of the latter might be the Gulf of Tonkin Resolution, which appeared innocuous enough at the time it was passed but had, as is now clear, many unexpected consequences.

This line of reasoning began with the assumption that the extrapolative developments were in hand and were simply being reviewed for inclusion or exclusion on the basis of importance and probability. In the real world, the set of extrapolative developments is often quite incomplete. Omitted developments, later recognized as important and stemming from historical events and trends, may not have been considered at all. Why? The answer may be failures of imagination and nerve, inadequate expertise, insufficient data, inappropriate models underlying the data collection or organization processes—any or all of these.

Mutant Unforeseen Developments

Mutant unforeseen events are unprecedented. This definition recognizes that the future will not only evolve along smooth paths but will also be subject to mutations—unexpected departures from expected trends—which, once having occurred, leave the world different for all time. Robert Prehoda, in his book, *Designing the Future,* calls these key steps "Hahn-Strassmann" points, after the researchers who showed in 1938 that atomic chain reactions were possible. Before them, forecasts about atomic energy probably involved little more than informed guesses but, after them, forecasts about atomic energy had a scientific basis.[4]

This represents one sort of mutant event: the *spontaneous mutant.* In this class are discoveries of new heavenly bodies, new physical phenomena, presidential assassinations, physiologic mutations, and so-called acts of God. There is at least one other sort of mutant event as well: the *coincident mutant* which, while forecastable perhaps through extrapolative methods, draws its importance from other, noncasually linked events which occur at the same time. The coincidence involved here is not serial, as is sometimes meant in statistics, five heads in a row, but a fortuitous time-phasing which places diverse events and developments of apparently different origin in justaposition. For example, the chance meeting of two people who later marry, or the availability of the transistor at a time when guidance system miniaturization held the key to viable ballistic missile development. The transistor came into being without reference to guided missiles, and the guided missile, without reference to the transistor; together they were synergistic.

It seems to me these definitions describe the types of unforeseen developments that we encounter. Now the questions are these: How can our methods be amended to permit the *a priori* identification of events which would have otherwise been unforeseen? How can we forecast their consequences? I will mention four approaches to the problem of reducing the population of unforeseen events:

1. A paradigmatic technique.
2. Normative search.
3. Morphological analysis.
4. Consequences of new instrumentation.

In addition, I will mention two new forecasting techniques under development at The Futures Group that may prove useful in forecasting the consequences of unprecedented events: Trend Impact Analysis (TIA) and Probabilistic System Dynamics (PSD).

Clearly there are many other techniques being developed elsewhere which could also fit under this rubric: Dynamo gaming and cross-impact simulation, to mention two. I have singled out TIA and PSD because they may be new to the reader.

A Paradigmatic Technique

Thomas Kuhn, in his brilliant book, *The Structure of Scientific Revolutions*,[5] studied the flow of scientific history. He found it not a continuous stream, each new idea adding its energy to ever-widening disciplines, but rather a discontinuous series of crises. In his image, the business of "normal science" is the probing of the limits of the accepted paradigm, the laws and theories that comprise the content of the disciplines. Most scientists do this kind of work most of the time, adding precision to known consistencies and testing the applicability of accepted concepts in new environments. Belief in these paradigms is a requisite of the guild, students are taught these, and when they know them by rote, they are graduated.

But occasionally old beliefs fail in new circumstances. Phlogiston, phrenology, and caloric are among the debris of advancing science. Kuhn calls the trauma which results in the rejection of accepted theories and their replacement with others "the crises of science." Crises are times of trial. Old ideas cannot be lightly discarded; careers are built on them, and reputations sometimes rest on their continued acceptance. When old ideas are discarded, part of the firmament disappears; the textbooks are wrong; honored professors, false. Every premise of the discipline must be reexamined and the pieces put back together with some semblence of order, hopefully greater than that which existed before.[6]

Of course Kuhn's model fits many other institutions. A school functions according to its set of written and unwritten rules; these are its paradigms. Corporations pursue their ends until changing profitability, shifting markets, or social intervention indicate that the old rules of operation are no longer functional.

If Kuhn is right, his model provides us with a means for helping to identify the unexpected: *search for the crises*. Where disciplines or organizations are in turmoil, where old paradigms are being questioned, new ideas will be acceptable. The deeper the crisis, the more likely that the new idea will be mutant rather than extrapolative. Are there Kuhnian crises today? A few. Psychology is trying hard to ignore the anecdotal literature derived from the field of parapsychology (but less so recently). Quasars are anomalous within the accepted concepts of cosmology.

Normative Search

Unexpected events, either mutant or extrapolative, might be discovered in forecasting studies by simply asking, "What is it that ought to happen?" This approach assumes a rationality in science or in other institutions which may or may not be present. At the very least, the question serves as a point of departure for stimulating imaginations to project forecasted events which might otherwise be omitted. Suppose we were forecasting in the field of international politics. What ought to happen? For example, resolution of the

Middle East conflict, establishment of a worldwide granary, reduction of the rate of population growth in developing countries, or establishment of an international currency which promotes monetary stability.

Morphological Analysis

The normative search asks what ought to happen. A morphological analysis asks: What is the full range of what *can* happen? Zwicky's approach is complex to use; basically it consists of first identifying the elements of a system and then stating all of the alternative methods by which the function of the subsystem could be accomplished. Combinations of each of the methods are formed by permuted entries in order to identify all of the ways that the system can be made to function. Before any combinations are discarded the question is asked: "Why won't it work?" In concept at least, this is a regimen which leads to discovery. In practice there are important limitations: How can one be sure that any list of alternatives is exhaustive? The definition of subsystems in itself forms a conceptual boundary. Nevertheless, the approach has probably not been used too frequently.

Consequences of New Instrumentation

A comparison of past forecasting studies with actual events shows that at least some of the unexpected events came about as a result of the invention of new instrumentation. If one had been able to project the new instrument, the event which followed might have been foreseen as well. For example, the discovery of pulsars was not forecasted in the 1964 Delphi study mentioned earlier. Yet radio telescopes existed at that time. The sensitivity of radio telescopes improved shortly after 1964 and, as a result, signal integration time could be reduced from the 20 seconds or so which was used in the mid-1960's to less than a second. Once the integration time had been reduced, radio sources with periodicity on the order of a second could be detected. Can we now ask what might be discovered as a result of improving some other instruments, through improving resolution and accuracy and diminishing the noise present in other regimes? For example, the electron microscope, the mass spectrometer, I.Q. tests, and electroencephalographs.

Trend Impact Analysis

Trend impact analysis (TIA), an analytic procedure developed by The Futures Group,[7] divides the task of extrapolation in such a way that humans and computers are assigned precisely the task that each does best. First, the computer extrapolates the past history of a trend. Second, an expert or group of experts specifies a set of unprecedented future events and how the extrapolation would be changed by the occurrence of these events. The computer then uses these judgments about impact to modify the trend extrapolation.

The impact judgments are specified in terms of five parameters:

1. Time from the occurrence of the event to first response of the variable.
2. Time from the occurrence of the event to maximum deviation of the variable.
3. Time from the occurrence of the event to steady state value of the variable.
4. Peak deviation of the curve from the extrapolation.
5. Steady state deviation of the curve from the extrapolation.

The heart of TIA is the computer program for using these judgments to calculate the expected impact of the selected events on the extrapolated trend. A closed-form procedure is used to solve this problem. The expected value, or mean, of the impact and upper and lower quartiles of the distribution of possible impacts are computed for each indicator.

The expected value of the impact is computed by summing the products of the probabilities of the impacting events for each possible year times the magnitude of their impact, taking into account their specified lags (see Figure 1). Probabilities of events for years not specified in our files are estimated by linear interpolation, assuming that an event has 0.00 probability at the present time. Similarly, impacts are linearly interpolated between three specified impact magnitudes. Typical TIA runs and input data are shown in Figures 2, 3, and 4.

Thus this technique permits the forecaster to break, finally, the assumption that past trends must continue. It permits explicit consideration of the effect of unprecedented events on future trends.

Probabilistic System Dynamics

The Limits to Growth study of Meadows[8] has stimulated a lively debate not only about its primary substantive findings (exponential growth of both population and capital investment cannot continue for

Year of event occurrence					
1979	–	–	–	–	$P_{79}XI_0$
1978	–	–	–	$P_{78}XI_0$	$P_{78}XI_1$
1977	–	–	$P_{77}XI_0$	$P_{77}XI_1$	$P_{77}XI_2$
1976	–	$P_{76}XI_0$	$P_{76}XI_1$	$P_{76}XI_2$	$P_{76}XI_3$
1975	$P_{75}XI_0$	$P_{75}XI_1$	$P_{75}XI_2$	$P_{75}XI_3$	$P_{75}XI_4$
	1975	1976	1977	1978	1979

P_X = Probability of occurrence in year X
I_Y = Impact of even Y years from occurrence of the event
$I_{TOTAL\,Y} = \Sigma I_{E_{1Y}} + I_{E_{1Y}} + \ldots I_{E_{iY}}$

– Assumes coupling among events and event impacts is negligible

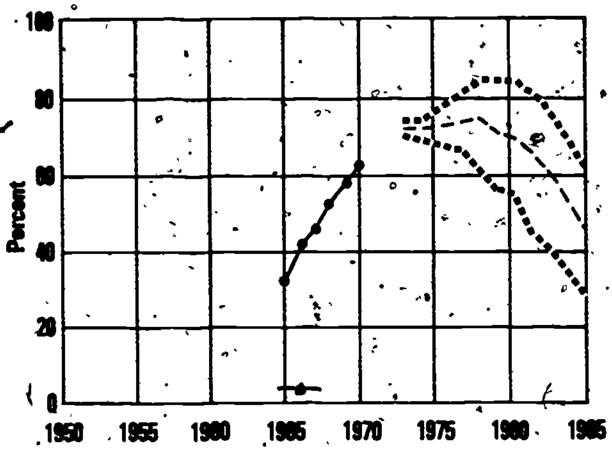
Figure 1. Expected value of an event impact.

long without serious consequences, a conclusion which can hardly be disputed), but about the method of analysis itself. Among the major criticisms of the modeling technique are the following:

1. The system under study is described as a closed system. Few, if any, outside factors influence the outcome of the model. In all but the world model, these exogenous influences may, with time, become the most important factors affecting the behavior of the system.
2. The model structure remains fixed during the entire time under study. Though specific values of variables will change with time, the equations describing the relationships among model variables will not change.
3. Specific events which may or may not occur but which, if they were to occur, would affect the model are not taken into account.

Despite these and other shortcomings, the model has great power. Its construction requires that the modelers reach some fundamental understanding of the system and that the intricacies among system elements be probed. The consequences of certain policies can be analyzed (e.g., "If agricultural produc-

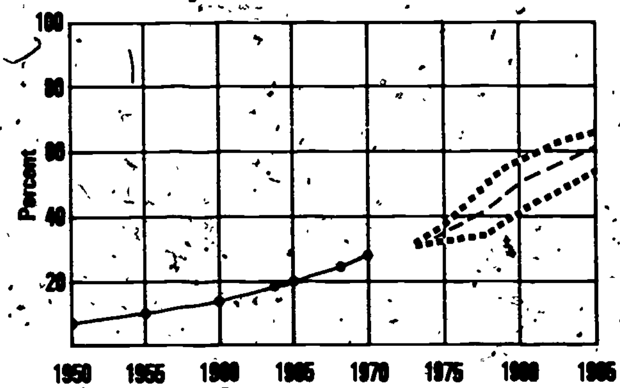
Percentage of new drug applications rejected by the U.S. Food and Drug Administration



Extrapolation inputs: 1975 = 75.0
1980 = 73.0
1985 = 58.0

Source of historical data: U.S. Food and Drug Administration *FDA Papers*, various issues

Percentage of pharmaceutical manufacturers sales to hospitals, clinics, and laboratories



Extrapolation inputs: 1975 = 33.8
1980 = 40.0
1985 = 45.0

Source of historical data: Drug News Weekly, *The Drug Market — USA*, various issues

Figure 2. Typical forecasts obtained by using trend impact analysis (TIA).

Scout file number	Forecast	Source Probability	year	Years to first impact	Years to maximum impact	Maximum impact (percent)	Years to steady state impact	Steady state impact (percent)	Forecast source code
021277	Single-unit drug packaging accounts for at least 50% of drug products sales.	.99	1975	1	10	-5	10	-5	769
020985	Enactment of federal legislation regulating prices of health and drug products.	.25	1975	2	3	5	3	5	3226
020989	Development of a national drug formulary by the FDA.	.50	1978	2	5	5	5	5	3226
009310	Establishment of regional centers for clinical examination and centralization and standardization of clinical examination data.	.25	1978	1	7	5	7	5	213
020988	Enactment of federal legislation requiring generic labeling.	.50	1978	1	2	7	2	7	3226
016245	The majority of U.S. hospitals are associated with a health maintenance organization system.	.50	1982	1	3	10	3	10	294
016244	Enrollment by 90% of the U.S. population in health maintenance organizations which emphasize preventive rather than remedial medicine and provide a broad range of medical services to subscribers for a fixed contract fee paid in advance.	.10	1975	1	7	15	10	12	215
020987	The three top drug manufacturers initiate a policy of direct sales to hospitals, clinics, and laboratories.	.70	1978	1	3	20	3	20	3226

Figure 3. Potential future events used to forecast percentage of pharmaceutical manufacturers sales to hospitals, clinics, and laboratories.

tivity were doubled in the absence of population control, how long would large-scale starvation be delayed?"), and occasionally instructive, sometimes counterintuitive, results can be obtained.

Probabilistic system dynamics is a new modeling technique designed to overcome the criticisms mentioned above by combining the modeling procedure of system dynamics with the event handling techniques of cross-impact analysis. A set of events, either exogenous or endogenous to the area of study, impact the model according to their individual time dependent probabilities. These impacts may take the form of changes in parameter values, changes in the model equations, or changes in the actual structure of the model.

The system dynamics model is developed using the information about variable relationships supplied by a panel of experts and the information gathered from the appropriate literature by The Futures Group staff. A model is developed that shows the dynamic behavior of the system being studied. The model's validity is tested by using initial data for some past date, for example, 1950, and having the model calculate variable values up to the present day. Comparisons of these calculated values with actual historical data can be used to assess the success of the model in simulating the system under study. All of this is rather standard.

A cross-impact matrix is then formed of events which, if they occurred, would affect the structure of the system being modeled or the relationship among variables which describe it. These are events which in general are not represented in the historical model. They are unprecedented.

The impacts of these events on the model are described. These impacts may come from expert judgment in cases where the impact is uncertain or they may be calculated in cases where the impact is obvious and not judgmental. These impacts on the model may

Scout file number	Forecast	Source probability year	Years to first impact	Years to maximum impact	Maximum impact (percent)	Years to steady state impact	Steady state impact (percent)	Forecast source code
021564	FDA requires complete evaluation of drugs by testing on animals prior to any human testing.	.35 1980	0	1	-20	10	-10	3230
021563	Removal of the "grandfather status" for pre-1938 drugs, such as digitalis, paragoric, aspirin, opium, and morphine.	.62 1980	0	5	-15	20	0	3230
021567	Government sponsorship of regional pharmacological centers which provide training through student participation and also accomplish evaluation of drugs to obtain data for the FDA.	.15 1980	1	5	-10	10	-5	3230
021278	Understanding of the mechanisms involved in sleep, including the functions it serves, the biological processes which take place, and means of inducing sleep.	.80 1978	5	10	-10	20	-5	3227
020983	Availability of a cure or treatment for cancer based on drug therapy or genetic engineering rather than radiation treatment.	.30 1980	10	12	-5	16	0	3230
020966	Implementation of a comprehensive federal health insurance program covering drugs and services for all medical and dental care.	.35 1975	1	5	5	10	2	254
021572	Marked diversification of the pharmaceutical industry from its 1972 product lines.	.90 1980	0	5	5	5	5	3231

Figure 4. Potential future events used to forecast percentage of new drug applications rejected by the FDA.

take several forms. The actual structure of the model may be changed, coefficients of the equations may vary, new terms may be added or subtracted from equations, or the table functions linking variables may be affected.

Finally, in our approach the model variables also feed back to the event probabilities so that, for example, if GNP increases in an economic model, events dependent on federal funding would be likely to increase in probability. Thus four new loops are introduced:

1. Event to event cross impact.
2. Event to structure.
3. Event to table function.
4. Model levels to event probability.

The main advantages of probabilistic system dynamics are as follows:

1. The inclusion of events in the model allows occurrences outside the area of focus of the basic model to be taken into account in the model predictions. In this manner, any number of exogenous events may be included and the scope of the model is no longer limited to the closed system defined by the model boundaries.
2. The structure of the model itself becomes dynamic. Relationships among variables and even among sectors of the model may change with time as the impacts from other parts of the model and from events are included.
3. Policies can be tested in terms of their effects on the relationships among model variables, model structures, or event probabilities. Policies that have their main impact on areas outside the basic model can still be tested for impact through the effect of those policies on the exogenous events. Thus a much more

complete description of the side effects of policy decisions is possible.

Some research suggestions that flow from this line of thinking are given here:

1. The need for a continuing comparison between forecasts and occurrences, with particular emphasis on a search for reasons for omissions.
2. An imaginative search for crises within scientific disciplines and institutions and, when found, a second search for ideas in test.
3. More work on the development of morphological analysis.
4. Why not a study in one or two disciplines of what new instrumentation might mean?

Almost everyone in futures research feels that the methods which constitute the crystal ball are cloudy. The cloud represents not only low or uncertain accuracy and validity, but also an obscuring of what may ultimately be seen to be important events. I urge that our research agenda include not only projects directed toward improved accuracy and reliability, but also toward diminishing the scope of the unknown.

FOOTNOTES

[1] Theodore J. Gordon and Olaf Helmer, *Report on a Long-Range Forecasting Study*, Paper P-2982 (Santa Monica, Calif., The Rand Corporation, 1964).

[2] Ian I. Mitroff and Murray Turoff, "Technological Forecasting and Assessment Science and/or Mythology?" *Technological Forecasting and Social Change*, Vol. 5, No. 2 (1973), pp. 113-134.

[3] Carl Sandburg, "The People, Yes," *Chief Modern Poets of England and America*, Gerald DeWitt Sanders and John Herbert Nelson (eds.) (New York: Macmillan & Company, 1936).

[4] Robert W. Prehoda, *Designing the Future: The Role of Technological Forecasting* (Philadelphia, Pa.: Chilton, 1967), p. 15.

[5] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, Ill., University of Chicago Press, 1962).

[6] Harry Harrison and Theodore J. Gordon (eds.), *Ahead of Time* (Garden City, N.Y.: Doubleday, 1972).

[7] The basic concept of TIA was developed principally by Theodore J. Gordon, Herbert Gerjuoy, and Harold Becker. Significant contributions were also made by Wayne I. Boucher, as well as by two consultants to The Futures Group, D. Holmes and J. Russman.

[8] Donella H. Meadows, et al., *The Limits to Growth* (New York, Universe Books, 1972).

PART II
ACCOMPLISHMENTS AND PROSPECTS

Chapter 5

Attitudes Toward Forecasting in Political Science and Sociology: A Comment on Social Forecasting

Wayne I. Boucher

Introduction

As argued in several other chapters in this book, the futures movement and the futures researchers within it function typically in considerable ignorance of theories, methods, results, standards, and institutions which could support their work. The field has several invisible colleges; it lacks an invisible university. The field lacks a professional society (at least in most countries, including the United States), and it lacks specified and generally accepted professional standards. Moreover, while training programs and academic courses abound and the field has a few good journals and newsletters, it still lacks a satisfactory means to exploit past and current experience with forecasting in established individual disciplines. These and many other related shortcomings which might be mentioned combine to create a peculiarity in the professional atmosphere that is anything but healthy in long-term perspective.

The peculiarity has several manifestations, three of the more important of which are these: First, organizations in the field tend to take on a style that is shaped almost entirely by the academic background and work experience of their founders. For example, engineers are inclined to view problems as engineering problems; researchers who have lived for twenty years with a particular set of techniques are inclined to favor those techniques in their current work. The inevitable result, of course, is that individual futures research organizations are biased in one direction or another, and the field as a whole grows less, not more, unified, despite lip service to the outlines of the same philosophy. Second, the practitioners in the field who simply follow their own lights are necessarily burning out the bulbs—that is, exhausting their intellectual capital. Without continual study of work completed elsewhere (where "study" means extensive and critical reading in sources ranging beyond the *New York Times, Fortune*, or *Newsweek*), these individuals and those who are influenced by their example must become increasingly superficial. In effect, the size of the black boxes they deal with grows larger, not smaller. Third, and most significant perhaps, the idiosyncracies and, especially, the lack of intellectual awareness that come to be associated with particular institutions and practitioners tend to weaken, not strengthen, the credibility of individual studies, thereby lessening the chances that the results will ever be used, no matter what their intrinsic quality. Worse yet, the naivete and resultant failings of even a single organization are often taken by potential users as evidence of debility (or worse) in futures research at large. In the long run, unfortunately, these difficulties can be expected to grow, assuming that no changes are made within the field.

In any event, it is certainly true today that organizations practicing futures research are inclined to improvise, reinvent, or rediscover much more often than is necessary. This may be unavoidable in a new discipline, but it is fair to ask when "new" is no longer new. Most institutions have hardly begun to devote the resources that will be required to establish the inter- and intra-organizational capabilities and the supraorganizational instruments that would alter this situation.[1]

One of the key steps that needs to be taken is to develop means whereby futures researchers can become familiar with state-of-the-art developments in the theory or practice of forecasting in the individual disciplines most relevant to their concerns. (A very promising corollary of this recommendation is that state-of-the-art practitioners in these disciplines would become involved in futures research.) This is not to suggest that all is well with forecasting in these disciplines, because most assuredly it is not. Nevertheless, few problems of serious current interest in futures research have not already been explored in one or more of these disciplines; frequently these problems have been analyzed in great depth, and well-defined answers—though not necessarily final answers—are now in hand. In addition, much of the futures movement itself is headquartered in academia, and in some departments important new research is being conducted that pertains directly to possible futures of futures research.

This chapter briefly reviews attitudes and approaches to forecasting in two disciplines that clearly have a close relationship to futures research. The purposes of the chapter are, first, to demonstrate that these disciplines and futures research have indeed much to learn from each other and, second, to indicate here and there certain points of common interest where one might immediately help the other. It is worth emphasizing that the focus in this chapter falls on attitudes and approaches rather than on substantive results or methods—both of which deserve more extensive treatment than is possible here.

We begin with political science and with a question in political science that is also the most important question facing futures research: the possibility of forecasting war. We then turn to sociology. Finally, we attempt to generalize from these reviews to assess the overall state-of-the-art in social forecasting, both as practiced in the established disciplines and as practiced in futures research.

Political Science

Perhaps the most audacious prediction of the twentieth century—certainly the prediction that has had the greatest tangible impact on the lives of people and civilization in our times—is one that falls in the domain of social forecasting. It provides a very instructive case with which to open this section on attitudes toward forecasting in political science, though the lessons it seems to teach apply throughout the whole domain of social forecasting.

The prediction was made on the morning of September 5, 1934, by a very determined and confident politician named Adolf Hitler:

> The German form of life is difinitely determined for the next thousand years. . . .There will be no other revolution in Germany for the next one thousand years![2]

What is remarkable about this statement is not that the Third Reich fell short of Hitler's expectation by 987 years and eight months, but that contemporary independent observers failed so completely to appreciate its ultimate significance. On its face, it could be taken merely as a boast.[3] Less superficially, it could be viewed as a confirmation that the regime had in fact fully consolidated its power. But at this point both friends and enemies in and outside of Germany knew so; the ruthless purge of June 30, 1934, the published telegram of approval of the purge signed by Hindenburg, the combining of the offices of Reich Chancellor and Reich President upon Hindenberg's death on August 2, the military's personal oath on the same day to Hitler rather than to the Constitution or the State, and the rubber-stamp plebiscite of August 19 all made it plain.

When then is its meaning? In retrospect, at least, its profound but unnoticed implication was that since the regime was finally and indisputably in control at home, the *foreign* policies of *Mein Kampf* were now operational. Total domestic control was the essential precondition.[4] Once it has been achieved, these policies could and would be pursued, among them principally the interlocking objectives of creating the Greater German Reich and securing needed *lebensraum*. But because *Mein Kampf*, plus dozens of speeches before and after its publication, left little doubt as to the meaning of these goals or the methods that would be used to attain them,[5] Hitler's prediction can be viewed as clear notice to the world that war was not merely possible but, given the existing political situation, a virtual certainty. In other words, Hitler's statement that morning amounted to a major public declaration that Germany would eventually wage the necessary war or wars—and indeed would win them so overwhelmingly that he and his successors would not be threatened for the next 1,000 years. Because we now have, he said, "the power to do everything," Germany's future actions "cannot be limited by anything, except through impulses of a tactical, personal, and hence temporary nature."[6]

Quite possibly such an interpretation can be sustained only in retrospect, especially if we insist, narrowly, on assigning a kind of magical significance to the date of September 5, 1934.[7] (We now know, of course, that Hitler himself considered war inevitable at this time—in fact, long before this time—though until the spring of 1939 he consistently hesitated to pinpoint the date.[8]) But it is rather interesting to observe how difficult it is to find a theory or an analysis from the early 1930's that tries to argue how such a "crazy state," to use Dror's phrase, could possibly be expected to continue in existence for even 20 years, still less 1,000 years, without war.

But apart from the existence of elaborate theories or an awareness of Hitler's private intentions, the more general question naturally arises whether it was possible for any outside observer to have forecasted the coming war, particularly before the string of foreign policy actions that began on March 16, 1935, with Hitler's announcement of the introduction of compulsory military service and the creation of an air force—"the first overt blow against the principles of Versailles."[9] A look at the literature of political science during that period is completely disheartening; for example, from 1930 through the end of 1939 the *Political Science Quarterly* did not publish a single article attempting to forecast the war. Some of the more distinguished books of the period—notably Carr's *The Twenty Year's Crisis*—"rigorously. . .eschewed prophecy."[10] Even today, most social scientists, among them a few who also are critics of futures research, rule out the possibility of confidently anticipating any such awesome and unique man-made disasters. Robert Nisbet, for one, expressly points to World War II as an instance of the type of occurrence which cannot, in principle, be predicted on "genuinely scientific" grounds, and he fancies that futures researchers would agree with him in this. He has them asking:

> . . .we [futurologists] are not expected to predict maniacs like Hitler and random events such as World War II, are we? Indeed not.[11]

Despite the dismal showing of the academics in the 1930's and the reservations of current critics, the fact of the matter is that World War II was correctly forecasted again and again between 1933 and 1935 (and frequently thereafter) and that, by any standard, these forecasts were not only reasoned but, where the combatants or the possible dates of outbreak were specified, sometimes astonishingly accurate. The most interesting of these forecasts were made by journalists—which, by the way, may explain why Hitler merely dismissed the "experts" but regularly excoriated the press.[12] Others were made by politicians and have only come to light since 1940. In any case, these forecasts were made. The three examples which follow will illustrate their nature and something of the approaches used in their preparation.

One of the first dates from mid-1933; it is Sisley Huddleston's *War Unless. . .*,[13] a book that was commended by persons like H. G. Wells and Lloyd George but obviously with little effect. Huddleston's approach, as his title suggests, is entirely conditional in the sense that he does not assert the inevitability of the war, but only its inevitability if certain facts are not understood and if a series of steps, which he defines, is not taken. As such, the book provides a superb instance of a forecast consciously intended to be self-defeating. The author's method combines historical analogy and reasoned analysis of the implications of the Versailles treaty in view of (1) the continuing quest for power and security by individual nations and (2) the likely possibilities of resolving European issues through the available mechanisms, such as The League of Nations or pacts to outlaw war. His general conclusion is stated early in the book:

> . . .we should make no mistake about it. Nonrevision [of Versailles] means war—or rather if there is no peaceful revision, there will be a warlike revision. The choice is not between revision or nonrevision. It is between the two kinds of revision—by agreement or by arms.[14]

"There is," he writes, "no need of conferences, no need of machinery, no need of publicity."[15] What is needed is a closed-door meeting by England, France, Germany, and Italy to work out "a reasonable plan in the general interest":

> Either the four Great Powers will act in this fashion, and will employ their influence in favour of a final and consented settlement—or they will, or at least some of them, sponsor irresponsible plans against each other, and encourage their proteges to acts of unreason, whether in defence of the existing frontiers, or in attack on existing frontiers. Of these two things, one. Which shall it be? One undubitably spells war; the other probably spells peace.[16]

Huddleston thus correctly forecasted the event, and a detailed analysis might also show that he predicted it as well. Be that as it may, however, he did not forecast when the war would occur, though it is clear in context that he anticipated war within a few years; it would not be fair to accuse him of omitting an estimate of the date in order to avoid disconfirmation.

Both the event and a range of years of probable occurrence were anticipated in what is unquestionably a most remarkable study: Leland Stowe's *Nazi Means*

War.[17] Stowe's method was far different from Huddleston's; he characterizes it as "rational deduction" based on facts and impressions garnered in an "unremitting journalistic enquiry" during a mere two months inside Nazi Germany (September and October 1933). No sources, printed or otherwise, from outside of Germany were used. He read *Mein Kampf*; he looked; he counted military and quasi-military personnel, training programs, and demonstrations; he talked; and he listened. What he heard from Hitler's speeches was that "Germany wants nothing but peace." On the other hand, what he saw was that "psychologically, spiritually, and physically the Nazi dictatorship is designed, par excellence, to create a Nation of warriors and a civilian population trained to follow blindly in their wake"[18]—and that it was frantically doing so. With what consequence? He sums up the views of other foreign observers then inside Germany:

> [they] are virtually unanimous in the conviction that present National Socialist policies, if pursued in their present methods and toward present goals, cannot fail to precipitate another European catastrophe sooner or later.[19]

His personal conclusion is much the same, though it adds the time frame:

> The Hitler movement. . .has increased inestimably the probability of another war in Europe at any time in the next five to fifteen years. (We say five to fifteen years where many would say three years or five, but in an effort to avoid possible overstatement and to express both the most hopeful and most conservative view.). . .The choice of Hitlerite Germany, reckoned by facts, figures, and actions alone, seems fatally projected toward a final war.[20]

It is noteworthy that *after* the war had broken out, some writers used essentially the same evidence and arguments as Stowe's to demonstrate that the war was "inevitable."[21] But like Huddleston, Stowe did not believe that it was; he simply judged it very unlikely that the actions necessary to avoid it would be taken. Incidentally, he also judged it very unlikely that the United States would develop the policies and make the sacrifices required if it were to maintain its neutrality in the coming war. Hence, "the opposite alternative of eventually becoming a belligerent would appear almost unavoidable."[22]

A final example of correct anticipation—this time involving a nonprobabilistic assertion about the future—is found in a book written in 1934 by Drew Pearson and Constantine Brown.[23] Their method, if it may be called that, differs radically from that of either Huddleston or Stowe, consisting as it does principally in the accumulation of headlines, diplomatic gossip, random impressions about how political decisions are customarily made, and topical anecdotes, until the very weight of this material points to an apparently inescapable conclusion. Along the way, these authors also draw on some very striking illustrations. For example, they recount that when Germany withdrew from The League of Nations and the Disarmament Conference in October 1933 the Polish response was to stage a massive military display—to which the Germans responded by erecting a statue on the Polish frontier, on the base of which was inscribed not only a list of German cities in Poland that "await liberation" but also the motto, "Germans! Bide the hours which will expiate these bleeding frontiers!"[24] After reviewing literally hundreds of such bits and pieces of trends in motion throughout the world, their prediction is firm—and broader than the others:

> War in Europe may come in any number of ways now. It may come almost immediately. It may not come for five years. [But] war will come. [And] when it comes to Europe it will come also to the Far East.[25]

Some significance probably should be attached to the fact that the Huddlestons, the Stowes, and the Pearsons were journalists, specialists in bringing together many different kinds of information to define that always elusive "big" event. But in these specific cases something more than this capability was certainly at play; recall Randall's ironical comment on the Alsops: "the Alsops are excellent historians, though one can be quite sure that their predictions of doom will never be realized: something worse will come to pass in the meantime."[26] Hence, at a time when everyone confesses the unavoidable, central, and continuing need to rely on informed judgment in forecasting, it would certainly seem important to reexamine books like these three at length to learn if possible just how the evidence was sifted and arranged and how the conclusions were drawn. Presumably, the results of such an examination could have some generality, not only in political science but throughout the fields of social forecasting.[27]

Be that as it may, these books point to another conclusion of general importance, namely, that at least some of the developments like those Nisbet calls "random events" can in fact be forecasted. The realm of the unforeseeable is extremely large, but nevertheless smaller than social scientists appear to believe. Certain kinds of cataclysmic political events, in particular, can be forecasted, and often are. The first problem, of course, is to identify the *types*, and no work of this sort has yet been done.[28]

The ultimate problems—how forecasts are disseminated to decisionmakers and what criteria come into play before the forecasts are believed—are especially severe in the field of political science, where credibility remains a serious issue. A professor in the field writes: "I believe I am correct in saying that Political Science has the lowest professional status of any of the social sciences. It receives least outside funding for research; it is least consulted by government; and its members are least frequently employed by government—as political scientists."[29] [illegible] basic reasons are offered by this author: (1) political scientists have done little or nothing to promote the field, and (2) "we have traditionally had little interest or knowledge of [the substance of] policy matters so we have had no contributions to make of any value to policymakers."[30] These and other factors have been barriers to forecasting efforts; the lack of such efforts has prevented the building of much of a track record; the lack of a record has helped make credibility a concern.

Increasingly over the past 25 years, though not in any major way because of failures to foresee events like World War II, political science has begun to take a futures perspective, and in the last decade or so, some practitioners have begun to think out loud, albeit haltingly, in the language of futures research.[31] For example, in his useful survey of the growth and present status of quantitative methods in political science, Karl Deutsch nods in the direction of work by Daniel Bell, de Jouvenel, Delapalme, Richta, Baade, Junck, Kahn and Weiner, and others and asserts that these efforts have made "the projecting of future trends and possibilities, and the suggestion of suitable provisions to meet various contingencies... a significant field of political and social science."[32] More important, perhaps, is that in the list of needed developments in methodology with which Deutsch concludes his essay, special prominence is given to a project—rather, to a program—that reflects an aspiration of many futures researchers, at least in their free-wheeling moments: the creation of a total world model of political and social change.[33] His premise is noteworthy, especially if taken literally:

> Food, resources, population, industry, economic prosperity or poverty, social communication, political stability or discontent, immobilism or reform, war or peace, all interact with one another, and none of them can be projected or predicted to an acceptable approximation of the future without taking account of the rest.[34]

Having reviewed a great many models, including the Club of Rome endeavor, Deutsch is well aware of the enormous difficulties and risks in such an undertaking. "But it would be still worse not to try; for we are likely to need for our survival every bit of true information as to where the world is going. [Work toward a model of this sort] represents our best hope to answer on a world scale an ancient and most basic question: how men and women have made their own history in the past, and how they can do so again."[35]

As an expression of a distant objective, worth seeking by all social scientists, this idea necessarily combines the continued practice of each discipline with several large doses of hope, not only about overcoming difficulties in theory,[36] but also about how policymakers and other analysts might test and exploit the various models developed along the way. There would seem, however, to be some grounds for hope in the experience of the last decade or two and in certain more limited current activities. The Navy-sponsored interdisciplinary study of the early 1960's concerned with influence processes in dealing with strategic problems (Project Michelson) and the Air Force-sponsored study of 1963 which concerned itself, in part, with the world political environment (Project Forecast) are examples of major efforts to map some parts of the political future in order to provide a framework for policymaking. Innumerable smaller efforts, many of them involving use of techniques (like Delphi or scenario writing), which have come to be associated with futures research, also have been performed—though typically with a goal of understanding perceptions or alternatives rather than of ranking the forecasts for policy choice.[37] Some very interesting work also has been done in political science classrooms.[38] And there have been many expressions of the need for taking better account of political futures,[39] as well as possible risks as one approaches success.[40] Additionally, of course, there has been a slowly increasing number of discussions of how new approaches, especially those of futures research, might be exploited by or further developed by political scientists.[41] These lines of thought are now coming to be crystallized in a handful of state-of-the-art volumes.[42]

All of this work reflects implicitly a growing acknowledgment, however reticent, of theses offered by de Jouvenel in his brilliant paper, "Political Science and Prevision."[43] Lucidly and authoritatively, he develops the case for forecasting in political science (and, indeed, elsewhere). A summary does no justice to his presentation, but it may be useful here to quote the points he focuses on:

1. Foresight is an expertise required in the political scientist.
2. Public decisions require a variety of foresights other than that of the political scientist.
3. The political scientist is competent to appreciate priorities and consistency in policies, the details of which he is incompetent to judge.
4. The political scientist must seek to coordinate anticipations (i.e., to identify the multifarious impacts of the same forecasted development and to assess the interaction of alternative forecasted states of affairs).
5. The political scientist should be a detector of trouble to come. . .Trouble is indeed his business
6. Political foresight requires study of political behavior. . .the behavior we presently observe is not the only behavior of which the subjects observed are capable (especially as individuals).
7. The political forecaster must guess how people will come to feel. . .The dynamics of moods.
8. The political scientist should foretell the adjustments suitable to improve the adequacy of the institutional system to cope with changing circumstances.

Interestingly, all eight points tend to describe the Huddlestons, the Stowes, and the Pearsons. If the literature reviewed here is any indication, however, they are also coming to describe a new breed of political scientist. But his number is small[44] and his influence on his own discipline or on futures research has been inconsequential so far. Unfortunately, given the weight of conventional attitudes and approaches in this field, it is likely to remain so for some time to come.

Sociology

Sociology and social psychology should have a closer relationship to futures research than any other disciplines, not only because "sociology grew out of a concern with prediction,"[45] but more importantly because the subjects investigated in these fields tend to overlap more with the present and ultimate concerns of futures research than those of any others. In general, however, there has been a certain historical tension within sociology that has, until recently, inhibited any widespread acceptance of the notion that a throughgoing futures-oriented sociology was feasible. This is the old conflict, also seen elsewhere, between the desire, on one hand, to classify phenomena and build theories and, on the other, to apply these concepts and theories to practical issues. Theory-building has carried the day, certainly over the last 50 years. Berelson has summed up the situation in these words:

> The present arrangements in modern sociology largely run against sociology in action [being] done most helpfully and efficiently: the rewards are given for theoretical or technical virtuosity; the "reference group" consists of sociological colleagues, not practical administrators; "newness" of insight or method or even vocabulary is often valued more than practical results; indeed the justification for doing applied research at all is often tied to the "basic" payoff that will allegedly accrue (the so-called Robin Hood effect of taking the funds from the rich client or foundation interested in the problem and putting them to the use of the poor discipline). The weight of disciplinary opinion, I think, requires one to justify an action study not by its contribution to practical affairs but by its contribution to disciplinary ends.[46]

Similarly, after reviewing conflicts in the choice of methods in sociology and some of the institutional constraints on real-world applications in the field, James Fennessey goes on to point out a fundamental "lack of fit between our *substantive* style in sociology and the needs of policy research":

> In my opinion, one basic problem faced by sociology is that it has not faced up to the implications of "future shock" for its own work. . .it seems fair to say that the mainstream of sociology still views theory as something fixed and stable, rather than as a dynamic, fluid entity. We still see theory in more or less Platonic terms, and aim to understand "the" theory of, for example, peer group pressure in the classroom. Our experience in research along these lines should perhaps have convinced us by now that such a theory is a will-of-the-wisp [as is the idea] that a research project ideally should "test" a hypothesis, just as the natural scientists do.[47]

Something of the deeper meaning of these comments may be gleaned from Table 1, which attempts to reduce

Table 1.—Paradigms of Social Change, Classified by Major Theoretical Emphases*

Class of Theory and Typical Representative	Principal Underlying Assumptions: Regarding Stability of the Societal Organization	Principal Underlying Assumptions: Regarding the Directionality of change	Popular Metaphorical Equivalents	Empirical Testability of Central Propositions
1. Evolutionary theories (Comte, Spencer, Durkheim)	Society and social change processes are considered to be inherently stable; change occurs smoothly and slowly	All societies at all times are moving uniformly in the direction of increased adaptability through specialization; output of change is "progress"	Life cycle of biological species	Little or no possibility, usefulness lies principally in describing past changes
2. Equilibrium theories, including functionalism and systems theory (Parsons, Ogburn)	Ditto, with the exception of Ogburn's theory of cultural lag	~~Ditto, but~~ homeostatic "mechanisms" rather than destabilizing ones are the focus of research; output of change is stability	Nonbiological cybernetic systems	Little or no possibility until propositions are "brought down to a more operational level"
3. Conflict theories (Marx, Dahrendorf)	Society and social change processes are considered to be inherently unstable; existing social unity or stability is primarily a function of coercion	Same as evolutionary theories, but emphasis falls on the study of social organizations as players in a zero-sum game; output of change is instability	Groups or classes of individuals competing for economic resources or for power	Little or no possibility for major ideas; many specific forecasts by writers like Marx have been disconfirmed
4. Rise-and-fall theories (Weber, Spengler, Sorokin)	Same as evolutionary theories (for Spengler and Sorokin); same as conflict theories (for Weber)	Change lacks a uniform direction from society; output of change is varying levels of growth, followed by decline or extinction	Life cycle of individual biological units; alternation of the seasons	Little or no possibility for major ideas; some specific forecasts have been tested (Spengler explicitly rules out the legitimacy of any test of verification)

* Derived principally from material in Richard P. Applebaum, *Theories of Social Change* (Chicago: Markham Publishing Company, 1970), especially Chap. 5.

to a single page the evolution of theories of change and the weightiest calls for allegiance in sociology over the past 200 years. Noteworthy is the final column, which suggests, quite simply, that *none* of the leading ideas in these theories can be established one way or the other, and hence they have no predictive value. Indeed, says Applebaum, these theories:

> . . .offer explanations of societal change which, although in part "convincing," remain untestable. To the extent that [such] theories. . .remain general. . .apparently they will neither be proved nor disproved. Perhaps we can agree with Parsons' pessimistic prediction that when a theory of structural change is available, "the millenium for social science will have arrived. This will not come in our time, and most probably never."[48]

Sociology inclines toward being the most masochistic of all the social sciences, of course. It is difficult to find an important sociologist who does not at some point lament the "triviality," the "pettiness," the "irrelevance," the "banality," of most of the work done by sociologists.[49] At their heart, these complaints can be viewed as a reflection of fundamental doubt as to whether the effort invested in the development of taxonomies and grand theory regarding the dynamics of social change has paid off in predictive power, or ever will. Crudely put, the practical choice on this issue appears to have become one of continued incessant tinkering with broad theoretical constructs (because, at least, they can provide "cogent" explanations of the past) or the taking of some other tack, out of which may come a greater ability to forecast and, thereby, the possibility of advancing more credible claims about the discipline and the profession of sociology.

Without rejecting the old systems or theories entirely, some writers have seen a way out of this choice by urging a new understanding (and practice) of sociological prediction on the basis of a clearer definition of the concept of prediction in the social sciences *as a whole,* vis-à-vis prediction in the natural sciences. The most thoughtful case of this sort by a sociologist has been made by Richard Henshel. Two of his points, in particular, might be mentioned.[50] First, he calls attention to the fact that, aside from celestial mechanics, the spectacularly accurate "empirical predictions of physical scientists typically do not concern "natural" systems (e.g., the weather) but rather "constructed" systems (i.e., entirely man-made representations of systems like the weather), representations that are, moreover, "fashioned strictly according to the requirements of the scientist or engineer." Success with constructed systems is what makes the news; unfortunately, it also has produced an overblown impression "of the predictive capacity of the [applied] physical sciences in dealing with *unmanipulated* reality." In contrast, social science has not given comparable emphasis to its own successful predictions derived from constructed systems. Its best-known failures in forecasting have concerned future states of *natural* systems, and these errors have been large enough to undermine the entire predictive enterprise—just the reverse of what has happened in the physical sciences.[51] Second, social predictions are usually compared unfairly with those in the physical sciences, in the sense that the accuracy of predictions from constructed systems in one field is weighed against that of predictions from a natural systems in the other. For these and other reasons, Henshel concludes that prediction in the social sciences is not only worth pursuing but is reasonably well grounded, more so than most people believe.[52]

Others have admitted that unique problems surround the practice of forecasting in sociology but have argued that the best way to solve them is to get down to cases, to try and then try again. This argument has actually been made out of a simple fear that sociology might end up being unsurped:

> The difficulties of making trustworthy projections, whether by sociologists or interdisciplinary teams, will eventually be met. . .The need from here on out for making them is greater than ever before. [If] sociologists refuse to get into the act, their [value] to the Western community in helping to frame policy, will surely decline. The economists, political scientists, psychologists, computer technologists, and interdisciplinary professionals like operations research analysts, are now making such projections for business, governmental and military leaders. There is no point in professional sociologists standing completely aside.[53]

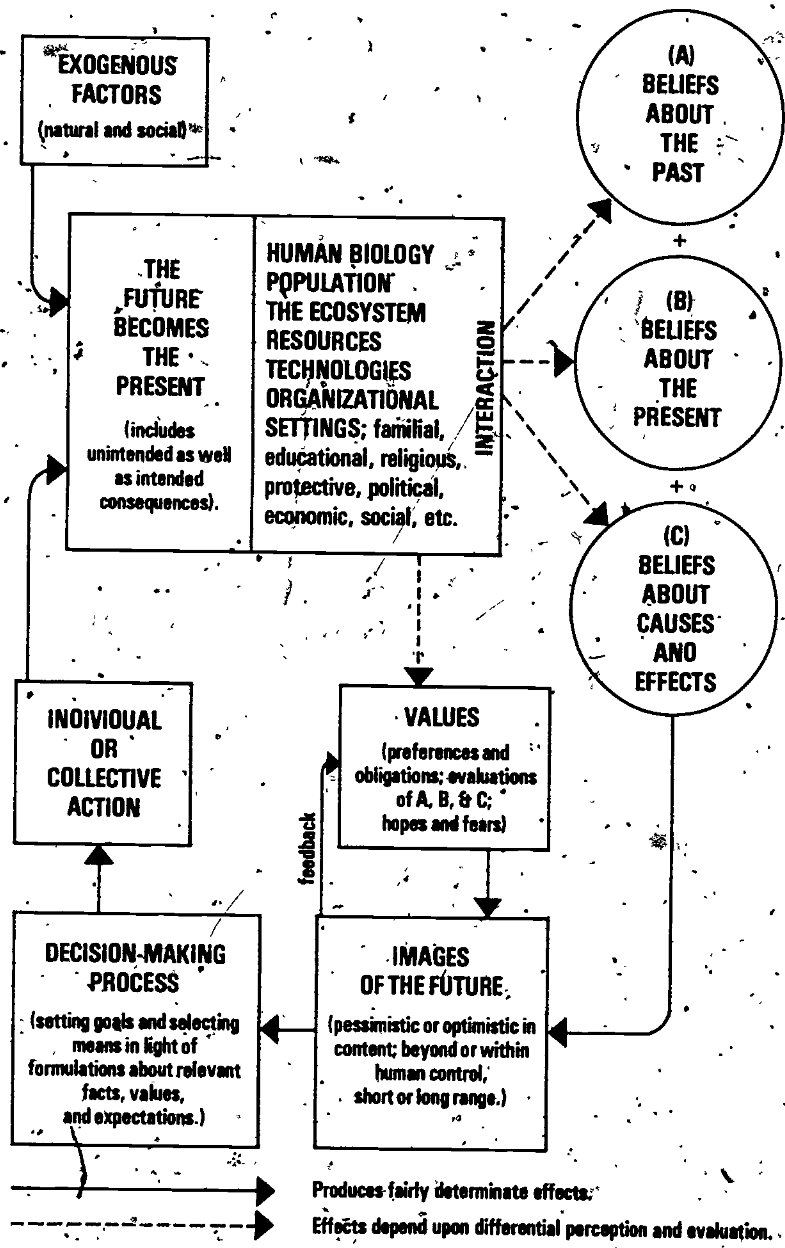
Still another approach toward futures thinking in sociology has been to urge, in effect, the creation of a new subdiscipline of "futuristics," one that builds explicitly on the tradition of Comte, Saint-Simon, Fourier, and others in the dual sense of forecasting as best one can, and then acting as if the forecasts were true. This approach, which is by far the most important from the point of view of futures research, has already found expression in a number of articles and books.[54] It has also led to the development of a futures-oriented "theory" of social change, involving the use of a "cybernetic-decisional model," the rudiments of which are illustrated in Figure 1.[55] The theory is founded on the notion that man's images of the future, all of varying probability, are central in shaping his beliefs, attitudes, and values and, in their presence, these images are utterly decisive in the choices he makes. Bell and Mau assert that development of such a theory is an appropriate task of sociologists; more generally, so are efforts to promote a new appreciation and new means of exploring the future. But for these changes in practice and understanding to occur in sociology and throughout the social sciences, some rather profound changes in attitude must also occur. Thus, Bell and Mau spell out a number of "directives" for the conduct of particular social inquiries intended to be "relevant to the future":[56]

1. The object of research should be viewed not as a static system, but as a dynamic one that takes its character through the complex interplay of past, present, and future.
2. Uncertainties should be acknowledged and, where they can be dealt with, should be handled probabilistically. Alternative futures should be considered.
3. The investigator should acknowledge that his study itself somehow affects the future.
4. Basic social values should be taken into account.
5. Attention should be paid to apparent conflicts between these social values and what is known about how society and people actually believe.
6. The investigator should have an explicit awareness of how his image of the future (and those of others) may affect the study results.
7. The possibility of basing projections on comparisons between past and present states of existing systems should be considered.
8. Theoretical or other explanatory devices used by the investigator should be formulated primarily in terms of concepts that will lend the results to specific policy options. Emphasis should be shifted from mere description to the enumeration of possibilities of control.
9. Attempts should be made where possible to test the future by carefully staging demonstration projects (i.e., design a sit-in and try it in one instance to see if it actually makes sense).
10. Use should be made of Waskow's "possidictions"-statements about future events that may have a low probability *ceteris paribus*, but can be made to happen through political action, if the events seem desirable.

These precepts, while not likely to be accepted in the short-term by many sociologists (nor indeed by all futures researchers), nevertheless reflect a very deliberate turning away from styles and principles long dominant in sociology, with their emphasis on what Mills once called the "sociological conception of fate." This is a view that makes the future the result of "specific kinds of social structure" operating in such a way that any particular historical event is usually to be viewed as the unforeseeable "summary and unintended result of innumerable decisions of innumerable men."[57]

Incidentally but surprisingly, this turning away does not seem to have involved as yet a basic recognition that sociology can claim several interesting priorities in the serious study of future. For example, the sociologist S. Column Gilfillan was not only the first of the moderns to study the accuracy of past predictions,[58] he was also apparently the first social scientist to declare that long-range, comprehensive social forecasting would be his life's work.[59] The interest in social indicators today—their derivation and use—finds its prime antecedent in William F. Ogburn's work on social trends, particularly the 1933 volumes, *Recent Social Trends in the United States*.[60] According to Gilfillan, Ogburn may also be credited as being one of the first to call for "the establishment of a predictive science."[61] Moreover, Ogburn, Gilfillan, and other collaborators can fairly be considered as having done some of the earliest and most important work on the social consequences of invention—a line of research that relates directly to today's "technology assessment."[62] Skipping over other contributions in a half-century's development of a futures orientation within sociology (some of which are mentioned elsewhere in this book), we can see in just the last decade a discipline that has been among the first to devote professional meetings (in this country and abroad) to the theme of future studies, a discipline that has shown the greatest growth in the number of courses being offered on the future;[63] a discipline that has provided a number of outstanding critics of social forecasting and futures research, among them Nisbet, Hoos, and Duncan; and, what is especially noteworthy, a discipline out of which has come the single most influential concept in contemporary futures research: Daniel Bell's postindustrial society.[64]

Without minimizing the problems of social forecasting that have long been discussed in the literature,[65] some of the sociologists who are familiar with both this futures-oriented tradition and the writings of the futurists have begun to find ways in



From Wendell Bell and James A. Mau, *The Sociology of the Future* (New York: Russell Sage Foundation, 1971), p. 21.

Figure 1. Cybernetic-decisional model of social change.

which futures research and sociology can draw on each other's experience and special strengths. Suzanne Keller, for example, suggests a number of important ways that sociology can be brought to bear in futures studies:[66]

1. In helping futures researchers to avoid the "Robinson Crusoe bias"—the undue emphasis on what "man" needs.
2. In single-cause or single key anticipations instead of patterned multiplicities.
3. In sociological concepts and information concerned with collectivities and social structures that help us to characterize gross aggregates and their modes of relating to environments.
4. In methods and measures of central tendencies.
5. In methods of measures of typical, minority, and deviant phenomena.

6. In avoiding projection from the given to eternity—if social scientists tend to speak as if there were an eternal past, futurists do likewise with the future.
7. In creating awareness of ethnocentrism: research has shown how difficult it is for individuals even when trained and intent on being unbiased, to succeed in this. Ethnocentrism marks investigations of preliterate societies, studies of the third world, and discussions of the woman question, among others. Each is seen through the special lens of males, or whites, or certain nationals. There is no reason to suppose that this will not be the case in futurism, unless [futurists] make a special effort to prevent this.
8. The ways of change: many changes in art, science, and living habits are being forecast by futurists. Few, however, pay attention to the phasing or sequence of change or by what steps and mechanisms these will be introduced. Nor do they link these changes to the ways in which social movements are generated.

Keller's view is that "the two fields have much to give to each other. Unfortunately, their paths cross all too rarely for them to do so effectively." One must wonder, then, why the paths do not cross more often. As this review may suggest, the reason does not lie in any absence of responsible, relevant, and recent work by sociologists.

The State-of-the-Art in Social Forecasting

"A 'state of the art' report on social forecasting should, in all honesty, be quite brief. Such an art, in the sense of a coherent body of percepts and practices, has not yet been developed." These are the opening sentences, and the conclusion, of an important paper written in 1969 by Otis Dudley Duncan on the state-of-the-art of social forecasting[67]—a paper that remains important because Duncan's conclusion still holds today. Many changes have, of course, occurred in social forecasting (and all other kinds of forecasting) since then, as indicated, in part, by the preceding review of developments in political science and sociology. Accordingly, a number of Duncan's specific observations are no longer accurate, or relevant or significant. To the extent that the state-of-the-art has changed, it appears, on balance, that these developments represent an advance. (For one thing, as many as 60 percent of social forecasters now seem to have learned how to spell the word "holistic.") But, generally speaking, it is clear that nothing approaching an established art, much less an established science, of social forecasting has yet come into existence. Indeed, it is probably fair to say that if someone finds himself writing a paper on the same subject 7 or 10 years from now, he may well be forced to the same conclusion, at least in so far as social forecasting in the West is concerned.

The reasons are many, and most of them are discussed in other chapters of this book. Here, then, it may be useful just to review some of the main points made in Duncan's paper, comparing the situation as he saw it in 1969 with the situation as it looks today. This should provide at least a partial basis for evaluating the current status of social forecasting as it is now being practiced in the United States, especially by those analysts we call "futures researchers" and Duncan calls "futurists." And it should also lay a foundation for attempting a short-term forecast of social forecasting.

One of Duncan's first points is that, while a great deal of social forecasting is regularly being performed implicitly by all of us, there is very little indeed that is being conducted routinely, explicitly, and continuously, relative to the level of formal efforts in economic, business, or technological forecasting. This was true then and remains true now, as pointed out in Chapter 1. Moreover, the inclinations and priorities of those who fund such work have not changed significantly over this interval. Thus, the absolute level of sustained explicit work has undoubtedly risen, but the relative mix of all kinds of forecasting probably has not changed importantly. Social forecasting per se surely remains one of the smaller categories of serious professional interest. Greater emphasis still falls on generating the kinds of forecasts that are presumed to have the greatest value in decisionmaking, such as technological or market forecasts.

Unfortunately, most people, including most forecasters, still do not know how to establish the links between social forecasting and planning and between planning and decisionmaking. Until more is known about this, and until this knowledge is more widely shared, it is likely that social forecasting will continue to come out near the bottom in relative comparisons.

The absolute growth in the number of efforts is, in any case, the significant fact, even though most of these studies have had no impact. Here and there experience is beginning to accumulate, and social forecasters are beginning to set their intellectual houses in order. Among other things, this is coming—slowly—to mean a clearer recognition of basic principles and basic limitations. For example, Joseph Coates writes that "one characteristic of holistic thinking"—he is speaking about technology assessment, a kind of social forecasting—"is that we do not know how to do it routinely; secondly, it almost certainly cannot be done routinely; and thirdly, it is not a scientific or an engineering or a disciplinary enterprise. It is essentially an art form."[68]

Duncan admits one branch of social forecasting that might properly be said to have reached the status of an art—demographic forecasting. Reviewing the results of such forecasts over the past 40-50 years gives him the opportunity to point out several pitfalls that have arisen in this area and are likely to exist in other domains of social forecasting. Among the difficulties he mentions, or quotes with approval from others, are the following six:

— First, social forecasters are inclined to be biased in their projections—or in the very choice of subjects to forecast—by their own personal, untested convictions about how the future may be expected to develop.[69]
— Second, forecasters have not been as courageous as they should be in resisting demands from clients to make forecasts in areas where they know full well that forecasts "cannot be reliably made."
— Third, forecasters have shown a tendency to become enamored of their own forecasts, gradually ignoring or forgetting all of the inevitable uncertainties or the full range of ignorance on which these forecasts rest. Worse yet, outside forces often reinforce this tendency. As Duncan puts it, "When the forecaster sees his results being adopted in many quarters and hears them coming back to him in the form of economic anticipations and the like, he can hardly resist the temptations to believe."[70]
— Fourth, forecasters have shown an inclination to be biased toward conservatism by their desire to appear scientific or "responsible." Duncan quotes one of the great pioneers of social forecasting, the sociologist William Ogburn, as saying, "try as we may to prevent wishful thinking, there is nothing so effective as plenty of data to correct it. But in cases of scarcity of data, one way of trying to reduce the error is to reduce the emotional element in thinking."[71] Yet this can lead the forecaster, as Ogburn indicates, to submit to social pressures that unduly discourage unconventional thinking.
— Fifth, forecasters exhibit a tendency not to question assumptions that have proved useful in earlier projections, even though changed conditions may have rendered them less valuable or even false. One consequence of this pitfall is that forecasters are led thereby to ignore alternative futures that may become significant. Duncan cites as an illustration the then common belief among demographers that the population would continue to rise throughout the rest of the century in this country, and he asks, with great foresight, "Should they not, at the same time, start now to state the demographic conditions under which a stationary state can be reached and chart for us some alternative time paths to that state, however unlikely [for the moment] such a prospect may seem?"[72]
— Finally, forecasters tend not only to fall in love with their own forecasts and with the assumptions on which they are based, but they also tend, at times, to focus exclusively on the elegance or mechanics of the methods they are using—especially if they invented these methods. The consequence, of course, is that they not only lose sight of what they are doing, but they become less imaginative in the process.

Now, taken together, these pitfalls tell us as much about human nature in general as they tell us about forecasters. Yet these weaknesses have characterized the practice of social forecasting in the past and they do seem to represent barriers to any serious possibility of advancing social forecasting, even if we all agree that it is "essentially an art." Moreover, anyone familiar with the state of that art today would have to admit that all of these pitfalls are still with us. If anything, they may be intensifying, as suggested at the beginning of this chapter. At bottom, the greatest threat to futures research is its tendency toward provincialism, a point that Duncan makes very gently when he introduces his readers to De Jouvenal's *Art of Conjecture* by saying that, while De Jouvenal is deft and witty and incisive, his book contains little that would not already be known to "a moderately well-read professional social scientist."

Futures researchers tend to pride themselves on having "escaped" the individual, established disciplines. Their work, they say, is "crossdisciplinary" or "multidisciplinary"—as indeed it is and must be. But they forget that the individual disciplines have much to teach, especially about the need to be modest. As Kenneth Boulding once pointed out,

> At the moment neither our theoretical structures, nor our inferences, nor our predictions, nor our perceptual apparatus and instrumentation in the

social sciences are in any way adequate to measure up to the complexity of the social system. . .We are using salt spoons to clear away snow drifts and reading glasses to study the structure of molecules.[73]

If this is true for the social sciences, how much truer it must also be for social forecasting and futures research generally. Hence the need to overcome parochialism by systematically and vigorously exploiting whatever is known in the established disciplines.

The point takes on special meaning in view of Duncan's review of the work by William Ogburn on social trends (what we would today call "social indicators) and on the social effects of invention. In discussing Ogburn's analysis of the impact of technological innovation on social change, Duncan recalls Ogburn's recommendation that such studies be conducted according to the following sequence. First, investigate possible applications of the invention and then the direct changes in individual or institutional behavior that would attend the adoption of the invention. Next, explore the effects that would follow in the wake of these direct changes. Then examine the effects of these effects, and so on, until the entire string of consequences has been identified and evaluated. This, of course, was the first expression of the essential idea behind what is now called "technology assessment."

In commenting on this idea, which he applauds in theory, Duncan fails to anticipate our present willingness to live with imperfection, and so makes a mistake. He points out, rightly, that only rarely can all aspects of a social change be traced uniquely to the influence of a single physical technology; anything the technology touches, whether it be the family or society at large, is simultaneously being affected by other forces, most of which are more important in explaining the total change. He also observes, again rightly, that the process of identifying and evaluating all of the higher-order effects of a technology will surely involve an endless and totally unassessable network of linkages. Gilfillan once said that as the analyst starts out boldly through the network, he "shortly finds that the effort is not worth making" because he soon reaches that point where the effects are simply unmanageable because their number is so large and their impacts are so attenuated. Duncan's conclusion, then, is that "quite apart from its theoretical merit. . .the concept of derivative effects of inventions cannot be a powerful tool in forecasting."[74]

In fact, as forecasting has developed over the last seven years, there are no longer many who would agree with Duncan—or who, at least, would state the conclusion with such finality. After all, systems analysis and futures research, in pure concept, implicitly deny this conclusion, and the whole technology assessment movement is explicitly premised on the notion that it is false.

To put it baldly: what this means is that we have decided to ignore the plain facts and to try to perform such analyses, even though the effort can be shown to be absurd in theory. More precisely, we have decided that the questions at stake in systems analysis, futures research, and technology assessment are too important not to warrant our experimenting with the "derivative effects" scheme in order to learn how far it can be pushed in practice and how valuable the results may be, even if limited.

At the heart of all three of these approaches—systems analysis, futures research, and technology assessment—is the notion that problems must be seen in their entirety, through time, and that it is no longer adequate to look at economic, demographic, legal, political, sociological, cultural, or technological and scientific changes in isolation from one another. As it happens, this is an issue on which some progress seems to have been made in the past few years, for at several points throughout his essay, Duncan urges his readers who are social forecasters to adopt a wider, more comprehensive perspective: "The analytic or projection model must take account of the interactions of population, environment, technology, and social organization."[75] This is what he calls "an ecological approach" to social forecasting, and it is interesting to note that he was evidently not very confident that anyone would listen to him—perhaps they did not—for at one point, he comments on the future of social forecasting by saying: "One answer to our question, 'What next?' is, therefore, more of the same."[76]

Recalling that most of today's social forecasters have now adopted an outlook similar to Duncan's "ecological approach," and taking account of the fact that some interesting and important innovations in methodology have occurred since 1969, the same question—"What next?"—can probably be answered in the same way for social forecasting today. What we see now is essentially what we will continue to see in the next five to six years. The prospect appears to include, first of all, a continued and substantial reliance on older methods of forecasting, such as scenario writing and simulation modeling, plus occasional and usually hurried efforts to improve the existing but newer methods, such as cross-impact analysis, the interview

Delphi, trend impact analysis, and probabilistic system dynamics.

To Duncan, it seemed that the methods he saw in use in 1969 were little different from those used in the 1920's and 1930's by Ogburn. He judged that the future of social forecasting would likely be jeopardized if "students of the futurist movement [failed] to analyze carefully the basic methodological questions that are raised by a concerted effort to see ahead."[77] The need remains. This book alone makes the argument, but there are other sources that do so as well.[78]

The general prospect also appears to include a continued shaking out of social forecasters who are amateurs, dilettantes, and mere enthusiasts, and a leveling off or absolute decline in the total number of organizations that practice futures research. Complementing these changes may be a corresponding rise in professional standards and professionalism. If futures researchers work at it, we may see fewer claims that this kind of forecasting is something totally new, which of course it is not. And as the number falls of persons Mumford calls the "one-generation minds" in the futures business, we should also see the beginnings of some serious historical research on the origins of futurist ideas—a need that Duncan described as "urgent" in 1969,[79] and which is no less so today.

Further, we should see a wider recognition that forecasting and futures research involve the study of the future, and not, as Duncan observed in 1969, an opportunity to assume "a rhetorical stance for discussing present social concerns."[80] We should also see less of what Duncan described as the "exaggeration" among social forecasters of "the extent to which futurist inquiry is in fact governed by explicit and communicable methods."[81] In short, we should see a greater modesty accompanied by a greater honesty.

Duncan's paper ends with a final overall "forecast of [social] forecasting." His last paragraph reads: "There will be no pretense that we can gradually move toward the perfection of methods of anticipating what will actually occur, for such perfectibility is not logically possible, esthetically appealing, or morally inspiring. What we may hope to improve, if not perfect, is our sense of responsibility for making known the implications of our knowledge."[82] The years since 1969 have belied both of these statements. Social forecasters have not discernibly improved their sense of responsibility for making known the implications of their knowledge. If there actually is room for improvement here, we might at least still continue to hope. Additionally, social forecasters have not, as far as one can tell, given up their goal—or is it really a "pretense"?—of gradually moving toward a perfection of methods of anticipating what will actually occur. Nor, by the same token, have they given up the belief that the entire art of social forecasting can be improved over time, just as Ogburn and Gilfillan and many others have argued for 50 years and more.[83] It seems clear now that no futures researcher will abandon these aspirations. Equally clear, however, is that mere wishful thinking will no longer be a satisfactory guide to the internal development of the field. The individual disciplines need to be studied carefully, exploited, and then, perhaps, discarded—*after* the case has been made that true cross-disciplinary social forecasting is a genuine possibility.

FOOTNOTES

[1] Needless to say, unless government or the foundations underwrite the costs, these resources can only be derived from fees earned on contracts, grants, or products. If fees alone are the source, major changes can come only very slowly.

[2] Proclamation at the 1934 NSDAP Congress in Nurenberg, as quoted in William L. Shirer, *The Rise and Fall of the Third Reich* (New York: Simon and Schuster, 1960), p. 230.

[3] An on-the-spot evaluation was provided by Shirer, who breezily dismissed it as simply one of many "lies," and who wondered why "such statements [could be] widly applauded as if they were new truths." See William L. Shirer, *Berlin Diary* (New York: Alfred A. Knopf, 1941), p. 19.

[4] Cf Karl Lowenstein, *Hitler's Germany: The Nazi Background to War* (New York: The Macmillan Company, 1939), p. 20.

[5] In the best study yet made of *Mein Kampf*, Werner Maser observes that "from [December] 1926, when Volume 2 was published, it was very easy to see what could be expected *should Hitler come to power*" (italics added). See Werner Maser, *Hitler's Mein Kampf: An Analysis* (London: Farber and Farber, 1970), p. 119; cf. also p. 201.

[6] Proclamation at the 1934 NSDAP Congress, as quoted in Karl Dietrich Bracher, *The German Dictatorship* (New York: Praeger Publishers, 1970), p. 246.

[7] As a matter of fact, Hitler had made roughly the same forecast more than once as, for example, in his speech on June 17, 1934: "This state is in its first youth, and you may be sure that in a thousand years it will stand unbroken" [as quoted in Konrad Heiden, *Der Fueher* (Boston: Houghton Mufflin Company, 1944), p. 752]. This milder form of statement probably reflects Hitler's lack of control; after all, on the same day, Von Papen, the vice chancellor, was delivering his famous anti-Nazi speech at the University of Marburg. In any case, it is the declaration of attainability that is important, as Hitler himself suggested in and by his speech of January 30, 1941, in Berlin: "Many years ago, in *Mein Kampf* I said that National Socialism will put its stamp on the next thousand years of German history." [See Franklin Watts (ed.), *Voices of History: Great Speeches and Papers of the Year 1941* (New York: Franklin Watts, Inc., 1942), p. 68]. Incidentally, in the same speech (pp. 61-62), Hitler also said, "When we came to power in 1933 our road was clearly mapped out. . . . My programme [in foreign affairs] was to do away with Versailles. People all over the world should not pretend to be simpletons and act as if I only discovered this programme in 1933, or 1935, or 1937. These gentlemen should only have read what I wrote about myself a thousand times." Unfortunately for the world, "every one of our prophecies was laughed at, every statement was represented as ridiculous, every picture of the future described as a fantastic chimera" (p. 65).

[8] Consider, for example, the remarkable conversation of September 12, 1932, between Hitler and Kurt Ludecke, which reports Hitler to have said: "Can I. . .fool those gentlemen abroad for any length of time [after coming to power]? . . .will I be able to rearm Germany before they get on to me and strike at me with a preventive war? That depends largely, I suppose, on whether they have the leadership and guts to strike—if they can get the people to go to war again, and that I doubt. [Meanwhile,] I can talk peace, but mean war." [See Kurt G. W. Ludecke, *I Knew Hitler* (New York: Charles Scribner's Sons, 1937), pp. 457, 468.] As Hitler put it in a conference with his military commanders on November 29, 1939, "Basically I did not organize the armed forces in order not to strike. The decision to strike was always in me." [Quoted in Office of United States Chief Counsel for Prosecution of Axis Criminality, *Nazi Conspiracy and Aggression: Opinion and Judgment* (Washington, D.C.: U.S. Government Printing Office, 1947), p. 19.]

[9] Bracher, op. cit., p. 296.

[10] Edward Hallett Carr, *The Twenty Years' Crisis, 1919-1939: An Introduction to the Study of International Relations* (London: Macmillan & Co. Ltd., 1951), p. vii. With Europe literally on the eve of war, Carr speculates with his readers that there may be a law governing the maximum possible size of political and economic units (e.g., nations or blocs), and he asserts that this issue is "perhaps likely to be more decisive than any other for the course of world history in the next few generations" (p. 230). Not only was the emphasis a bit misplaced in focusing on this question, but at least part of the assumption about concentration on which it rested was apparently false. See Bruce M. Russett, *Is There a Long-Run Trend toward Concentration in the International System?* Paper P-3666 (Santa Monica: The Rand Corporation, 1967).

[11] Robert A. Nisbet, "Postscript: June 1971" in Albert Somit (ed.), *Political Science and the Study of the Future* (Hinsdale, Ill.: The Dryden Press, 1974), p. 275. A "genuinely scientific" prediction, according to Nisbet, is an atemporal statement of "casual effect on the basis of. . .prior and certain intelligence that [the requisite antecedent] conditions prevail" (p. 277). Similarly, though not as critics of futures research, Klaus Knorr and Oskar Morgenstern, in their generally insightful paper, *Political Conjecture in Military Planning*, Policy Memorandum No. 35 (Princeton, N.J.: Center of International Studies, Princeton University, 1968), observe that "During recent decades, the United States has become involved in four important wars, [Which] United States involvement was predictable, or predicted with any degree of confidence, especially at the official level, even five years ahead, let alone ten or fifteen years? None of them was" (p. 10). These authors go even further. Working backward to 1907 from 1967, they divide the past into fifteen-year intervals, and for each they list (on p. 11) a number of "major politico-military events that occurred during each period but were not predicted, and not predictable, at its beginning." Among the events for the period beginning in 1937 are (1) the war and (2) its "configuration and outcome." Their argument why such events are "unpredictable" is identical, in the end, to Nisbet's, but it should be noted that they scrap the reason for making the argument at all when they introduce the notion of conjecture ("reasoned inference from admittedly defective evidence") and assert that "there is no sharp dividing line between conjecture and prediction" (p. 19). On the issue of confident predictions by high officials, see footnotes 19 and 22, as well as the memoirs of other actors in these developments.

[12] Highly representative are the slashing references to the press in Hitler's brilliant speech to the Reichstag on April 28, 1939, among them the following (the reference at the end is to the Orson Wells's broadcast of *War of the Worlds* in October 1938): "The reason for this fear [of war] lies simply and solely in an unbridled agitation on the part of the press, an agitation that is as mendacious as it is base, in the circulation of vile pamphlets about the heads of foreign states, and in an artificial spreading of panic which in the end goes so far that interventions from foreign planets are believed possible and cause scenes of desperate alarm." If the foreign press were controlled, Hitler went on, "the fear of war will disappear at once."

[See *International Conciliation*, No. 351 (June 1939), p. 327.]

[13] Sisley Huddleston, *War Unless...* (Philadelphia: J. B. Lippincott Company, 1934).

[14] *Ibid.*, p. 65.

[15] *Ibid.*, p. 266.

[16] *Ibid.*, pp. 281-282.

[17] Leland Stowe, *Nazi Means War* (New York: Whittlesey House, McGraw-Hill Book Company, Inc., 1934).

[18] *Ibid.*, p. 121.

[19] *Ibid.*, pp. 124-125. Stowe includes journalists and politicians in this summation.

[20] Stowe, *ibid.*, pp. 124, 129.

[21] For example, Lowenstein, *op. cit.*, pp. 171-172.

[22] Stowe, *op. cit.*, p. 140. The then U.S. Secretary of State, Cordell Hull, recalls that "in my first year in the State Department [1933] I had no doubts as to Hitlerite Germany's intentions and capabilities. But, had I had any, the events of 1934 and the first half of 1935 would have completely removed them." Hull told the German Ambassador on November 2, 1933, that "a general war during the next two to ten years seems more probable than peace," and he advised the President from 1934 on to implement programs to ensure U.S. stockpiles of strategic materials. See Cordell Hull, *Memoirs* (New York: The Macmillan Company, 1948), Vol. 1, pp. 242, 231, 624.

[23] Drew Pearson and Constantine Brown, *The American Diplomatic Game* (Garden City, N.Y.: Doubleday, Doran, and Company, Inc., 1935).

[24] *Ibid.*, p. 378.

[25] *Ibid.*, p. 395.

[26] John Herman Randall, Jr., *Nature and Historical Experience: Essays in Naturalism and in the Theory of History* (New York: Columbia University Press, 1958), p. 44.

[27] It could be argued that essentially the same analysis that led Stowe to forecast World War II could have been used to forecast wars like the Korean one or the Middle East conflict of 1973. It would probably have failed, however, in forecasting Vietnam, though an approach like Huddleston's might have worked. Indeed, using a comparable method, the British Royal Institute for International Affairs accurately forecasted in 1963 that the war in Vietnam would last "at least another ten years." [See "Victory in S. Vietnam Believed 10 Years Away," *Los Angeles Times* (October 7, 1963), p. 23.] We might conclude, therefore, that the Huddleston approach is the more general of the two.

[28] The energy crisis may have prompted the beginnings of such a discussion. *Time* magazine, for example, opines in a blistering "Essay" that the Arab oil boycott could easily have been forecasted and cites some evidence and a number of predictions to support this assertion [see "What Went Wrong," *Time* (December 10, 1973), pp. 49-50]. *Time* was far from being alone in this.

[29] Phillip O. Foss, "Policy Analysis and the Political Science Profession," *Policy Studies Journal*, Vol. 2, No. 1 (Autumn 1973), p. 68. In saying this, Foss distinguishes between a profession and a discipline.

[30] Foss, *op. cit.*, p. 69. A "preference model" forecast of the profession sees these and other difficulties removed (and replaced by others) by 1990, assuming that political scientists adopt a policy science orientation. See Harold D. Lasswell, "The Future of Professional Political Scientists," in Somit, *Political Science and the Study of the Future*, pp. 246-254.

[31] Excluded here are those occasional thin books which have to do with the future of the very largest of political affairs, usually foreign policy, but which seem to be allowed to fall quickly from the press into oblivion, however insightful they may in fact be. Examples that come to hand are Robert R. Bowie, *Shaping the Future: Foreign Policy in an Age of Transition* (1964); David Ormsby-Gore, *Must The West Decline?* (1966); and Gladwyn Jebb, *Halfway to 1984* (1966)—all, coincidentally, from Columbia University Press in New York. Why such books receive little attention, in or outside of the social sciences, is somewhat of a mystery; it is certainly true that much of the futures literature is no less thin, physically or otherwise. Cf. the impressions of Paul Seabury, "Practical International Futures," in Somit, *op. cit.*, pp. 279-291.

[32] Karl W. Deutsch, "Quantitative Approaches to Political Analysis: Some Past Trends and Future Prospects," in Hayward R. Alker, Jr., Karl W. Deutsch, and Antoine H. Stoetzel (eds.), *Mathematical Approaches to Politics* (San Francisco: Jossey-Bass, Inc., 1973), p. 10.

[33] His other suggestions about research are (1) undertake *large-scale* simulations of highly developed countries (the data on these countries being rich and very current, these models would offer some promise of making political science more of an experimental activity); (2) undertake simulations of the less-developed countries or of important sub- or supranational units; (3) develop "genuine cybernetic models," particularly of systems and processes that are largely autonomous; (4) develop more and better data, both original and derived, for use in model building (special attention being paid to data relevant to quality of life, personal well being, etc.); (5) develop queuing theory for use in tackling even such "core problems" of political science as the questions of freedom and power; (6) undertake major extensions of game theory; (7) continue work on stochastic process models ("drift models"), including further development of transitional probability matrices; and (8) extend preliminary work in the combining of cybernetic and drift models. Deutsch, in Alker, Deutsch, and Stoetzel, *ibid.*, pp. 25-55.

[34] Deutsch, in Alker, Deutsch, and Stoezel, *ibid.*, p. 56.

[35] *Ibid.*, pp. 57, 60.

[36] It should be emphasized, perhaps, that Deutsch does not doubt that a useful world model can be built. Similarly, it has been argued that "the ultimate method of prediction [in political science is through] the employment of comprehensive general explanatory theory. . . of the political system and its environment or of the social system within which the political system resides. . . .Once we have a general theory. . .our predictions or forecasts would be comprehensive, accurate, and credible." See David V. Edwards, "Political Forecasting," in James R. Bright and Milton E. F. Schoeman (eds.), *Technological Forecasting*," (Canoga Park, Calif.: XYZYX Information Corporation, 1970), pp. 437 and 439. Deutsch certainly would not go so far; moreover, other students would, of course, question the entire undertaking.

[37] A random sample: Lincoln P. Bloomfield, *Western Europe, 1965-75: Five Scenarios* (Cambridge, Mass.: MIT, 1965); Alastair Buchan (ed.), *Europe's Futures, Europe's Choices: Models of Western Europe in the 1970's* (New York: Columbia University Press, 1969); T. W. Milburn and J. F. Milburn, "Predictions of Threats and Beliefs about How To Meet Them," *American Behavioral Scientist*, Vol. 9, No. 7 (March 1966), pp. 3-7; Wayne Wilcox, *Forecasting Asian Strategic Environments for National Security Decisionmaking: A Report and a Method*, Rand Memorandum RM-6154-PR (Santa Monica, Calif.: The Rand Corporation, 1970); Johan Galtung, "On the Future of the International System," in Franklin Tugwell (ed.), *Search for Alternatives: Public Policy and the Study of the Future* (Cambridge, Mass.: Winthrop Publishers, Inc., 1973), pp. 226-259; Ithiel de Sola Pool, "The International System in the Next Half Century," *Daedalus*, Vol. 96, No. 3 (Summer 1967), pp. 930-935; Zbigniew Brezinski, *Between Two Ages: America's Role in the Technetronic Era* (New York: Viking Press 1970); Rudolf Rummel, "Forecasting International Relations," *Technological Forecasting and Social Change*, Vol. 1 (1969), pp. 196-226; James A. Dator, "Political

Futuristics Toward the Study of Alternative Political Futures," in David Plath (ed.), *Aware of Utopia* (Urbana, Ill.: University of Illinois Press, 1971); and the important anthology, Harvey S. Perloff (ed.), *The Future of the United States Government* (New York: George Braziller, 1971) These items are cited because they are intrinsically interesting (and handy, as this is being written); they certainly do not, however, exemplify all of the approaches that have been taken by persons within the field. For additional and more systematically organized references, see the bibliographies in the works cited in footnote 42.

[38] These range from use of systematic and quasi-theoretical schemes for organizing the material of political science to novel approaches for bringing the views of the classic political philosophers into focus on contemporary issues Two examples of the former are Paul T. David, "Analytical Approaches to the Study of Change," *Public Administration Review* (Summer 1966), pp. 160-168, and Norman Uphoff, "Integrating Policy Studies into Political Science," *Policy Studies Journal*, Vol. 1, No 4 (Summer 1973). The latter is discussed in Harlan J Strauss, "Past, Present, Future. A New Curriculum for Introductory Courses in Political Science," paper presented at the Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September 4-8, 1973. As political science seeks to become more of a science, and as it becomes embued with more of a futures orientation (especially in the classroom), there are dangers, of course, of confusing mere "relevance" with a policy orientation. This idea figures in Fred Kort, "The Multiple Dilemma of Political Science," *Modern Age*, Vol. 16, No. 1 (Winter 1972), pp. 15-24.

[39] See Yehezkel Dror, *Futures in Government*, Paper P-3909 (Santa Monica, Calif.. The Rand Corporation, 1968); Todd R. La Porte, "Politics and 'Inventing the Future'. Perspectives in Science and Government," *Public Administration Review*, Vo. 27, No. 2 (1967), pp. 117-127; and James A. Dator, *Comments on the Hawaii Senate's Seminar on Planning*, Working Paper 3 (Honolulu: Program in Futures Research, University of Hawaii, 1973). An argument for retaining the older intuitive and largely nonfutures oriented style is given by Hedley Bull, "International Theory: The Case for a Classical Approach," *World Politics* Vol. 18, No. 3, (April 1966) pp. 361-377; implicitly at least, it finds earlier expression in George Allen Morgan," Planning in Foreign Affairs: The State of the Art," *Foreign Affairs*. Vol. 39, No. 2 (January 1961), pp. 271-278, where a need is declared for "new ideas in preparing for future," but the tools recommended for the purpose are (1) the planner's "own qualities of mind and experience," (2) active practice in the conduct of political affairs, (3) conversation with others; (4) documents; and (5) formal consultation with other experts. Ironically, Morgan's article is followed in the same issue of *Foreign Affairs* by Franklin A. Lindsay's "Program Planning: The Missing Element" (pp. 279-290), which demonstrates that even on the program level in foreign affairs, the United States was then doing an "inadequate job" of planning "because we are not anticipating sufficiently our needs for these instruments of policy execution."

[40] Notably, Bertram Gross, "Friendly Fascism: A Model for America," *Social Policy* (November-December 1970), reprinted in Tugwell, *op. cit.*, pp. 287-301.

[41] This tendency, like similar tendencies in other disciplines, seems to begin in the context of systems analysis; see, for example, David Easton, *A Systems Analysis of Political Life* (New York: John Wiley & Sons, Inc., 1964). General reviews of futures research and other methods are given in Edwards, *op. cit.*, and Stuart Greenberg, *Forecasting in International Relations*, Staff Discussion Paper 407 (Washington, D.C.: Program of Policy Studies in Science and Technology, The George Washington University, June 1970); because of their generality and brevity, such papers are of little value today, except as indicators of change in the field. Specific new techniques for specific applications are described and advocated in papers like Yehekel Dror's "The Prediction of Political Feasibility," *Futures*, Vol. 1, No. 4 (June 1969), pp. 282-288, and Olaf Helmer's *The Use of Expert Opinion in International-Relations Forecasting* (Los Angeles, Calif.. Center for Futures Research, University of Southern California, July 1973). Paralleling in many ways the world model goal of Deutsch are comprehensive study recommendations for domestic political forecasting. see in particular the several proposals by Dror, especially "Alternative Domestic Political Futures: Research Needs and Research Design," *Futures*, Vol. 2, No. 4 (December 1970), pp. 302-311. Dror's statement of research needs for this task can be subsumed under Deutsch's list (see footnote 33), though he emphasizes the necessity of further research on judgmental matrix techniques.

[42] The most important of these is Nazli Choucri and Thomas Robinson (eds.), *Forecasting in International Relations* (San Francisco. Freeman, in press). Despite its title this volume is pertinent to all areas of political science, covering as it does not only the international problems, but also basic issues of the nature of forecasting, the choice of methods, the problem of validation, and so on. Another important volume, one which lays stress on technological as well as political, social, and economic issues in forecasting, is Klaus P. Heiss, Klaus Knorr, and Oskar Morgenstern, *Long Term Projections of Power* (Cambridge, Mass.: Ballinger Publishing Company, 1973). This volume surveys "power" in all of its dimensions and offers a critical review of methods of forecasting it. Basic anthologies that cut through much of the earlier literature are those by Somit and Tugwell (both cited earlier).

[43] Bertrand de Jouvenel, "Political Science and Prevision," *The American Political Science Review*, Vol. LIX, No. 1 (March 1965), pp. 29-38.

[44] If the willy-nilly juxtaposition of literature from political theory, international relations, American politics, comparative politics, public administration, and other areas in this survey were reorganized by category, the grounds for making an estimate would be somewhat clearer—though perhaps more dismal, except in international relations.

[45] Karl F. Schuessler, "Prediction," *International Encyclopedia of the Social Sciences* (New York: The Macmillan Company, 1968), Vol. 12, p. 423. He cites Comte's "*savoir pour prevoir*" in this connection. On this and other matters discussed in this section, see also the same author's "Continuities in Social Prediction," in Herbert L. Costner (ed.), *Sociological Methodology* (New York: Jossey-Bass, 1971), pp. 302-329.

[46] Bernard Berelson, "Sociology in Action: In Population and in General," in Arthur B. Shostak (ed.), *Sociology in Action* (Homewood, Ill.: The Dorsey Press, 1966), as reprinted in U.S. House of Representatives, Research and Technical Programs Committee of the Committee on Government Operations, *The Use of Social Research in Federal Domestic Programs* (Washington, D.C.: U.S. Government Printing Office, April 1967), Vol. III, p. 518. In its various manifestations, the conflict highlighted here can be traced back to the founders of modern sociology. For example, observe the characterization by Lazarsfeld (following Joseph Lottin) of the different images of the social sciences held by the contemporaries Comte and Quetelet: "Comte was trying to derive from history broad developmental trends which could be projected into the future, while Quetelet was bent on finding precise regularities which could help explain the contemporary social scene." See Paul F. Lazarsfeld, "Notes on the History of Quantification in Sociology—Trends, Sources, and Problems," in Harry Woolf (ed.), *Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences* (Indianapolis, Ind.

The Bobbs-Merril Company, Inc., 1961), pp. 147-203; the quotation is from p. 169.

[47] James Fennessey, "Some Problems and Possibilities in Policy-Related Social Research," *Social Science Research,* Vol. 1, No. 4 (December 1972), p. 367. Against this background, Fennessey recommends that sociologists begin to experiment with the use of a Bayesian approach to some important types of problems.

[48] Richard P. Applebaum, *Theories of Social Change* (Chicago, Ill.: Markham Publishing Company, 1970), pp. 136-137. The quotation from Talcott Parsons is from his *The Social System* (New York: The Free Press, 1951), p. 534. Robert Bierstedt points out in another context that sociologists "have a great many taxonomies, but we have a dearth of casual propositions. This is the most embarrassing thing we confront. One of the things we ask ourselves once and awhile is: Do you have any laws, any principles, any universal propositions in sociology? The answer is that we do not." Later on, it should be said, he adds, "I conceive of sociology. . .as a humanistic discipline, not only a scientific one. I would judge [an] essayist approach in terms of the cogency of its argument. [An] important illustration [is] Max Weber's thesis. I submit to you that no one knows whether it is true or false. We judge it in terms of its cogency." Quoted from James C. Charlesworth (ed.), *Integration of the Social Sciences through Policy Analysis,* Monograph 14 (Philadelphia, Pa.: The American Academy of Political and Social Science, October 1972), pp. 160, 221-222.

[49] Cf. F. Machlup, "The Inferiority Complex of the Social Sciences," in M. Sennholz (ed.), *On Freedom and Free Enterprise* (Princeton, N.J.: Van Nostrand, 1956), pp. 161-172, and A. Mazur, "The Littlest Science," *American Sociologist,* Vol. 3 (August 1968), pp. 195-200. The sociology of knowledge, to say nothing of the social sciences generally, might benefit from an anaylsis of this theme of masochism, which, by the way, is certain to manifest itself sooner or later in the writings of important practitioners of futures research also.

[50] These observations are drawn from Richard L. Henshel, "Sociology and Prediction," *The American Sociologist,* Vol. 6 (August 1971), pp. 213-220. Some other ideas in this paper have been carried forward in later publications, including R. L. Henshel and L. W. Kennedy, "Self-Altering Prophecies: Consequences for the Feasibility of Social Prediction," in L. von Bertalanffy and A. Rapoport (eds.), *General Systems Yearbook,* [illegible] (Ann Arbor, Mich.: Society for General Systems Research, [illegible]73), and R. L. Henshel, "The Prestige of Sociology and the Accuracy of Its Predictions: A Feedback Loop with Serious Implications," paper presented at the Society for the Study of Social Problems, New York City, August 1973.

[51] Henshel adds, "the relationship commonly assumed between prediction and control specifies that as the predictive power of a discipline improves, its ability to control is enhanced. Without denying this, we advance the complementary thesis that as control is increased, a discipline's *apparent* predictive power is heightened." Henshel, *op. cit.,* p. 214.

[52] Something of this line of thought is implicit in Wilbert E. Moore, "Predicting Discontinuities in Social Change," *American Sociological Review,* Vol. 29, No. 3 (June 1964), pp. 331-338, where the claim is made that sociology has advanced to the point where it can, on good grounds, make probabilistic predictions of even those changes that are irregular in magnitude, rate, and direction.

[53] Henry Winthrop, "The Sociologist and the Study of the Future," *American Sociologist,* Vol. 2 (May 1968), p. 142. Winthrop also asserts that because sociologists know more "about the social system of which they are a part" they have more of an "edge" in making projections. Winthrop makes a quite different argument—omitting reference to competing diciplines and emphasizing social need and social values—in his "Utopia Construction and Future Forecasting: Problems, Limitations, and Relevance," in Wendell Bell and James A. Mau (eds.), *The Sociology of the Future: Theories, Cases, and Annotated Bibliography* (New York: Russell Sage Foundation, 1971), pp. 78-105.

[54] Two such books are Wendell Bell and James A. Man, *The Sociology of the Future* (New York: Russell Sage Foundation, 1971) and the anthology edited by Arthur Harkins, *1972 American Sociological Association Seminar on the Sociology of the Future* (Minneapolis, Minn.: Office for Applied Social Science and the Future, University of Minnesota, 1972).

[55] Bell and Mau, *op. cit.,* p. 21. Perhaps it should be said that the cybernetic concept embodied in this model derives from the work of Karl Deutsch, and the emphasis on "images," from Frederik Polak and Harold Lasswell. See the bibliography in this book for references.

[56] The list that follows is paraphrased from Bell and Mau, *op. cit.,* pp. 29-37.

[57] C. Wright Mills, *The Causes of World War Three* (New York: Simon and Schuster, 1958), pp. 12-13. Mills rejects this idea, of course, as forcefully as tract-writing permits; he argues: "What men might become, what kinds of societies men might build—the answers to such questions are neither closed nor inevitable. Yet. . .the ends of which men might dream are consigned to 'merely utopian fantasy.' In the meantime, virtually all images of the future—from Aldous Huxley's to George Orwell's—have become images of sociological horror, and 'practical action' has been usurped by frightened and unimaginative mediocrities. . . .What 'practical men of affairs' do not face up to is the fact that 'politics' has to do with the willful making of history. The enlargement and the centralization of the means of history-making signify that, for better or worse, power elites are no longer in a situation in which their will and reason need be overwhelmed by 'impersonal forces beyond their control'. . . .A politics of semiorganized irresponsibility prevails. But that fact ought not to blind us to the political possibilities opened up by this great structural change: It is now sociologically realistic, morally fair, and politically imperative to make demands upon men of power and to hold them responsible for specific courses of events" (pp. 94-95). In this, Mills directly anticipates some of the current thinking among futurists in sociology; the novelty of the new school resides mainly in its emphasis on *beginning* analysis by elucidating and testing *alternative images* of the future, perhaps using models of the sort devised by Bell and Mau or perhaps simply by "acting out" what Robert Boguslaw has called a "social design." See Boguslaw "The Design Perspective in Sociology," in Bell and Mau, *op. cit.,* pp. 240-258, or his *The New Utopians: A Study of System Design and Social Change* (Englewood Cliffs, N.J.: Prentice-Hall, 196[illegible]). Somewhere between these approaches is the one enunciated in a highly elaborate and maddeningly idiosyncratic language by Bernard C. Phillips, *Worlds of the Future: Exercises in the Sociological Imagination* (Columbus, Ohio: Charles E. Merrill, 1972), wherein the author combines gaming concepts, science fiction, mathematical modeling using systems dynamics concepts, and probably other perspectives in order to analyze opportunities for creating the future.

[58] See his *Successful Social Prophecy in the Past,* unpublished master's thesis (New York: Columbia University, May 3, 1920); results of this study, as well as subsequent work, are highlighted in Gilfillan's "A Sociologist Looks at Technical Prediction," in James R. Bright (ed.), *Technological Forecasting for Industry and Government: Methods and Applications* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1968), pp. 3-34.

[59] "There is," he wrote in his MA thesis, "a call for Mellontologists, students of future civilization in general, just as there are Archaeologists who reason out all the interacting aspects of

prehistoric culture. And since no one in the world at present seems to be working on this synthesis, it has seemed to the writer a very appropriate endeavor for vita sua" (pp 1-2). Gilfillan asserts that he had made this decision "a dozen years ago" (i.e., in about 1908), in his autobiography (p. v), he notes that he "had long planned, since 1909 or earlier, that my great work would be to make prophecy scientific." See "An Ugly Duckling's Swan Song: The Autobiography of S Colum Gilfillan," *Sociological Abstracts* (February-April 1970), pp. i-xxv and (May 1970), pp. xxvi-xl. He tells us (p. viii) that he chose to become a sociologist because he considered that field "nearest to Prediction"—and, secondarily perhaps, because in those days (1919-1935) eugenics was "a respectable science, and helper of Sociology." (Beginning in 1963, Gilfillan turned his attention from forecasting and the sociology of invention to issues of eugenics. Indeed, he considered his "life's best work [to be] my eugenic discovery of lead poisoning as the principal destroyer of ancient genius, culture and progressiveness" (autobiography, pp xxviii-xxix). In the larger scheme of things, however, he is likely to be remembered—maybe "discovered" is the better word—as one of the persons who helped prepare the way for futures research, in and out of sociology.

[60] Published by McGraw-Hill A good bibliography of these and other scientific writings by Ogburn will be found in W F. Ogburn, *On Culture and Social Change*, Otis Dudley Duncan (ed.), (Chicago, Ill. Phoenix Books, The University of Chicago Press, 1964), pp. 349-360.

[61] Gilfillan in Bright, *op. cit.*, p. 28. He cites Ogburn's "Prospecting for the Future," *Social Frontier*, Vol. 1 (April 1935), pp. 20-22.

[62] Two studies led by Ogburn are fundamental: first, *Technological Trends and National Policy. Including the Social Implications of New Inventions*, Report of the Subcommittee on Technology to the U.S. National Resources Committee (Washington, D.C · U.S. Government Printing Office, June 1937), and, second, Ogburn's *The Social Effects of Aviation* (Boston, Mass.: Houghton Mifflin Co., 1946). Both works, by the way, contain chapters on the general question of forecasting; both may have been written by Gilfillan, though he signed only the first. For Gilfillan himself, see especially his *The Sociology of Invention* (Cambridge, Mass.: The M.I.T. Press, 1970, original ed., 1935), Chap. VII, and the somewhat modified discussion of the same questions in *Supplement to the Sociology of Invention* (San Francisco, Calif.: San Francisco Press, Inc., 1971), Chap. 7.

[63] The number is not as significant, of course, as what it implies regarding the status, content, and aims of futures research in academia. See H. Wentworth Eldredge, "Teaching the Sociology of the Future, 1972," in Arthur Harkins (ed.), *1972 American Sociological Association Seminar on the Sociology of the Future*, pp. E1-E35.

[64] The concept goes back to 1959 in Bell's writing. Its most recent formulation will be found in his book, *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (New York: Basic Books, Inc., 1973). As Bell points out, the "idea of a post-industrial society, like that of industrial society, or capitalism, has meaning only as a conceptual scheme" (p. 114). Its explanatory or predictive value, therefore, depends on its being understood and exploited in its entirety—something that has not yet happened, even in Bell's own works. But however imperfectly the notion has been incorporated in futures research, it has had an overwhelming allure throughout the world as an organizing principle or point of reference. By the same token, it has also been debunked, in the context of sociological thought, as a throwback to the old "evolutionary" modes of attempting to understand changes in the social structure—as a rather obvious device to cope with the problem that advanced society does not in fact culminate, as was once expected, with an end to ideology in the glories of the industrial society. (The device, of course, is simply to tack on the postindustrial society as the new ultimate stage in social evolution.) This criticism is expressed best in Krishan Kumar, "Inventing the Future in Spite of Futurology," *Futures*, Vol. 4, No. 4 (December 1972), pp. 369-374, which concludes with these observations: "If Bell and his followers are right, all we can say is that the call to invent the future is empty rhetoric, since the future seems to promise no more than the past writ large, and it would be idle to attempt new designs in a context so bound in the traditional mould. At its most distinctive the post-industrial society does no more than offer its Saint-Simonian administrators greater facility for social control than ever were possible in the industrial society. . . . That approach seems to remain the prisoner of its own tradition and its own history." In this sense, at least, the concept can be said to embody a kind of futures thinking that is moving in a direction quite different from that of some of the authors discussed above.

[65] Scheussler, in the article cited in footnote 45, discusses five issues as being representative of the range of these problems. I summarize them as questions. (1) Can predictions be derived from case materials (without consideration of probabilities) and, if so, are these predictions in any sense more accurate than those based on statistical averages or rates? (2) What, empirically, is the influence of predictions on subsequent events? (3) How might the efficiency of social prediction be improved (where "efficiency" is a measure of the accuracy of particular methods relative to that of an alternative method that is taken as a norm)? (4) Should explanation (i.e., the development of theory) or prediction (i.e., the test of factual hypotheses) be given greater weight in sociology? (5) Are some classes of social events inherently unpredictable? (See Schuessler, *op. cit.*, pp. 421-423.)

[66] This list is from Suzanne Keller, "The Utility of Sociology for Futurism" in Harkins, *op. cit.*, pp. K8-K9.

[67] Otis Dudley Dunchn, "Social Forecasting—The State of the Art," *The Public Interest*, No. 17 (Fall 1969), pp. 88-118.

[68] Joseph F. Coates, "Some Methods and Techniques for Comprehensive Impact Assessment," *Technological Forecasting and Social Change*, Vol. 6 (1974), p. 344.

[69] Duncan, *op. cit.*, p. 91.

[70] Duncan, *ibid.*, p. 93.

[71] Duncan, *ibid.*, pp. 95-96.

[72] Duncan, *ibid.*, p. 94.

[73] Kenneth E. Boulding, "Dare We Take the Social Sciences Seriously?" *American Psychologist*, Vol. 22 (1967), pp. 879-887.

[74] Duncan, *op. cit.*, p. 98.

[75] Duncan, *ibid.*, p. 114.

[76] Duncan, *ibid.*, p. 112.

[77] Duncan, *ibid.*, p. 108.

[78] For example, two recent studies might be cited: Daniel P. Harrison, *Social Forecasting Methodology: Suggestions for Research* (New York: Russell Sage Foundation, 1976) and Richard L. Henshel, *On The Future of Social Prediction* (Indianapolis: The Bobbs-Merrill Company, 1976).

[79] Duncan, *op. cit.*, p. 105.

[80] Duncan, *ibid.*, p. 106.

[81] Duncan, *ibid.*, p. 107.

[82] Duncan, *ibid.*, p. 115.

[83] As suggested in this chapter, the notion of forecasting in order to create the future (rather than just predict it) is being taken more and more seriously.

Chapter 6

Technological Forecasting: State-Of-The-Art, Problems, and Prospects

Ralph C. Lenz, Jr.

What is the state-of-the-art in technological forecasting? The answer to this question may be expressed by a description of the forecasting methodologies in use, by the size and status of the profession of forecasting, by the extent of its use and utility in the decision process, by assessing the ability of technological forecasting to explore the future, and by its role in improving the mechanisms of planning and change. All of these expressions are pertinent to the question, but more importantly they each bear significantly on the problems and future prospects of technological forecasting.

Although the state-of-the-art may be defined without delving into historical background, a brief look at some of the major milestones in technological forecasting may provide a useful platform from which to view the present situation. With no pretense at representing a definitive history of technological forecasting, the following chronology provides an overview of the development of forecasting methodology. The Gompertz curve is the prime, and possibly the only, contribution of the 19th century. For those who note the prose of Jules Verne and the poetry of Lord Tennyson as contributions during this period, it must be said that while both demonstrate the method of scenario description of the future, neither provides a basis for decisions or improvement in the process of planning.

Writing for private publication in 1906, Henry Adams first proposed the theory of exponential increase in technology which still forms the underpinnings of most technological forecasting methods. Although bibliographies cite numerous forecasts in the early part of the 20th century, the absence of methodological basis is notable in almost all. Not until 1935 do we find the first major milestone with a continuing link to the present in Gilfillan's *Sociology of Invention*. In 1937, the first fully organized and methodological forecast appeared under the sponsorship of the National Research Council, titled appropriately, even for today's environment, *Technological Trends and National Policy*.

A major war, reconstruction, and a conference at Princeton in 1951 stage-managed to discredit any and all approaches in the forecasting of technological progress,[1] combined to produce a hiatus in further development of methodology until 1959. In that year the phrase "technological forecasting" was first used in the title of a thesis by Lenz describing specific methods for the development of quantitative forecasts of technological progress. The same year saw the further unfolding of the Delphi technique in a paper on the "epistemology of the inexact sciences" by Helmer and Rescher.

Publication of Air Force monographs in 1961 and 1962 written by the present author and based on his thesis cited earlier, provided the first widely circulated document bearing the title "Technological Forecasting." From this point on, the field developed rapidly with Honeywell's PATTERN in 1963, further development of Delphi by Gordon and Helmer in 1964, founding of the World Future Society in 1966, followed by Jantsch's massive survey and Bright's first conference on technological forecasting in 1967. Gordon's development of the cross-impact matrix during this period, Mansfield's substitution model and Floyd's figures-of-merit forecasts in 1968, and Seaman's model of the competitive process in 1969 provided significant new techniques in forecasting. If a professional journal is evidence of the existence of a profession, the American-Elsevier publication of *Technological Forecasting* in 1969 under Linstone's leadership must be cited as a major milestone.

Also in 1969, Forrester's *Urban Dynamics*, based on his earlier work with dynamic modelling of large systems, brought wider attention to the possibilities of forecasting based on rules of logic. Fusfeld's development of the technological progress function in 1970 should be the last milestone cited in this historical review, since the developments of the 1970's more properly belong in the description of the state-of-the-art.

State-of-the-art

In 1967, Jantsch was able, in several hundred pages, to summarize the state-of-the-art in his book *Technological Forecasting in Perspective*. Such an undertaking is obviously beyond the scope of this paper. As an alternative, the state-of-the-art may be defined in collective terms—that is, what are the techniques of forecasting capable of doing, and what can they as yet not achieve?

At the present time, it may reasonably be asserted that one or more effective techniques are available for making a quantitative forecast of probable progress in any technology which has attained at least initial operational capability or commercial viability. These techniques are sufficiently varied in terms of information provided and cost to produce useful forecasts that can be provided within a wide range of resource expenditures. Forecasts made by using these techniques can provide quantitative estimates of attainable performance over a period of 20 years into the future, which estimates will be probably within 25 percent of the achieved value at that time.

As a guide to research needs in the future, it may be more important to note what we cannot do. First, none of the techniques now available is effective in predicting the impact of new discoveries or the rate of adoption of technologies which have not yet attained a 2-percent penetration of the market. Second, techniques for predicting the date of occurrence of critical events necessary for the initiation of a new technology, such as the Hahn-Strassman point, are entirely inadequate. Third, techniques do not exist for searching out, identifying, and synthesizing the scattered bits of research which are the precursors of new discoveries. Also, methods for synthesizing the cumulative effects of discoveries in several related fields require much further development of the techniques developed by Acey Floyd.

Fourth, we cannot as yet forecast the cross-impact effects of technological progress and economic development. The limited research on techno-economic relationships as a forecasting tool by Mansfield, Seamans, Roberts, Fusfeld, et al. has only pointed out the general directions for research in this area. No general merging of the technical and economic disciplines has occurred to enable quantitative cross-impact analysis of the effects in these two intimately related spheres. Regression analysis approaches to this problem have proven particularly sterile in the absence of experimental demonstration of cause-and-effect relationships.

Fifth, knowledge of the rates of change in social structures and value systems, and of the political reactions which reflect these changes, is only slightly removed from the Dark Ages arguments concerning the number of angels that might dance on the head of a pin. Recognition of the need for a better data base and useful forecasting techniques in these areas is widespread, but little is yet available.

What is the status of technological forecasting as a profession? With reference to definitions found in the dictionary, it has reached the status of a calling requiring specialized knowledge, and much of that knowledge is documented in books and journals. On the other hand, intensive formal academic preparation is not yet available nor is it a prerequisite for the general practitioner. College level courses are available in technological forecasting, but as yet there is no 4-year program leading to a Bachelor of Science degree in Technological Forecasting. We are not yet at the stage of knowing what should be included in a 4-year program, and are even further from being able to define graduate and doctoral level programs.

Under sometimes different labels, one can identify individuals whose principal calling, vocation, or employment is in technological forecasting, which meets the second dictionary test for a profession. On the other hand, for most practitioners, futures forecasting is an avocation pursued as an adjunct to other professional efforts, so that it is difficult to identify a whole body of persons engaged in this calling. Only informal associations and exclusions provide any measure of the technical and ethical standards which are the necessary hallmark of a profession. We have been generally successful in excluding the fortune-tellers and prophesiers from the ranks of the profession, yet Jeanne Dixon probably has more believers than the entire body of serious professionals. More importantly, justifiable criticism of charlatanry within the ranks of the profession is being raised both by insiders and by those we serve. Thus perhaps one area for research is the exploration of appropriate technical and ethical standards to judge and govern professional conduct in the practice of technological forecasting. One final test of a professional is whether or not he participates for gain or livelihood in an activity often engaged in by amateurs. Certainly there are enough amateur forecasters to enable the few professionals to meet this test.

How widely and effectively is technological forecasting used in the decision process? At the national level, as we move from crisis to crisis, it is apparent that forecasting plays no role whatever in the decisions that are made. Indeed, where public and government interest shifts from one enthusiasm to another and from one panic reaction to the next with the attention span of a child, it is very difficult for rational forecasting efforts to influence decisions. Within the individual departments of the government, there is some evidence that reasonable forecasting efforts are being made and that the forecasts are utilized in making decisions in those areas where public interest is not dominant. Although the Defense Department probably still leads in the utilization of technology forecasts, other factors tend to dominate decisionmaking on major issues.

Research to date has not been notably successful in quantifying the degree of utilization of forecasting in industrial decisions. Some highly visible forecasting efforts have been quietly dropped by various companies. However, the continued repeat attendance of industry representatives at various short course offerings and the growing use of consultive forecasting services indicates that a solid base of technological forecasting capabilities is being established and used within most industries.

Insight will be required to find ways to determine the utilization and utility of forecasting in decisionmaking. No measures currently exist for determining the rate-of-return on investment in technological forecasting, but this frequently asked question may be both inappropriate and unanswerable. Surveys asking forecasters to indicate the utility of their forecasts and the effect of their forecasts on decisions are bound to produce biased responses, and surveys asking executives the same questions are unlikely to produce any statistically significant response at all. Until a definitive analysis of the decision process, as it exists in the real world, becomes available, it seems unlikely that the question of forecast utilization can be answered.

What about the ability of technological forecasting to explore the future? This question was much easier to answer 15 or 20 years ago when, for example, Peter Drucker could claim confidently that, "The major events that determine the future (for the next two decades) have already happened—irrevocably." Then we had a Babbitt-like confidence in the stability of our value system and complete faith in the theory of exponential acceleration of technology first advanced by Henry Adams. Today, with better forecasting techniques and a greater level of experience in their use, we can only say that these techniques are powerful tools for examining possible futures, given certain assumptions (about the stability of the social and economic systems) which were inherent to the development of these techniques. The adaptability of the forecasting techniques described in books by Cetron, Bright, Ayres, Gordon, Martino, and Lanford, and in the journal *Technological Forecasting and Social Change* to alternate assumptions concerning value systems and economic stability has not been tested. Research directed toward such adaptability or unadaptability seems much more profitable at the present time than refinement of current methodology which may rest upon now shaky premises.

What can be said about the role of technological forecasting in improving the mechanisms of planning and change? If one accepts Jantsch's definitions of explorative and normative forecasting, the question is unanswerable since to him "normative" forecasts are the mechanisms of planning and change. For that body of techniques which fits under Jantsch's definition of "exploratory" forecasting, the present state-of-the-art offers abundant reason for their use in improving the planning process. At the very least, these methods can provide vital information about probable events if current courses of action are pursued. If such events are undesirable, preventive planning is possible. If these events are desirable, compatible planning will be profitable. Current quantitative methods of forecasting can identify areas of future conflict—technical, economic, or social—with a significant degree of accuracy in the timing and magnitude of such conflicts. Indeed the question is not whether the current state-of-the-art in forecasting will permit its use in planning and promulgating change, but rather why the available forecasting techniques have been so little used for this purpose. Certainly the massive effects of the environmental protection laws upon energy resouce requirements were highly predictable, and one can only marvel that rational men could project themselves into the current chaotic situation.

If the preceding is an acceptable summary of the state-of-the-art in technological forecasting, then it is appropriate at this point to examine some of the problems which inhibit further progress and to recommend areas of research which may prove useful in resolving these problems.

Current Problems in Technological Forecasting

The major problem with technological forecasting methodologies in use today is that they are not in use. Most methods are described adequately in the literature. derivations are mathematically sound, and further refinement of technique is scarcely warranted in view of the imprecision of the data bases available for use with these techniques. However, one problem is the mathematically indiscriminate use of data points which have unequal significance with respect to the state-of-the-art. This suggests that research providing means to evaluate, discriminate, and weight data points so that trends regarding performance levels would be more indicative of the true state-of-the-art would improve forecasting capabilities.

Problems with forecasting as a profession do not appear overwhelming at the moment. Growth appears within reasonable bounds, although a sudden increase in demand for forecasting would certainly disclose a serious shortage in experienced forecasters and in ability to provide adequate training for new entrants. A body of literature exists which is adequate for the support of serious academic efforts to provide specialists in technological forecasting. The principal problem here is that of overcoming the traditional disciplinary barriers of academia to create new programs and to establish what must be a multidisciplinary specialty in forecasting.

The use of technological forecasting in the decision process presents no problem. The decisionmaker either uses the best forecasts available to him or adopts a course of action independent of such forecasts. The situation is analogous to betting on games of skill between opposing teams. The man who bets on the basis of the best available forecasts of performance will not win every time but he will do consistently better than either the person who bets purely on chance or the person who bets on hunches. It is unlikely that any type of research will convince an executive that he should use technological forecasts unless he already sees the need for improvement in his decision processes. Reasonably realistic games or simulations played with and without forecasting aids might convince a few executives if they (1) would take the time to play such games, (2) believed in the outcome, (3) felt the game was not-"fixed," and (4) lost when they didn't use the forecast. The only problem presented by the company executive who does not wish to use technological forecasts is a lost market for the forecaster and the possible demise of the company. Possibly the same can be said at the national level.

It may be argued reasonably that how one uses technological forecasts in the decision process is a significant problem. Nevertheless, this appears to be more of a procedural problem or a matter of improving the decision process itself than a problem for solution by the technological forecasting community or profession.

It is when we try to assess the ability of technological forecasting to explore the future that we find the most perplexing problems today. In spite of all the discussion about changing value systems there may be more stability in the value system of the American public than in indicated in the current literature. Measurement of change in this area is still sparse, surveys tend to indicate peoples' thoughts rather than their actions, and substitution of the so-called new lifestyles has scarcely reached a level justifying predictions of a takeover. In fact the application of the quantitative techniques of technological forecasting would be most helpful in exploring the nature and the rate of change in our value systems.

The following quotation may indicate both the necessity and difficulty of determining whether value changes will take place:

> One after another he saw the ideals of what a great democracy might strive for abandoned in the lust for gain, for personal comfort and riches. We, the people, had become we, the plunderers. The really great and noble American dream, the dream of a better and fuller life for every man, had become a good deal like the stampede of hogs to a trough. There are not a few signs today that in this America of ours, there is wide revolt against the direction that our life has taken. We are no longer sure that wealth will create a satisfying scale of values for us. There is an interest in people who have insisted on being themselves and suiting themselves; a questioning of all concepts, including those of failure and success. Against the whole rushing stream of contemporary life, the individual feels himself rather powerless, and prone to irony as a way of escape.

Does this quotation sound much like current predictions of an impending change in the American value system? If it does, reflect that it was written by James Truslow Adams as an introduction to the 1931 edition of *The Education of Henry Adams*. Using this quotation as a baseline for a qualitative measure of value change it is difficult to see that significant changes in the value system have occurred during the past 40 years and it makes current similar forecasts of future value changes somewhat suspect, unless supported by measurable evidence of change.

Greater difficulties appear in considering the relationship between economic stability and technological progress. Random, small, and transient changes in value systems appear capable of inducing large excursions in precariously balanced economic systems. The mutual dependency of economic growth and technological progress is one of the biggest problems facing technological forecasting today. The long term history of both in the United States indicates little more than general concurrency in the trends of economic development and technological improvement, in both the long term and the short term. It has been noted that the Roman economy faltered and the empire fell as the result of a chronic energy shortage, which may or may not be a portent of similar consequences if U.S. energy problems are not solved. Research in technological forecasting should be directed toward development of at least a few provable assertions concerning the expenditures required to maintain desirable rates of technological advance and the actions required to assure sources of funding for such expenditures.

Weakness in the underpinnings of the basic concepts in technological forecasting raises the greatest questions concerning the ability of technological forecasting to explore the future. Nothing more significant has been written on this subject than Henry Adams' 34th chapter.[2] Adams noted that technological progress accelerated in a manner analogous to the motion of a comet, as a result of the attraction of men's minds to concepts of energy and physical forces. He believed in 1905 that this progress would reach perihelion within 30 years, i.e., circa 1935. Adams' concepts are based on serious study and documentation of rates of scientific and technological progress and a thorough understanding of motivational forces within society.

The question of perihelion is paramount in consideration of technological forecasting's view of the future. If the course of society's activities is now directed away from concepts of energy and physical forces, then the rate of technical progress should decrease in accordance with the same formula which governed its prior motion.

What evidence exists, if any, for concluding that a lessening in the attraction of men's minds toward concepts of energy and physical forces is taking place, and the corollary conclusion that the rate of technical progress in the future will decrease in a manner proportional to its previous acceleration? In the 1930's, it would have been quite easy to accept Adams' prediction of perihelion as being right on schedule, and the literature of that period discloses that many indeed did believe that technological progress was being permanently slowed. Of course, the development of nuclear power in the 1940's, earth-orbiting capabilities in the 1950's, and space travel and computer development in the 1960's demonstrated instead the existence of continuing acceleration of technology.

Should we now read the signs of the 1970's as a true indication of perihelion or as a perturbation similar to that of the 1930's? Are the cancellation of the SST, the hiatus in manned space flight, the decline in NASA and DOD R&D budgets, the energy crisis, and the decline in engineering enrollments tangible signals of a permanent decrease of man's interest in the forces which produced exponential increase in technological capabilities? And are the interest in ecology, consideration of limits to growth, emphasis on social needs, and organized opposition to science and technology positive symptoms of the existence of new forces which will become the compelling attraction for the efforts of our society?

Technological forecasting cannot answer these questions today. It can, however, project alternate futures, one set based on the assumption that perihelion was reached in 1969 with man's lunar landing and another set assuming that the acceleration of technology will continue as it has in the past, at least until the year 2000. Research may then be directed toward a search for evidential demonstration of the validity of either these hypotheses.

Of course, technology is not a single aggregated body and its path must be described in terms of the multiple paths of many disaggregated bodies. In this sense it may act more like a group of meteoroids on a more or less common path rather than like a single comet. Thus some bodies of technology may already have passed perihelion and be slowing their rate of advance (for instance, transportation technology); while others are still accelerating (such as energy technology).

What problems must technological forecasting solve for itself in order to improve the mechanisms of planning and change? First, we must strip away some of the mumbo-jumbo about "self-fulfilling" and "self-defeating" forecasts and treat this area in more rigorous terms. No amount of resources or planning can bring about "self-fulfillment" of a forecast for an "idea whose time has not yet come." For example, a plan[3] to put a man on the moon within the 10-year period following Jules Verne's fictional forecast would have required an annual expenditure equal to one-fourth of the U.S. gross national product in technologies not then conceivable, let alone available. Such a plan would have had no greater chance of self-fulfillment in 1940, and if initiated in 1950 it would have resulted in failure and disillusionment, or at best massive cost overruns and completion 10 years late, i.e., almost exactly in accord with the actual date of accomplishment of this objective.

On the other hand, "prophecies" can be "self-defeating," but a properly prepared "forecast" will point out that if certain trends continue, then (and only then) will the forecast situation come about. Obviously if men have the power to change their course of action (which is not always possible), the actual outcome of events may differ from the original forecast. Research is not required to solve this problem; rather it should be sufficient to develop and document a representative sample of cases to demonstrate that the course of technology, while not independent of society as a whole, is generally independent of individual segments of society and can neither be accelerated nor impeded very much by the separate actions of those segments.

If the technological forecasting profession can present its findings in terms of achievable technical capabilities and probable consequences (both good and bad) of those capabilities, without emotional or self-serving bias, it will markedly improve the planning process. Assumptions must be specified, the techniques used in preparing the forecast must be presented clearly, and the conclusions must be logically derived from the evidence presented. Perhaps the most obvious problem in this area is the tendency to allow value judgments to distort the forecasting process and to let value judgments influence the forecast itself. The forecaster should not let his concern with what is "good" or "bad" interfere with his judgment on what is likely. Since the forecaster's value system is his alone, no one else can possibly know the effects of the forecaster's values unless they are explicitly stated in the forecast, so that others may use their own values to judge the likelihood and desirability of the forecast events. Carried to a logical extreme, the professional should be able to present a forecast of catastrophe without expressing either joy or dismay.

In spite of all efforts by forecasters to comply with the above guidelines, technological forecasting can do little to improve the mechanisms of planning and change as long as these mechanisms are controlled by people who have made *a priori* judgments about what is good and what is bad and are more interested in compulsive adherence to those judgments than in open evaluation of possible consequences of given actions.

Prospects For Technological Forecasting

What will happen in, to, and from technological forecasting? It seems likely that one segment of the profession will continue the development of more esoteric and mathematically abtruse derivations of the current body of forecasting techniques. At random intervals new techniques, new insights, and new applications will be introduced through genius or hard work. Surveys and conferences will produce much fruit—some bitter, some rotten, and some worth all the effort. It does appear that technological forecasting is an idea whose time has come and that forecasting capabilities will increase exponentially with time. Unfortunately, we have no measurement of present capabilities, nor of the past rate of increase, so we cannot forecast when any given level of capability will be attained.

The profession will increase in size exponentially until it approaches a limit, as yet undefined, but probably less than 1 percent of the total population. Status of the profession will increase in the near-term as clamor grows for means of avoiding recurring crises and will subsequently decline from disillusion when it becomes apparent that forecasting cannot solve conflicts of interest.

The decisionmakers will call upon technological forecasting in a sporadically increasing manner, as they alternately find they cannot get along without it, and then cannot get along with it. As more and more executives come to realize that their prime function is to plan for the future, their acceptance and use of forecasting will increase. In 10 years most of the old generation who now dominate the executive ranks will have passed from the scene and the new generation—computer educated, acclimated to future shock and knowledgeable of the need for forecasting, along with all of their other bad qualities—will have had responsibility thrust upon them. Although they may never be able to measure the utility of forecasting in the decision process, they will have an understanding that it is not zero and that it is a positive quantity.

Technological forecasting will never be able to explore the future—nor will any other technique—since we must arrive on the scene in order to explore it. Prospects are increasingly good that we will be able to present alternative theories about what that future might contain and to project routes we will want to travel. We should be able to increase the probability of avoiding dead-end trails, blind canyons, and areas devoid of resources and of detouring impossible barriers. We should be able to estimate the resources required to reach some of the alternative futures and be able to guard against starvation and known disasters enroute. In this respect technological forecasting is an excellent, although by no means the only, technique for planning the exploration of the future.

Technological forecasting will continue to improve the process of planning and bringing about change. Its present rather minor role will expand to become a dominant consideration in planning. The quantitative projection of technological capabilities and resultant consequences will provide earlier warning of undesirable impacts and will enable timely evaluation of alternative courses of action. We might hope that this knowledge will help to prevent crises and, even more importantly, prevent overreaction to presumed problems and emergency surgery for chronic ills of society. The prospects of help from forecasting in the prevention of crises will require the abandonment of the principle of "brinkmanship" in internal affairs, just as it has been abandoned in world affairs because of its dangers. However, the prevention of crises with the aid of forecasting will be difficult as long as emotional, financial, and political rewards can be obtained by jumping on band wagons led by the "music men" con artists with their "76 trombones."

Technological forecasting offers additional prospects for aiding economic stability, for ameliorating some of the problems of future shock, and for moderating social reaction, if it becomes more widely used in the planning process.

Research Recommendations

The areas for research in technological forecasting that have been noted in the body of this paper may be summarized as follows:

1. Develop methods for predicting the impact of new discoveries.
2. Examine a large number of case histories to quantify the time-lag between first commercial or operational demonstration of new technologies and the beginning of significant market penetration, and to provide data on the probabilities of ultimate achievement of commercial or operational success.
3. Perform a historical analysis of several technologies to determine if any patterns exist which could be used to predict the date of occurrence of the critical events necessary for the initiation of a new technology.
4. Conduct a relevance tree examination of several established technical capabilities to identify the separate technologies that were necessary to achieve initial operational capability. Once identified, the sources of invention and development in these corollary technologies should be determined, together with estimates of attendant resource expenditures if possible.
5. Develop an analytical demonstration of cause-and-effect relationships between technological advance and economic growth.
6. Quantify rates of change in value systems and social structures based on measurable activities.
7. Identify instructional and educational needs in the field of technological forecasting.
8. Develop technical and ethical standards to judge and govern professional conduct in the practice of technological forecasting.
9. Establish an annual census of technological forecasting practitioners and of expenditures for forecasting.
10. Perform a rigorous philosophical examination of the relationship between value systems and technology.
11. Using quantitative forecasts available in the literature, evaluate each data point with respect to its significance as a measure of technological progress as a basis for the development of methods to evaluate and adjust forecasts to account for the differences in significance.
12. Develop simulations (games) to demonstrate the utility of technological forecasting in real-world situations.

13. Analyze expenditure levels required to maintain given rates of technological advance, and analyze processes for, and sources of, funding for such expenditures.

14. Collect and analyze evidence to test the alternate hypotheses that technological progress is (a) continuing to increase exponentially or (b) continuing to increase but at a constantly slowing pace.

15. Document a representative sample of cases to clarify assumptions regarding "self-fulfilling" or "self-defeating" forecasts.

FOOTNOTES

[1] Conference on Quantitative Description of Technological Change, at Princeton, April 6-8, 1951; reported in *Review of Economics and Statistics*, November, 1952.

[2] Henry Adams, *The Education of Henry Adams* (New York. The Modern Library, 1931).

[3] Equivalent to the NASA program of 1960.

Chapter 7

On Normative Forecasting

Willis W. Harman

Perhaps there are only two kinds of futures research: overtly normative and covertly normative. Perhaps the pretense of examining alternative futures with dispassionate objectivity, as if from outer space, is not only ridiculous but pernicious as well. Perhaps our confusion about the meaning and methods of normative futures research is part of a more general confusion about goals and values that we critically need to address.

The following argument will be brief and incomplete. But the issues it raises are crucial to the endeavor called futures research.

Some General Observations About Futures Research

Let us start with a few observations and assumptions about the nature of futures research.

1. *It is utility-oriented.* The basic reason for desiring to know more about the future is to guide present actions and especially to anticipate, avoid, and resolve problems. There is little or no interest in a pure, unapplied science of alternative futures—if, indeed, one can even imagine it.

2. *It is intrinsically global in approach.* There is no need here to summarize the impressive evidence that "everything is connected to everything else" and that we ignore this at the peril of being rudely surprised by "unanticipated consequences" of actions. A corollary of this observation is that global, holistic techniques are required and that methods (e.g., for technology assessment) which are predicated on simpler cause-effect relationships (even stochastic ones) will consistently prove inadequate.

Ecology provides a useful example. A study of an ecological system that attempted to start from such cause-effect links as "animal A eats plant B, resulting in condition C, etc." would get nowhere. Progress is made through invention of new global concepts such as nitrogen cycle or climax community. The needed holistic approaches must accommodate the "soft" cultural phenomena (values, attitudes, beliefs, and expectancies) as well as the "hard" and quantifiable economic, demographic, and technological data. Economic growth, for example, clearly depends upon scientific and technological development and upon changes in life styles, but these in turn are functions of such abstract societal goals as national prestige and quality of life, which in turn have both metaphysical and psychodynamic components.

3. *Bias is inevitable.* This is especially true of any single systematic approach. If it deals with quantified data, there is bias of indeterminable amount in favor of that which is easily quantifiable. If it centers around cultural and subcultural belief systems, it may slight irrational and unconscious forces. The method may be biased by an implicit image of man—as free and rational or as unfree and pushed about by his drives, habits, and social roles. It may be biased by a Malthusian pessimism or a technocratic optimism.

This unsatisfactory situation is alleviated somewhat by deliberately employing several diverse approaches, synthesizing the results where they complement one another, and being directed to further analysis where results are in conflict. A little further on we shall examine and comment on an attempt to make such a synthesis that is applicable to America today.

An important aspect of this inevitable bias is that occasioned by being immersed in a particular culture in a particular area of the globe at a particular time in history. Every culture has its blind spots, and there is no reason to suppose ours is an exception. The anthropologist studying a primitive tribe or the foreign traveler sees things that the native misses. An historian is aware of characteristics of a particular period that were not apparent to the person living at that time. We might liken the futurist attempting to be objective to a scholar of the Middle Ages trying to explore what a post-Copernican world might feel like. The training of a futures researcher needs to include, as does that of an anthropologist or a psychotherapist or a judge in a court of law, learning that quality of detachment that allows observation relatively freed from the prejudices and expectations of past conditioning.

4. *Bias is desirable.* In another sense, the output of futures research is valuable precisely because it *is* deliberately biased—toward those characteristics of the future that are relevant to the focus of interest. The art of alternative futures forecasting must be, as with any art (the writing of history included), the imposition, on the unstructured universe of data of some sort of framework for viewing. Technology assessment, for instance, is valuable to the extent that it singles out those aspects of the future that would be affected by the application or nonapplication of the technology in question.

Because futures research deals fundamentally with the future of the human community, it is useful only to the extent that it *is* normative and that it focuses on those aspects that *matter* to human beings. It would be a strange and useless kind of nutritional research that ignored the normative question of what foods are wholesome for man and the value-laden goal of good health. Similarly, the high place of economists in policy-making circles results from the normative nature of their science—their bias, if you will, toward such valued goals as high employment, price stability, and balance of payment.[1]

5. *Dissemination of futures research affects the future.* Economic forecasts affect economic behavior—that is their function. But this in turn changes the world being forecasted. Similarly, as people's thinking about the future is affected by the output of futures research, their altered behavior in turn affects that field being examined. Rational anticipation of shortages can lead to planning that alleviates the effects. On the other hand, fear of a threatening event can lead to unconscious contriving to help bring about that which is feared.

6. *Resistance plays a role in acceptance of results of futures research.* The phenomenon is well known in psychotherapy that the client will resist and avoid the very knowledge he most needs to resolve his problems. A similar situation probably exists in society, and there is suggestive evidence both in anthropology and in history that a society tends to hide from itself knowledge which is superficially threatening to the status quo but may in fact be badly needed for solution of the society's most fundamental problems.

This point, while not demonstrable in any conclusive way, can not be overstressed. The reason contemporary societal problems appear so perplexing may well be not so much their essential complexity as the collective resistance to perceiving the problems in a different way. Other societies have had their selective blindnesses and defenses against knowing—no doubt ours does, too.

Types of Approaches

We argued above for integration of diverse approaches to the future, because any single method is characterized by an indeterminable bias. Let us now examine briefly several types of approach to knowledge of the future. Our purpose is not to be comprehensive but to illustrate how the various approaches are complementary and corrective of one another's biases.

1. *Systematic generation of alternative "paths to the future."* In essence this group of approaches attempts to construct some sort of system model adequate to the kind of knowledge of the future that is desired and from this to generate descriptions of alternative futures as functions of time. There are two main types: quantitative and nonquantitative. (The latter does not ignore relevant numerical data, of course, but it attempts to reduce the bias toward emphasis on those characteristics which are most easily quantified.) Validation of the model is mainly in terms of checking how well it describes the recent past.

Among the quantitative systematic methods, econometric models and combined econometric-demographic models are probably the most familiar. Varying the various assumptions built into the model (in terms of constraints, relations between variables, parameter variations, etc.) allows generation of alternative plausible future conditions. One model that attempts to include a very wide range of socially important variables and that has received much attention is the "World Dynamics" simulation (Forrester, 1971; Meadows et al., 1972).

Jantsch (1969) argues the need of integrative system models that view man in his total dynamic-feedback natural, cultural, institutional, and technological environment. One such systematic approach to generating alternative futures that puts particular emphasis on structural characteristics is provided by Rhyne (1971). In this method a coded description of the state of the society is constructed, taking into account the sectors (political, cultural, economic, technological, etc.) deemed to be of importance. By systematically applying tests of self-consistency, continuity, etc. plausible time-sequences of these states are separated out from the much larger number of implausible sequences. Thus a comprehensive set of alternative "future histories" is generated.

2. *Extrapolative trend examination.* Extrapolative approaches are familiar in economic and demographic analyses and in technology assessment (Ayres, 1969). A well-known example of extrapolative generation of alternative futures is provided by Kahn's deviations from a "long-term multifold trend" as delineated in Table 1 (Kahn and Wiener, 1967; Kahn and Bruce-Briggs, 1972).

Daniel Bell (1973) provides an important example of extrapolative social forecasting. As an "axial principle" for understanding the industrial era, he selects "the centrality of theoretical knowledge as the source of innovation and of policy formulation for the society." To arrive at a delineation of "postindustrial society" he projects this principle ahead, along with other components of the long-term multifold trend, including two not specifically identified by Kahn:

> The decisive social change taking place in our time—because of the interdependence of men and the aggregative character of economic actions, the rise of externalities and social costs, and the need to control the effects of technical change—is the subordination of the economic function to the political order. . . . The second major historical change is the sundering of social function (or place in society, primarily occupational) from property. [p. 373]

Table 1. The Long-Term Multifold Trend of Western Culture*

1.	Increasingly sensate (empirical, this-wordly, secular, humanistic, pragmatic, manipulative, explicitly rational, utilitarian, contractual, epicurean, hedonistic, etc.) cultures
2	Bourgeois, bureaucratic, and meritocratic elites
3.	Centralization and concentration of economic and political power
4	Accumulation of scientific and technical knowledge
5.	Institutionalization of technological change, especially research, development, innovation, and diffusion
6.	Increasing military capability
7.	Westernization, modernization, and industrialization
8.	Increasing affluence and (recently) leisure
9.	Population growth
10.	Urbanization, recently suburbanization and "urban sprawl"—soon the growth of megalopoli
11.	Decreasing importance of primary and (recently) secondary and tertiary occupations; increasing importance of tertiary and recently quaternary occupations
12.	Increasing literacy and education and (recently) the "knowledge industry" and increasing role of intellectuals
13.	Innovative and manipulative social engineering—i.e., rationality, increasingly applied to social, political, cultural, and economic worlds as well as to shaping and exploiting the material world—increasing the problem of ritualistic, incomplete, or pseudo-rationality
14.	Increasing universality of the multifold trend
15.	Increasing tempo of change in all the above

*Source, The Hudson Institute (Kahn and Bruce-Briggs, 1972).

Additional alternative futures can be generated by identification of possible new trend components, either because of approaching limits (e.g., resource limitations) or because of new technological or cultural developments.

3. *Historical analysis and analogy.* At least three kinds of historically based futures analysis can be identified. One is the sort of political analysis in which nation-states or other stake-holder groups are considered as actors in an ongoing drama of forces and counterforces, goals and strivings, actions and reactions. A second kind amounts to analysis of the nature of present social dilemmas, and a delineation of possible alternative futures in terms of the ways in which the society moves to resolve these dilemmas. (Examples are Ellul, 1964; Michael, 1965; Ferkiss, 1969; Revel, 1971; and Boulding, 1964.)

Third, historical analogs can be useful, particularly in examining the hypothesis that industrialized society may currently be undergoing a profound transformation which will affect all aspects of the future. The anthropologist Anthony F. C. Wallace (1956) in a comparative study of crisis-precipitated cultural change derived a series of idealized stages through which such transformations tend to pass. Thomas Kuhn (1970) recognized a somewhat similar pattern in cases where the basic knowledge paradigm is replaced with another, such as the Copernican revolution in science. Again, studies of revolutionary change throughout history reveal typical patterns and precursors (Johnson, 1966; McEachron, 1971).

4. *Collective opinion techniques.* A fourth category of method widely used for futures studies is that of gathering and processing individual judgments

relating to the future. This ranges from surveys through Delphi (Gordon and Helmer, 1966) and cross-impact analysis (Gordon, 1968). In a somewhat different and more dynamic guise it includes games and simulations, wherein rational and intuitive judgments are acted out in time sequence in the presence of feedback.

A Tentative Interpretation of the Present Decade

It will be most useful, I think, to look briefly at a specific endeavor to carry out the sort of integrative futures research described above. The tentative findings of this effort pose a crucial test for futures research methodology if it is to be adequate to contemporary needs.

The U.S. Office of Education made a daring commitment to futures research in funding in 1967 two educational policy research centers to examine alternative futures for the nation 20 to 30 years ahead and to derive from them guidance for long-term educational policy. In the center based at SRI we developed a systematic method of generating "alternative future histories," and by the end of two years' work we had come out with some conclusions that were startling to us—and completely unusable to the Washington bureaucracy (Harman, 1970). Among the plausible "paths to the future" that emerged from the analysis, most were intolerable in the sense that no society would deliberately choose them (though they might end up there through inadvertence and ineptness). The only practical paths that led to a generally desirable future seemed to require a monumental task of social learning and a major transformation of social institutions to be substantially accomplished in the period before the mid-1980's.

Accordingly, we forecast a time of troubles and social disruption in the mid-1970's, characterized by a series of crises that would have apparent and separate causes but would in fact be more fundamentally symptoms of the massive social wrenching taking place—something like the century of irrational religious wars and disruptions that characterized the Reformation period in Europe (but compressed into a decade). If there were any validity at all to this interpretation of the data, then clearly the implications for educational and social policy were most profound.

The USOE response (not inappropriately, in view of the Washington climate of the times) was to withdraw further support of general futures research. We continued attempts to flesh out and test this picture and to relate it to social policy in other areas. I would like to summarize below results from three types of probings mentioned in the preceding section.

1. *The extrapolative approach.* Table 1, as earlier indicated, lists components of a "long-term multifold trend" that has been used effectively in taking an extrapolative look at alternative futures. One might wish to modify this slightly to emphasize some aspects that seem more important than they may have a few years ago. These could include the following trends:

1. The increasing scale of environmental impact of human activities.
2. The increasing rate of use of "nonrenewable" natural resources of minerals and fossil fuels.
3. The movement toward a single world economy with closely linked worldwide economic institutions.
4. The increasing gap between rich and poor populations.
5. The increasing subordination of the economic function to the political order (Bell).
6. The decreasing importance of private property (Bell).

Let us look briefly at some of the more important recent developments that may result in new trend components and hence in an overall shift in the long-term multifold trend:

1. "Spaceship earth" thinking, awareness of the biosphere as a life-support system, concern for environment, ecological systems, and resource control.
2. "Alternative futures" thinking, awareness of the "Faustian powers" of technological manipulation to shape the future and of the new dimensions of human responsibility for the future of the planet and for life thereon.
3. Shift from the predominance of problems of inadequate capability to manipulate the natural environment to predominance of problems of technological success (see Table 2).
4. Shift, throughout the industrialized world, from deficiency-need concerns to self-actualization concerns [to use Maslow's terms (1972)], occasioned by increasing levels of nutrition, education, affluence. (These latter are long-term trends, but the shift of need-level concerns is a discontinuous change characterized by changes in value emphases away

Table 2. Selected Successes and Associated Problems of The Technological/Industrial Era

"Successes"	Problems Resulting from Being "Too Successful"
Prolonging the lifespan, reducing infant mortality	Regional overpopulation; problems of the aged
Highly developed science and technology	Hazard of mass destruction through nuclear and biological weapons, threats to privacy and freedoms (e.g., surveillance technology, "bioengineering")
Advances in communication and transportation	Increasing air, noise, and land pollution, "information overload" vulnerability of a complex society to breakdown
Efficient production systems	Dehumanization of ordinary work
Affluence, material growth	Increased per capita consumption of energy and goods, leading to pollution and depletion of the earth's resources
Satisfaction of basic needs	Worldwide revolutions of "rising expectations," rebellion against nonmeaningful work
Expanded power of human choice	Unanticipated consequences of technological applications, management breakdown as regards control of these
Expanded wealth of developed nations	Increasing the gap between "have" and "have-not" nations, frustration of the "revolution of rising expectations"

from materialistic and status values and toward humanistic and spiritual values. Manifestations of such a shift can be found in survey results, new forms of worker discontent, new political and social emphasis on self-determination, and new styles of management.)

5. Awareness, throughout the lesser developed world and poorer groups, brought about in part by modern communication and transportation, of major inequities in access to the earth's resources and of the power to change this.

6. Beginning of the shortage of meaningful work roles brought about by automation and cybernation. The employment statistics and survey data are equivocal with regard to the seriousness of unemployment and worker discontents. Nevertheless, the evidence strongly suggests that the thrust for efficiency and productivity, for replacement of men's muscles and brains by machines, had brought about by around 1930 a lasting condition of a potential labor force far greater than needed to conduct the necessary activities of the society. Furthermore, the division of labor had been carried out to the point where many jobs were segmented to subhuman tedium. That meaningful work opportunity has come to be considered an increasingly scarce commodity is attested to by concern over unemployment and "exporting jobs," rising worker complaints over stultifying jobs (Sheppard and Herrick, 1972), inflated job-entry requirements, forced early retirement policies, featherbedding and makework practices, and generation of jobs through arms races and "pyramid building" (Theobald, 1970).

7. "New Transcendentalism"; emergence of

widespread interest in a "New Naturalism" (Yankelovich, 1972), religious and spiritual concerns, psychic phenomena and the occult, esoteric and metaphysical philosophies, meditation and other inner-exploratory techniques.

Most of these developments are related to the five dilemmas of modern society to be delineated in the following section. The major trend shifts they may lead to are suggested a little further on.

2. *Identification of fundamental dilemmas.* A second way in which we tried to discern fruitful ways of visualizing alternative futures was to pursue a long list of contemporary societal problems to their more basic forms, eventually being led to a set of five interrelated fundamental dilemmas that characterize this Nation and, to considerable extent, all of industrialized society. Alternative attempts at resolution of these dilemmas will act as shapers of alternative future states.

The five dilemmas are, briefly:

a. *We need continued economic growth but we cannot live with the consequences.* This dilemma is by now widely discussed, with frequent reference to "limits to growth" and to arguments that we have to shift toward a "steady-state economy." On one hand, there are worsening problems of materials shortage, energy supply, environmental degradation, and interferences with biological cycles and ecological systems—all associated with a continued high rate of technological and economic growth. On the other hand, there are a host of reasons that a sudden decrease in growth rate would be disastrous. Unemployment and economic depression are the most obvious. Then, too, if growth slows there will be increased pressure for redistribution of wealth and income—pressure for a "bigger piece of the economic pie" that was less in evidence when the pie was expanding more rapidly.

b. *We need guidance of technological innovation but shun centralized control.* The powers of technology to change any and all aspects of the total environment (physical, social, political, and psychological) grow ever greater. There is increasing recognition of the need for technological assessment and some form of societal control. It remains to be shown that a democratic society can anticipate technological impacts and protect the interests of the overall society and yet preserve the basic characteristics of a free-enterprise system.

c. *Ever-closer coupling between individuals and organizations appears to lead inexorably in the direction of reduced liberties and system fragility.* Modern transportation and communication and growing interdependencies make each person or part of the system more closely connected to the rest. For the individual this places his freedoms in jeopardy—the astronaut's actions are more circumscribed than those of the cowboy. For the economy this close coupling seems to call for more and more government regulation to reinforce a faltering "invisible hand" (price regulation, restrictions for environmental protection, controls to protect the consumer from dangerous drugs, etc.), which interferes with response to market pressures and contributes to such dislocations as shortages of fuel and other commodities. At the international level there is inescapable interdependence of world economies such that depression or inflation becomes "contagious." Within nations, as well as internationally, the complexity of the overall system and the various subsystems—economic, transportation, and electric power—makes them more vulnerable to breakdowns, accidental or deliberately caused.

d. *Possession of a societally supported work role is essential to the individual's sense of self-esteem, yet the economy seems increasingly unable to provide enough satisfactory work opportunities.* We listed above indications that the society considers meaningful work opportunity to be an increasingly scarce commodity. Lack of a satisfactory work role (as employee, self-employed, student, etc.) tends to result in personal disintegration (Department of Health, Education, and Welfare, 1973). Competition for jobs exacerbates racial and intergroup conflict. Employment fears also constitute a barrier to the solution of problems of environmental pollution and waste of natural resources. Economic needs may be met with welfare payments or income maintenance, but this does not deal with the need for satisfying and valued social roles and the psychological aspects of employment. The inability of the economy to provide enough satisfactory work roles to go around (except perhaps at intolerable inflation levels) is a serious flaw.

e. *The industrialized nations will find it costly to move toward a more equitable distribution of the earth's resources; to not do so may be even more costly.* As the less developed nations modernize and begin to demand their share of scarce materials, including fossil fuels, exacerbating present shortages, this pressure may become a major threat to world stability. The achievement of a level of life in accord with fundamental human dignity for the world's nearly 3 billion poor does not appear possible without continued economic growth in both developing and developed nations. And yet, as earlier noted, economic growth on the pattern of the past poses an undeniable threat to the environment and to the health of man. Furthermore, the expectations and demands of the lesser developed world may well come at such a pace that they could be met only by lowering the standard of living in the rich nations.

Several modal approaches for dealing with these interrelated dilemmas can be identified:

1. Status quo extended. As soon as we can get beyond the temporary need for controls, we go back to depending on the market to handling most decisions better than bureaucracies can. With deregulation, rising materials and fuel prices will automatically take care of mineral and energy shortages. When people insist enough on clean air and pleasant environments, the costs of environmental cleanup will be included in the price of potentially polluting goods and services. Technology can be depended upon to create more jobs than it replaces. And so on.
2. Humanist-environmentalist reaction. Abandon the ~~growth syndrome;~~ aim for drastically lowered economic and technological growth and increased emphasis on quality of life, conservation of natural environments, and a more person-centered society.
3. "New socialism," recognizing the need for more and more control over those decisions which, in a period of lower technology, smaller organizations, and a frontier-oriented economy were adequately made in the marketplace (Galbraith, 1973).

Each of these would, if it were to come to dominate the society, lead to a significantly different future. There is reason to question, however, whether any one of the three could bring about resolution of *all* the five dilemmas simultaneously or whether this would require a more thoroughgoing transformation of culture and institutions.

3. *Possible revolutionary change.* There is much that emerges in the comparisons of present times with historical examples of profound social transformations which suggests we may be well into such a revolutionary transformation:

1. Increases have been apparent in the past decade in various "lead indicators" which have been precursors of past transformational periods. These include an increased sense of alienation and purposelessness, the occurrence of violent crime, the frequency of personal disorders and mental illness, social disruptions, the use of police to control behavior, the public acceptance of hedonistic behavior, and noninstitutionalized religious behavior.
2. The "new transcendentalism" has made rapid and large gains as a cultural shift, as is evidenced by indications of value and belief shifts in survey data; widespread understanding of allusions to the esoteric and occult in current movies and plays; expanded interest in books and articles dealing with religious, spiritual, occult, and psychic phenomena and topics; increased membership in voluntary associations centering around these interests; observable influences of the "New Age" subculture on mainstream culture (from metaphysical beliefs to hair styles). A parallel development of scientific interest in exploring altered consciousness, physical correlates of subjective states, biofeedback, meditative states and mystical experience, and psychic phenomena suggests that the emergent paradigm may have staying power.
3. Social observers during the past few years have come forth with somewhat similar analyses of what they perceive to be a cultural and social transformation (Fromm, 1968; Mumford, 1970; Revel, 1971; Roszak, 1972; Markley, 1973).

A Crucial Question for Futures Research

These sorts of analyses and explorations have led me and some of my colleagues to a gradually evolving interpretation of the present moment in history—an interpretation which has evidenced some predictive capability over the past few years and which is most startling in its implications if taken seriously. I believe it merits being taken very seriously.

Few people today would doubt that, however difficult it may be to define precisely what is meant by the terms, the Copernican Revolution and the later Industrial Revolution have had a great effect in shaping the present state of society. *Our conclusion from the above arguments is that this society, and the industrialized world in general, is simultaneously undergoing a conceptual revolution as thoroughgoing in its effects as the Copernican revolution and an institutional revolution as profound as the Industrial Revolution.* Furthermore, this overall transformation is proceeding with extreme rapidity, such that the most critical period will be passed through within a decade. Whether the social structure can withstand the strain is very much at issue, and this will greatly depend on how well we can understand the nature and necessity of the transformation as we experience it. The more it is misunderstood, the higher will be the anxiety level and the more inappropriate will be the responses.

Now the validity of such an interpretation can hardly be demonstrated—certainly not at this point. But the question it poses for futures research is this: *If* such a revolutionary societal transformation were in process, of such a fundamental nature that it should affect all major policy decisions, would our methods of futures research discern this fact? Or, on the other hand, does most of our methodology subtly include prejudices from the old paradigm that would make it blind to indications of the new?

Let me elaborate a little further on the characteristics of this hypothesized transformation. A major driving force is the superabundance of indications that the industrial era is nearing its close. The period since the Industrial Revolution has been one in which man has made increasing demands on the natural storehouses of minerals and fossil fuels and has had an increasing impact on ecology and environment. The dominant social paradigm over this period has been characterized by:

1. Development and application of the scientific method, wedding of scientific and technological advance.
2. Industrialization, emphasizing achievement of efficiency and productivity through organization and division of labor, machine replacement of human labor.
3. Acquisitive materialism as a dominant cultural value.
4. Belief in unlimited material progress, drive toward technological and economic growth.
5. Manipulative rationality as a dominant theme, man seeking control over nature, positivistic theory of knowledge.
6. Individual responsibility for one's own destiny, but individual determination of the "good"; society as an aggregate of individuals pursuing their own interests.

The fundamental dilemma of industrialized society is that this paradigm, in addition to its important successes, has resulted in processes and states (e.g., extreme division of labor and specialization, artifically stimulated consumption, planned obsolescence, and exploitation of common resources) which end up counteracting human ends (e.g., enriching work roles, resource conservation, and environmental enhancement). Microdecisions made by individuals, corporations, and government agencies, in accordance with the paradigm, produce societal macrodecisions that are on balance increasingly intolerable. The five dilemmas mentioned earlier are rooted in this more fundamental predicament. The present series of crises may be interpreted as symptoms that the industrial-state paradigm is no longer viable and that the dilemmas will require a new social paradigm for their resolution.

Thus it seems likely that technically advanced societies like the United States are undergoing a major historical transformation to some sort of "post-industrial" age, characterized by diminished labor-force employment in industrial production, by increased prominence of service activities, by increased concern with value questions relating to quality of life, and presumably by institutional changes that will provide some satisfactory resolution of the fundamental dilemma described above.

At the same time, the dominant knowledge paradigm, the scientific world view, is proving to be an inadequate base on which to build a suitable science of man. Kuhn (1970) vividly describes how, in the replacement of scientific paradigms, a watershed point is reached where the accumulated weight of discrepancies and anomalies that cannot be fitted into the old paradigm tip the balance; and it becomes more profitable (in emotional as well as rational terms) to seek a new paradigm than to patch up the old. Recent experimental researches in a number of sciences, but particularly in the area of most direct challenge, psychic phenomena, strongly suggest that human potentiality is far greater and more diverse than is ordinarily realized (in particular, including abilities for which there is no conceivable physical explanation), that a far greater portion of significant human experience is unconscious than is ordinarily assumed, and that expectations and images have far more power than could be accounted for under the old paradigm. One crucial experiment by Puthoff and Targ (1974) seems to provide clear evidence of a universal telepathic capacity and the almost complete repression of awareness of this source of knowledge.[2] The implication of the experiment is that other reported paranormal abilities are probably also universal and also almost totally repressed.

The emergent paradigm will almost certainly include the old paradigm as a special case. It will reconcile, through complementary images and conceptualizations (as with the well-worn example of the wave and particle images of light), such dichotomies as materialism versus transcendentalism, free will versus determinism, and goal-seeking explanations versus reductionism. Thus it will be freed from the positivistic reductionistic bias that characterizes much (though not all) of modern science. In it the vast range of reported psychic phenomena (e.g., telepathy, clairvoyant perception, abnormally rapid healing, precognition, retrocognition of other lifetimes, teleportation, "thought photography," and other forms of psychokinesis) could be accommodated as possible but normally repressed happenings.

These two components a profound transformation of the social and economic institutions and a replacement of the dominant knowledge paradigm of the society, are interdependent. The increasingly obvious troubles of the industrialized world prepare the ground for the new knowledge paradigm to take root. And the "new transcendentalism" of the emergent knowledge paradigm brings value emphases that contribute to the growing challenge to the legitimacy of the industrial-age institutions and incentives.

It would be folly, and inappropriate to this discussion, to attempt to guess the precise forms of these institutional and cultural changes. Certainly some of the characteristics could be identified, such as the predominance of an ecological ethic and a shift from a consuming society to a frugal society. Very difficult problems lie ahead—e.g., the reconciliation of lower material growth rates with avoidance of mass unemployment. If the postulated transformation is really underway, we can anticipate a troubled transition period characterized by extreme confusion, anxiety, social disruption, and economic decline. This near-term future is probably not very amenable either to rational planning or to accurate forecasting.

The Future of Futures Research

We return to the issue. If this transformational interpretation and forecast of the near- and midterm future is not completely fanciful, if the arguments supporting it make a plausible case, then will our futures research illuminate us with regard to the choices it poses?

If there is to be an affirmative answer to this question, it follows that futures research needs to be well supported in the short term, since that is when the illumination is most urgently needed. It follows further that certain emphases and priorities are appropriate, specifically:

1. *Exploratory research.* There needs to be an emphasis on support of exploratory research. This should encourage diverse efforts and attempts at synthesis of results from diverse methods. There needs to be recognition of the aesthetic and intuitive element in futures research, and support should not be concentrated on that which is methodologically neat. Objectivity is most difficult to come by, especially in a time of fundamental social change, and the strategy should be to seek objectivity through diversity.

2. *Focused research.* There needs to be an emphasis on research that is focused on alternative resolutions and attempts at resolution of society's fundamental dilemmas. Integrity is the quality to be sought here, not methodological sophistication. When a paradigm is failing is just the time it may generate the most sophisticated methodologies.

3. *Explicitly normative.* There needs to be an emphasis on futures research which is explicitly normative, which in the best tradition of utopian thinking attempts to delineate an image of a workable and desirable midterm future that we can get to from here. When faith in the old gods is weakening, when the old structures are threatening to crumble and the new cannot yet be discerned, dependable maps of alternative paths to the future are especially needed. If the maps cannot show final destinations, they need at least to identify a few paths that promise to be tolerable along the way and appear to head in a desirable direction. (Some recent Malthusian and Ellulian forecasts provide no such options.) The more difficult are times in the near-term future, the more crucial will be a vision of what is worth building and where is worth going to.

This is not, of course, to set the futures researcher up as some sort of technocratic judge of what is good for the society. The aim, instead, is to help the society better judge what is good for itself. Seldom has the ancient dictum been more pertinent, "Where there is no vision, the people perish."

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FOOTNOTES

[1] Polak, 1969, describes the recent battles among economists over the issue of whether economics should become a normative scien[illegible] and the contemporary arguments among social scientists and futurists on the same point.

[2] This experiment depends upon the discovery that if a stroboscopic light at about 16 flashes per second is shined in a subject's eyes, a characteristic alpha component appears in his electroencephalogram. In the Puthoff-Targ experiment at SRI, two remotely isolated subjects are used, some prior means of rapp[illegible] having been established between them. The light is flashed in [illegible] subject's eyes, and the other is asked to guess whether, in a given ti[illegible] interval, the light is on or off. He is unable to guess better than a chance basis, but the telltale alpha voltage component appears in his EEG. The important deduction is that unconsciously he knows with a certainty, in an extrasensory way, when the light is in the other person's eyes—even while he is denying such knowledge to his conscious mind.

Chapter 8

Forecasting in Economics: State-of-the-Art, Problems, and Prospects

Victor Zarnowitz

Concepts and Distinctions

Economic forecasts are statements about economic aspects of unknown and uncertain events. In its broadest meaning, this would include inferences from known to unknown events regardless of whether (1) the latter precede, coincide with, or follow the former and (2) the latter refer to the past, present, or future. However, in this chapter forecasts are defined in the narrower and more conventional sense, namely, as propositions that always concern the "future," i.e., reach ahead in time from the "present" which is the informational jump-off period for the predictor. Because data collection and processing take time, that point of departure or base of the forecast is itself usually located in the recent past, so that the calendar-time "present" would already belong to the "future" that must be predicted; but the forecasts generally reach beyond the present into the chronological future proper. The deductions, then, are as a rule from the earlier to subsequent occurrences, and the latter extend beyond the point of time when the statement is made.

Clearly, it is the future that is of primary concern to the forecaster and his audience in economics and other social sciences, as in meteorology or technological innovations. Information about the past is often inadequate, but it is virutally always richer and firmer than whatever may be "knowable" about the future. In fact, inferences from the past are the sole basic source for the expectations of those events that are beyond the forecaster's control. Furthermore, the accuracy of these expectations is certainly a major determinant of the quality of plans and decisions relating to factors over which the maker or user of the forecast does have substantial control.

In its scientific uses, the word "prediction" has commonly, though not inevitably, the general meaning defined at the outset in this chapter; but this embraces forecasts of future events, so no strict terminological distinction is introduced here. Another useful differentiation is between forecasts of individual events and those of certain classes or sets of events, and "prediction" is sometimes applied broadly with reference to the latter. But again this usage is not generally accepted, and I shall treat the terms forecast and prediction for the most part interchangeably as in everyday language.

No strict identification of what is meant by things "economic" need be attempted at this point, fortunately. The forecasts to be discussed refer to processes or activities that are commonly understood to belong in the economic sphere and to their statistical representations which are generally categorized as economic time-series data. Thus, in these applications at least, the boundary between economics and other social sciences appears to be fairly clear.

Economic as well as other forecasts vary greatly in form and content: they can be qualitative or quantitative, conditional or unconditional, explicit or silent on the probabilities involved. A minimum requirement, however, is that the forecast at least in principle must be verifiable. Trivial predictions that are so broad or vague that they could never be falsified merit no consideration, and the same applies to predictions that are rendered meaningless by relying on entirely improbable assumptions.

One can further distinguish between single and multiple predictions, depending on whether the forecast refers to one particular or several interrelated events; and between point and interval predictions, depending on whether the forecast identifies a single value or a range of values likely to be assumed by a certain variable. These categories overlap in various ways, and some of their possible combinations are interesting. For example, an unconditional interval prediction can be viewed as a set of conditional point predictions; that is, the forecaster estimates the range of probable outcomes by setting limits to the variation in the underlying conditions.

Looking into the future, economic forecasts may be classified into short run (with spans or distances to the target period of up to one or two years), intermediate (two to five years), and long-term (relating to more persistent developments or distant occurrences). Random events and seasonal changes influence economic variables and relations most forcefully over relatively short periods (measurable in days, weeks, or months); cyclical processes work themselves out much more slowly (in years); and trend factors account for "secular" developments, notably economic growth phenomena of long duration (decades). Forecasts spanning periods limited to several months or quarters are attempts to predict the more systematic components of near-term economic changes—seasonal and, particularly, cyclical—which must be isolated from shorter irregular movements. Intermediate forecasts with spans of several years would pay more attention to trends, while aiming also at any broad cyclical swings in the target variables. Long-term forecasts in economics are essentially trend projections that abstract from cyclical as well as shorter, seasonal and erratic, changes.

Sources and Uses

Forecasts in all ranges are made for purposes of business planning and of aiding economic policies of governments. Business forecasts deal largely with the near future because this is both what is most needed and what stands a relatively good chance of success in the reduction of avoidable business risks. However, in some areas, such as planning new industrial plant construction or acquisition of new business, projections of rather long developments are required and made with apparently increasing (but undoubtedly quite varying) boldness and sophistication. With the recent emphasis on growth, whether viewed positively as economic stimulant or negatively as ecological depressant, long-range forecasts also are gaining ground as tools of governmental and other organized social planning and decisionmaking.

The principal scientific and academic use of forecasts is in testing the various hypotheses, models, and methods employed by the forecaster. Predictions of economic change also can serve as data for studying the ways expectations are formed, transmitted, and revised. In all these research applications, too, short and long forecasts alike are of interest. However, long forecasts are particularly difficult to verify and evaluate, because much time must lapse before they can be confronted with "facts." Much change in the state of the world is likely to occur meanwhile, and the assumptions of the forecast may well be invalidated.

Important generalizations of economic theory typically imply qualitative conditional predictions; for example, the "law of demand" predicts that a decline in the price of a good will lead consumers to purchase more of the good. Such relations help to formulate forecasting models and to test the general consistency of these models with the implications of the theory of the (presumably prevalent) rational economic behavior. But specific quantitative prediction, which is the main subject matter of this essay, requires many other inputs—judgmental, historical, and statistical—in addition to the general insights of economic theory.

In the history of economic thought one also finds broad predictions of things to come which are essentially empirical hypotheses based on assumptions that may only have been valid initially or not at all. Secular forecasts by some classical and socialist economists provide several major examples, such as the law of historically diminishing returns, the Malthusian population principle, and Marx's projections of a falling rate of profit and increasing pauperization and crises. History dealt harshly with some of these prognostications and left some others untested by events uncertain or of dubious interest.

The verdicts of history on recorded forecasts of economic (and other) trends and events can in general be ascertained retrospectively, though not always unambiguously. The influence of forecasts (expectations) on human attitudes and actions and hence on the course of history is much murkier. There are sharp contrasts between the realms of Clio and Delphi, but also important if subtle interactions. Unlike history, which deals with presumably knowable but immutable facts, forecasting is concerned with unknown but probable and often partially controllable events to come. It is a guide to thinking about and planning for the future, a means whereby one tries to impose some mental discipline on what is inevitably hypothetical and conjectural.

Forecasting has not only these various bona fide purposes and uses but also some dubious ones. Some forecasts are at least occasionally and secondarily used as a means of communicating intentions and influencing opinion, which may bias them or make them otherwise questionable. The extent and effects of such practices are, not surprisingly, difficult to appraise. In macroforecasting (for the economy as a whole) there seems to be little scope and reason for outright and deliberate misuses. This is so in part because much of the time there is sufficient consensus among the professionals in this field, whose views are widely disseminated, for the extremely optimistic or pessimistic forecasts beyond the range of the consensus to be heavily discounted. Microforecasts, especially of company sales and profits, can affect the public's expectations of the firm's value and hence stock prices; and they would seem to be less subject to knowledgable ex ante evaluations by outsiders; they are therefore much more exposed to possible manipulation and distortion.

Economic forecasting in the United States is a sizable and highly diversified "industry" which has grown rapidly in the past-World War II period and will probably continue to grow. A large majority of its members are business economists whose main function is to provide information that should help to improve managerial decisions. Other forecasters work for government agencies, labor unions, and trade associations. A relatively small but influential group consists of economists who also teach and study in the Nation's universities and research institutions. In other major industrial countries with large business enterprise sectors, private macroeconomic forecasts are as yet much less abundant and ambitious, but trends similar to those in U.S. forecasting are here and there detectable and apt to assert themselves generally.

Specific quantitative prediction is of much more recent origin in economics than prediction in the general sense (which, as already indicated, is among the oldest products of economic thought).[1] Systematic numerical forecasting of economic time series had to await the development of empirically oriented research and its statistical and mathematical tools. Forecasts of economic conditions not only grew greatly in numbers over the last two decades, but they also became much more definite and detailed. Vague, hedged, or purely qualitative predictions of "what's ahead for business" are still quite common, but they no longer dominate. Also, attempts are increasingly made to predict the course of the economy over a sequence of short periods, e.g., the four or six quarters ahead, and this represents a particularly ambitious, "dynamic" type of forecasting.

These developments reflect an increased and diversified demand for economic forecasts. Business management has a very large share in that demand, and its general preference has been for unconditional, specific, numerical predictions. Government policymakers constitute another sizable source of the demand, but their main need is for forecasts conditional upon alternative policy courses.

Changes contributing to the growth and specialization of forecasters' output occurred also on the supply side of the market for new "economic intelligence." The rapid development of electronic computer technology accelerated greatly the rate of output of economic data (the raw materials for the forecasting process). Also, the development of large-scale econometric forecasting models could hardly have gotten anywhere near its present stage without the computer.

However, it is well to add at once a caveat: the proliferation of the forecasts need not inply that their quality has greatly improved. The evaluation of forecasts, which will be discussed later, apparently lags far behind the production of forecasts. The main reason for this is probably that the latter activity is more attractive or rewarding than the former. In any event, there is also much hard work yet to be done on problems of the proper criteria and methods of forecast evaluation.

The hazards of economic forecasting are well known. They are often compounded by the fact that many a forecaster serves a number of different masters. If he works for business or government, much of his output is communication to the administrator who employs him, but some of it is being done for the outside world—peers, professionals, and the interested public. Again, an econometric model developed at a university may serve as a basis for scientific work and also as a source of forecasts circulated to business subscribers. Aggregate forecasts by the economic staff of a corporation are a major input to micropredictions of sales and the like, and are also used in the company's publications and speeches by its executives. The forecaster usually faces all sorts of conflicts of evidence and opinion, which his essentially conjectural work must often resolve by various compromises. The multipurpose nature of many forecasts may complicate the situation considerably by bringing forth some conflicts of interest as well.

Types and Methods of Forecasting

Economic forecasts can be classified by several different criteria. One is the degree to which they use formalized methods vis-a-vis informal judgment.

According to this criterion a whole gradation can be established, from forecasts that are "purely judgmental" to those produced by fully specified and strictly implemented econometric models. However, judgment in the broad sense of the word is of course a necessary ingredient of all types of prediction; it must be used to select the data and methods of analysis as well as in the interpretation and evaluation of the results. An econometrician cannot and must not avoid it either—even after his model, whatever its size and complexity, is ready for applications to forecasting.[2] To be sure, the proper role of judgment is to complement, not to substitute for, a competent economic and statistical analysis. Informed and trained judgment can be most valuable to a forecaster, but it should be recognized that its incidence depends on personal talent and experience rather than on any well-defined technical and transferable skill.

Many forecasts, particularly from business sources, are not based on formal models and do not disclose the underlying assumptions and methods. Some are likely to be little more than products of intuition, yet often they are numerical point forecasts. However, there is no general presumption that the informal forecasts are largely hunches (which can only rely on luck for success). Rather, the better ones among them, of which there probably are many, originate in the application of various analytical techniques (as well as judgment) to diverse and substantial bodies of information.

Judgmental inferences from data samples involve probability distributions. It would be highly informative if forecasters stated the odds they attach to the expected outcomes. Regrettably, this practice (which is frequent in weather forecasts, for example) appears to be seldom followed in practical business forecasting. Complete probabilistic distribution predictions are altogether rare, but some have recently been introduced in a regular survey of macroeconomic forecasts.[3]

Another criterion is the degree to which the forecasts rely on projections of past behavior of the variable to be produced vis-a-vis relationships among different variables. According to this criterion forecasts would be divided into a class of diverse "pure extrapolations" restricted to the history of a single variable or process x, a class of outputs of different equations in which x is a lagged function of other variables $(y, z, \ldots)$, and a class of mixed forms in which x_t depends on x_{t-i} as well as on y_{t-j}, z_{t-k}, etc. (where all kinds of discrete or distributed lags may be involved).

Still another distinction is between forecasts constructed by a single source, which could be an individual or a team (say, the staff of a business or government agency), and forecasts derived as weighted or unweighted averages of such predictions. The latter category would include both small-group forecasts and large opinion polls. Of particular interest here is the degree to which the forecasts are macropredictions from a single source or averages of macropredictions or aggregates of micropredictions from different sources. For example, a forecast of total business capital expenditures (say for the United States in 1978) may be: (1) a product of one person A; (2) some average of global predictions by individuals A, B, and C, who may or may not interact; (3) the aggregate of expectations or plans of business concerns about their own capital outlays, based on a survey; or (4) a composite of a number of different forecasts of the above kinds.

In fact, the economic forecasts that are actually proffered for business and public policy uses are generally the products of judgmental combinations of various types of prediction. Thus a forecast of GNP and its major components may incorporate extrapolation, relations of the target series to known or estimated values of other variables, external information from surveys of businessmen's and consumers' anticipations and government budget estimates, and the judgment of the forecaster. Pure forms of any type—mechanical applications of any well-defined technique—are used more often as benchmarks of predictive performance than as *ex ante* forecasts proper. This applies to each of the currently most relevant forecasting procedures, which can be broadly classified as (1) extrapolations, (2) surveys of intentions or anticipations, (3) business-cycle indicators, and (4) econometric models.

Any detailed consideration of these methods would exceed the scope and intent of this chapter, but some comments on each of them are in order.[4]

Extrapolations. Since these techniques relate the future values of a series to its own past values, they have little to do with economic theory, which deals largely with relations between *different* variables. However, in some cases the theory does have testable implications about the statistical structure of the economic process represented by the series, which may be useful in this context.

Extrapolations vary a great deal technically, from very simple to very complex forms. The simplest "naive models" project forward the last-known level of the last-known change in the series, they are employed mainly as minimum standards against which to measure the performance of forecasts proper. Since trends are common in many economic time series because of the pervasive effects of growth in the economy, trend extrapolations often provide more effective predictions and are therefore more demanding as criteria for forecast evaluation. Particularly in application to long-term forecasts, trend fitting and projections are widely used. Strictly periodic, repetitive fluctuations are, like persistent trends, relatively easy to extrapolate; stable seasonal movements would often be approximately of this type.

For sequences of successive point predictions, a common form of economic forecasts, the requirement of a good forecast is that it predict well the systematic movements, not that it predict perfectly the actual values of the series in question. Economic time series as a rule contain random elements, and the forecasting errors that are traceable to very short random movements must be accepted as unavoidable. Such "shocks" as an outbreak of a war or a strike started without advance warning cannot themselves be predicted individually with the tools of economics and statistics, although their effects on the economy are, of course, the proper concern of the forecaster. In probabilistic predictions, which aim at the distribution of unknown parameters and outcomes rather than at point forecasts of future events, the consequences of the shocks and other random errors are an important part of the system to be analyzed.[5]

It is the cyclical fluctuations in economic processes—recurrent but nonperiodic and varying greatly in duration and amplitude—that produce the greatest difficulties in short-term forecasting. Smoothing techniques can reveal the past patterns of systematic changes in deseasonalized series, and extrapolations can help the forecaster in his task to the extent that they preserve these patterns and to the extent that the patterns continue to apply. But in regard to economic and related social events, the future seldom reproduces the past without significant modifications. Thus extrapolations are often very good in predicting trends but incapable of giving dependable signals of cyclical change and particularly turning points.

The best extrapolative models are statistically sophisticated products of pure time-series analysis as represented by the recently developed techniques for optimal characterizations of discrete linear processes.[6] Predictions with these models establish rather high standards of accuracy for the economist's forecasts.

Surveys of anticipations or intentions. The collection and evaluation of expectational data for the U.S. economy progressed at a rapid pace in the last two decades. The data relate to future consumer purchases (primarily of automobiles and household appliances), planned or anticipated business expenditures on plant and equipment; business expectations about "operating variables" such as sales and inventories, and government budget estimates. Similar surveys have spread recently in other major industrial countries.

Business expectations have been classified into intentions (plans for action where the firm can make binding decisions), market anticipations (relating to the interplay between the firm's actions and its suppliers, customers, etc.), and outlook (concerning conditions which the firm cannot significantly influence, but which do affect its markets). For example, new appropriations or plans for capital outlays fall into the first category; sales forecasts, into the second; and forecasts of the general economic situation in the United States and abroad, into the third.

Consumer intentions to buy are in principle akin to business plans to acquire productive resources, but in practice they are often more vague and attitudinal and usually less firmly budgeted. Government business estimates also represent intentions to spend (of the administration, subject to legislative approvals). In government, as in many large business companies, the process of forming "expectations" or forecasts is often highly decentralized and complex.

It is plausible that the accuracy of expectational data will tend to be higher the greater the degree of control that those holding the expectations have over the variable concerned. This suggests that the predictive value of intentions on the whole should be greater than that of market anticipations and that the latter in turn should be more accurate than the outlook forecasts. There is some evidence consistent with this view, notably the fact that business anticipations of plant and equipment expenditures have a much better forecasting record than business sales anticipations.[7] However, such comparisons are sometimes modified by other relevant factors, such as the variabilities of the predicted series and span of forecast. Predictions of a very stable aggregate, classifiable as outlook, may be more accurate than market anticipations for a variable which is highly volatile. Surveys looking far enough ahead, even for largely "controllable" variables, will be in the nature of market anticipations and less of intentions, and they may yield worse forecasts than outlook surveys for the very near future.

Expectations presumably tend to draw on historical evidence, such as extrapolations and inferences from observed relations, between different economic variables. But they may often, as a result of expert insight or mere intuition, include some additional information not contained in any patterns of the past. Hence, even where expectations are not very efficient when used alone as a direct forecast, they may still have a net predictive value as an ingredient in a forecasting process that combines expectational with other inputs. Inside or expert knowledge is particularly valuable in forecasting variables the successive changes in which are largely independent, where one's ability to forecast such changes is not significantly enhanced by any extrapolations of the past movements.[8]

In long-term forecasting, informed judgment or expectations play a major role along with extrapolative techniques. In large part, these forecasts are growth projections, which have been described as tools for exploring economic potentials. They are not intended to provide predictions of actual conditions in a distant year, but rather estimates of likely conditions under some specified suppositions. Alternative projections are often made, based on assumptions of several different paths of economic developments within the range considered plausible. Essentially, these forecasts are concerned with trends of the economy at full employment, and they make large use of projections on the supply side, of population, labor force, technology, productivity, etc.

Business cycle indicators. These are time series selected for the relative consistency of their timing at revivals and recessions in general economic activity, with consideration being also given to other criteria, namely, their economic significance in the business-cycles context, statistical adequacy, historical conformity to general movements of the economy, smoothness, and currency. The indicators are used in analyzing and forecasting short-term economic developments generally, but mainly for predicting the cyclical turning points. Several successive selections, based on studies of hundreds of economic time series and reviews of the results, were made at the National Bureau of Economic Research.[9]

The indicators are cross classified by economic process and by characteristic timing, i.e., according to whether they tend to lead, coincide, or lag at business cycle peaks and troughs. The choice of the series in each category, while based in the first instance on historical evidence, is also generally supported by economic theory and logic. Thus the aggregative series on production, employment, and income measure approximately the general level of economic activity whose major fluctuations are defined as the business cycle. Not surprisingly, therefore, these comprehensive indicators are "roughly coincident." The leaders include variables that anticipate production and employment, such as the average hours of work per week, job vacancies, new building permits, new orders for goods made in response to prior offers or commitments to buy (mainly for durables and, especially, capital equipment), and contracts for industrial and commercial plants. The execution of new investment orders and contracts takes time, so business expenditures on plant and equipment is a roughly coincident or slightly lagging series.

Another type of sequence arises from the fact that a stock series often undergoes retardation before reversal; hence the corresponding flow series (or rate of change in the stock) tends to turn ahead of the stock. Thus inventory changes lead at business cycle turns, while total inventories lag. Still other sequences relate downturns in some indicators to upturns in others. For example, the decline in inventories lags behind and is a possible consequence of the downturns in such comprehensive measures of economic activity as GNP, industrial production, and manufacturing and trade sales. But the downturn in inventories also leads, and may be contributing to, the later upturns in these and other series (as the need for the stocks to be ultimately replenished will stimulate orders and help to bring about the next business recovery). Such considerations suggest that the coinciders and laggers are not merely useful as confirming indicators; they also play an active role as links in the continuous round of business cycle developments.

Some economists, following the massive work on money by Milton Friedman and his associates, see in changes in money supply a major casual factor in business cycles.[10] In view of this hypothesis, which is in the center of a vigorous debate on issues of economic theory and policy, the series of rates of change in the major monetary aggregates deserve a special emphasis. These indicators show long leads and rather pronounced irregular component movements.

On the whole, the lead time provided by the indicators is short, so that often, especially when the economy shifts its course in a relatively abrupt manner, the best obtainable result seems to be to recognize the cyclical turning point at about the time it is reached. Even this is not a negligible achievement, however, since revivals and recessions are not generally recognized as such until several months after they have occurred. Thus the use of indicators does help significantly to reduce the lag in the recognition of major changes in the business cycle.

The greatest difficulty in using indicators for forecasting purposes arises from the fact that the trend and cyclical movements in these series are typically overlaid and often obscured by short-period variations, partly of seasonal but mainly of random nature. Hence the need, but also the persistent and hard problem, to establish on the current basis the direction in which the indicators are moving or the dates of their cyclical turning points. The leading indicators, in particular, are highly sensitive to all kinds of short-term influences and not only to the forces making for the general cyclical movements. They have anticipated marked retardations in business activity as well as full-scale recessions (absolute declines). It is apparently very difficult to distinguish in advance between these different episodes, even for highly skilled analysts: certainly no mechanical, replicable method of applying the indicators has been able to do the job.

Individual indicators occasionally fail to signal the approach of a general business reversal, and their leads often vary a great deal over time. The evidence of groups of indicators is considered to be on the whole more reliable than the evidence of any single indicator. Accordingly, the degree of consensus in the behavior of these series attracts considerable attention from forecasters. Two measures of the consensus are in use, namely: (1) composite indexes for groups of indicators classified by timing (and, for the leading series, by economic process), with amplitude and trend adjustments designed to allow for some differences in dimension and behavior, and (2) diffusion indexes which show the percentage of series in a given group that are expanding in each successive time period. These indexes are now available regularly, along with a large number of individual cyclical indicators and other economic time-series and analytical measures, in *Business Conditions Digest* (BCD), a compendium of charts and tables published monthly by the U.S. Department of Commerce, Bureau of Economic Analysis.

Economic fluctuations have definitely become milder in the post-World War II period in the United States and, even more, in other highly developed market economies (Western Europe and Japan), where slowdowns in the rate of growth of total spending and income have largely replaced sustained declines in the levels of these aggregates.[11] Hence the "growth cycles" that show up only in the rates of growth and in the deviations from trends of the comprehensive series measuring aggregate economic activity have recently come to attract a great deal of attention. However, many features of the historical business cycles (sequences of generalized expansions and contractions) reappear in the recent growth cycles. This applies, in particular, to the timing sequences involving the leading and the other indicators.[12]

What this suggests is that the indicator approach is likely to continue to be useful in a world in which the probability of growth cycles greatly exceeds that of the historical (recession-prone) business cycles. However, that approach, having been devised to observe the latter type of economic fluctuations, needs to be adapted for a more efficient observation of the former.

Econometric models. These are systems of equations designed to represent quantitative relationships among economic variables. Here our interest is in those models that concentrate on major aggregates, such as national income and product, consumption, investment, employment and unemployment, the price level, etc., and also aim at tracing the behavior over time of these variables. These models serve the purposes of explanation and control as well as prediction: they are employed in testing macroeconomic hypotheses, simulating the effects of alternative policies, and forecasting changes in the economy.

The equations in econometric models describe the behavior of consumers, producers, investors, and other groups of economic agents; they also describe the market characteristics, institutional conditions, and technological requirements that guide and constrain economic action. The variables are selected to represent the major systematic factors entering each function, and their net marginal effects are treated as unknown (as a rule, constant) parameters. The latter are estimated on the assumption that all principal determinants in the relationship have been properly identified, leaving only random disturbances with expected values of zero. Ideally, these residual disturbances' terms should be small, not serially correlated, and independent of the systematic factors.

All these so-called structural equations, then, are stochastic, i.e., supposed to hold only approximately up to a random error. In contrast, the remaining equations of the model are accounting "identities," which are based on definitions and are therefore supposed to hold exactly and include no unknown parameters.

The variables whose values are determined by the model are called the jointly dependent or current endogenous variables; their number equals that of the equations in the complete system. To solve for these variables, each of them is expressed as a function of the estimated structural parameters, the disturbances, and the predetermined variables. The latter comprise both the values of the exogenous variables determined outside the model and the lagged values of the endogenous variables given by the past operation of the system (or, for some purposes, by outside estimates). Government policy and foreign countries' actions, demographic changes, and other "non-economic" variables are usually treated as exogenous.

Models which contain lagged endogenous variables are dynamic in the sense that the values they predict depend on the historical values generated by the system. In other words, the solution of such models can yield trends and cyclical movements even without changes in the parameters, disturbances, or exogenous factors Nonlinearities in variables (but not in parameters) appear in most of the contemporary full-scale models.

The comprehensive models for the economy as a whole are typically simultaneous equation systems, i.e., they are based on the concept of general interdependence and thus include elements of interactions among current endogenous variables. Much simpler single-equation and recursive models can be used to analyze relationships with unidirectional flows of cause and effect: any endogenous variable depends on predetermined variables only.

It is the jointly determined estimates of the current endogenous variables, all of which are functions of the predetermined variables, with given parameter estimates and disturbances typically (but not always) assumed to be zero, that represent the forecasting output or the "reduced form" of the model. If the predetermined variables are taken as given, say at their reported values, *"ex post"* predictions conditional upon these data can be made from the reduced form. Unconditional *"ex ante"* forecasts of the endogenous variables are obtained when the unknown values of the predetermined variables are themselves predicted (which for the exogenous factors necessarily means prediction outside the model).[13]

Thus unconditional forecasts could have substantial errors because of wrong projections of exogenous variables, even if the specification and solution of the model were essentially correct. The accuracy of the conditional forecasts, on the other hand, depends (apart from any effects of errors in the data inputs) only on the errors that occur in the construction and solution of the model. These may arise because of (1) incorrect specifications of economic relationships, (2) deficient methods of statistical inference, (3) sampling errors in the parameter estimates, and (4) errors resulting from the assumption that the disturbance terms vanish.[14]

The severity of errors due to sampling variations and inefficiency of estimation can be evaluated by means of statistical inferences as to the probabilistic meaning of the parameter estimates, the goodness of fit to the sample-period data, and the autocorrelation characteristics of the disturbance terms. Misspecifications, however, are clearly the decisive source of errors as far as the model itself is concerned, and they are difficult to identify and assess. Theoretical considerations and the predictive value of the model provide the main clues in dealing with these matters, but large and complex models, in particular, pose many detailed specification problems that theory and empirical research have not yet been able to resolve.

The econometric models that have had been developed in the recent past for the economies of the United States and many other countries vary greatly in size and complexity. To illustrate this diversity, among the smallest U.S. models is one consisting of four structural equations and an identity. In contrast, the largest U.S. model, developed by 20 economists for the Brookings Institution, contains 230 equations and 104 exogenous variables. This and other late models are quarterly, whereas their predecessors were generally annual and as such considerably less useful for purposes of current economic analysis and forecasting.

For the most part, the models are built around the structure of national income accounts, a basic framework that is widely used in noneconometric analyses and forecasts as well. Most models also incorporate major notions of Keynesian economics such as the "multiplier" effects on spending and income of changes in government purchases and private investment expenditures, the dependence of personal consumption on disposable income of households, and the functions linking tax revenues and imports to income and some other variables. A few models give a good deal of attention to the monetary-financial sector of the economy, and thus to the consequences of monetary policy, but most place greater emphasis on the fiscal policy, i.e., the role of government purchases, taxes, and transfer payments.

The decisions on the precise form and content of the equations (e.g., whether to estimate the model in terms of levels or changes, what discrete or distribute lags are to be used, etc.) often result from much experimentation with alternative hypotheses and specifications. While it is usually impossible to avoid such explorations without sacrificing possible gains of information, the outcome for the sample situation may thereby be significantly biased. This underscores the need for predictive tests of the models, based on applications to nonsample periods.

Evaluating the Forecasting Record and Prospects

Forecasts must be properly evaluated if their makers are to learn from past errors and if their users are to be able to discriminate intelligently among the available sources and methods. For a long time efforts to test economic forecasts systematically have been lagging behind the production of the forecasts, but the resulting serious inbalance is being gradually corrected.[15]

In principle the decisionmaker who knows the costs of acquiring and using the forecast and the returns attributable to it should be able to evaluate the goodness of the forecast exactly in retrospect. But such knowledge is rare, because is it difficult and costly to develop fully the information required. However, it is often possible to measure meaningfully the *accuracy* of forecasts by comparing the predicted with the subsequently recorded actual outcomes; and it seems natural to view the overall accuracy of a given set of predictions as the principal single aspect of their quality. Other important aspects of quality are more difficult to assess, so users, confronted with comparable forecasts, tend rationally to prefer those from a source which has proved significantly more accurate in the past.

Ideally, forecasts should be unbiased and efficient; that is, their *average* error should be approximately zero (no systematic under- or overestimation), and there should be no significant correlation between errors and forecast values. This is because the forecaster should learn from past errors how to avoid bias and inefficiency. In practice, however, many economic forecast series are not long enough or not consistent enough to permit successful measurement of, let alone correction for, the "systematic" error components.

Whenever possible, it is desirable to study the average performance of a forecasting source, model, or method over a reasonably long stretch of time, including diverse economic developments. Isolated successes or failures in forecasting can be due more to chance than to the quality of the forecaster's skills and tools. Also, individual predictions can suffer from excusable errors in assumptions about exogenous, perhaps noneconomic, events; or, conversely, they can happen to be relatively precise in spite of being based on wrong premises (i.e., they can be "correct for wrong reasons," which usually involves offsetting errors). However, if a forecaster's record is poor on the average over time, it is unlikely to be so only because of consistently wrong assumptions (and, if it were, this would hardly be an acceptable excuse).

Judging from the results of recent studies,[16] forecasts of GNP and industrial production made late in the year for the year ahead have for the most part been more accurate than simple extrapolations of the last level or change and projections of trends and of relationships between present and past values of the series in question. As a very general rule, the averages and dispersion of errors in economic short-term predictions (for one to six quarters ahead) increase with the time span to the target period. Forecasts for one to three quarters ahead are in most cases better (in the sense of having smaller average errors without regard to sign) than many types of mechanical extrapolations, but the longer forecasts within the examined range are often worse than the products of the more effective of the employed benchmark models (autoregressive and trend projections). Presumably, each of the various ingredients or sources of forecasts, such as signals from anticipations surveys, cyclical indicators, econometric relations, and the forecaster's judgment, are subject to a deterioration with the lengthening of the predictive span.

Many forecasts tend to underestimate the growth of the economy, that is, they predict too small increases in such comprehensive series as real GNP, employment, and industrial production. Inflation also is often underestimated (which adds to the errors of GNP and components in current dollars), and changes in price level indexes are in general rather poorly predicted. The frequency of such errors suggests to some observers that forecasters tend to be too timid or cautious (which, in a growing economy, much of the time would amount to a relative pessimism on real growth but optimism on inflation).

Errors in predicting percentage changes in personal consumption expenditures tend to be considerably smaller than those in corresponding forecasts of private investmest outlays, while errors in predicting government spending tend to be of intermediate size. The forecasts of total GNP are often better than those of most major GNP components from the same source, which indicates a partial cancellation of errors for the components In sum, smoothly growing aggregates, such as consumption of nondurable goods and services (which account for a large part of GNP), are in general much more accurately predicted than the more volatile series, such as several major investment categories, particularly change in inventories. To be sure, the former variables are easier to predict than the latter, being much more amenable to satisfactory extrapolations Thus good trend projections for consumption are better than some economists' forecasts, which suffer from underestimation errors. Changes in the less growth-dominated and more fluctuating series are seldom systematically under-estimated (or overestimated), and here the forecasts compare on the whole more favorably with mechanical extrapolations.

Forecasters face problems of varying nature and difficulty in dealing with different types of economic change The prevalence of growth trends in the economy makes it relatively safe to predict the continuation of a business expansion through much of its progress, but a reasonably accurate forecast of *when* a current expansion is going to end is hard to make, even over a rather short span of time. The declines in the comprehensive series of income, output, and spending are much less frequently underestimated than the increases. Many are initially missed, and some turns are predicted that do not occur. Forecasts of troughs are in general better than those of peaks, a tendency which can be explained by the distribution of durations of expansions and contractions. On the whole, the ability of economists to predict the dates of turning points several months ahead must be rated as rather poor, even though they are apt so recognize these events more promptly than other observers of business and financial developments, and even though extrapolations tend to be considerably worse in this regard.

It is difficult to determine whether the accuracy of macroeconomic forecasts has improved or deteriorated over the years Some comparisons suggest an improvement, since the errors declined relative to the levels and changes in the predicted series (even though they increased in terms of dollars).[17] But the results vary for different forecast sets and criteria, so no clear-cut conclusions emerge.

To establish what the forecast errors are is necessary but far from sufficient for understanding what makes them what they are and thereby for learning how to make or choose better forecasts. Thus measurement and comparison of the errors ought to be followed by qualitative and quantitative analyses of sources and causes of errors. But this requires much more information on how the forecasts are produced than is available for many types of economic prediction. Detailed information of this sort is scarce and costly and difficult to assemble. Most of the forecasts are of the eclectic variety favored by business economists, they use in varying combinations models of economic relationships, anticipatory data, cyclical indicators, and judgments about the probable effects of recent and expected events. It is usually very hard, even for the forecaster himself, to decompose and reconstruct such predictions in numerical detail, and the cost may often be prohibitive.

Econometric forecasters use models that are much more specific, explicit, and formal. However, they must project the exogenous variables, and these predictions are essentially judgmental though often derived with the aid of various extrapolations. In addition, the *ex ante* forecasts in this class typically incorporate judgmental adjustments of the computer solutions of the models. Data on these inputs to, and alterations of, the models are frequently unavailable or incomplete. They need to be systematically recorded and analyzed in the interest of better understanding of the performance and potential of econometric model forecasts.

Where the information has been collected, the analysis indicates that the forecasts have been in many cases substantially improved by judgmental adjustments of the models.[18] Errors in the specifications of the models and in the forecasts of the exogenous variables often cancel each other or are offset in various degrees by the adjustments made.

The *ex ante* forecasts with econometric models are in most of the examined instances more accurate than the noneconometric forecasts, but the differences are small and the evidence is limited and inconclusive. It is clear that many of these forecasts are as much a product of their authors' judgments and other information (e.g., from anticipations surveys) as of the formal models used. These models, despite (some say because of) their relative complexity, do not confer upon the econometric forecasts a definite, consistent advantage over predictions that are derived by more informal approaches. But this is not inconsistent with the view, which I share, that the interaction of the econometricians with their models can be highly instructive and rewarding (in forecasting as in other services that the models can help provide).

The participants in the quarterly ASA-NBER surveys (see footnote 3, above) are regularly asked to rank the items in a short list of general forecasting techniques according to their own working preferences. More than half of them rank as first the "informal GNP model," in which the major expenditure components of the gross national product are predicted in various ways, combined into an overall forecast, and then checked and adjusted for plausibility and consistency. Most forecasters also report using leading indicators, anticipations surveys, and econometric models. But none of the listed approaches enjoys a general and regular preference of the survey participants, suggesting that none is expected to prove consistently superior to the others. Rather, the forecasters distinctly favor using the several techniques in varying combinations.

Conceivably, the effectiveness of forecasts could be such as to seriously complicate their evaluation. Forecasts may influence economic behavior and thereby either validate or invalidate themselves. For example, if almost everyone predicted better economic conditions in the period ahead, this very consensus of optimistic expectations could help stimulate the economy. Or, conversely, if the government accepted the forecast that the economy is threatened by recession, it would probably adopt policies designed to avert that undesirable outcome. However, it is easy to exaggerate or misjudge such feedback effects. In the United States, changes in GNP and other macrovariables that are predicted by different sources for a given period tend to show sufficiently large dispersion to suggest that no single forecast is generally accepted. *Consistently* superior forecasts evidently are hard to find. Some predictions are much more influential than others, and the special significance of those issued officially by the government is widely recognized. But the government forecasts, too, are at times much disputed and occasionally found less satisfactory than many private forecasts.

Moreover, the effects on the economy of macroeconomic—monetary and fiscal—policies also have been frequently overstated. An important reason for this is that these policies operate with substantial but complex and varying lags. When too much is claimed for the policies, the public at large is led to expect standards of both prediction and performance that economists presently do not know how to achieve.

So far as I can tell, very little is known about the accuracy and other measurable characteristics of long-term forecasts. *Ex post* assessments are few and fragmentary here—much more limited than for the short-term forecasts. Indeed, the verification of long projections is particularly difficult, and not only because of their far and wide horizons. If several conditional projections based on different assumptions are provided, which of them should be treated as preferred? The answer is apt to be uncertain or arbitrary, unless given *ex ante* by the forecaster himself (who often neglects to give it or is unwilling or unable to). Some long-range estimates are conceived as optimal targets for policymakers, others as judgments of what is likely to be achieved. Are the former "better" because they are more ambitious, or are the latter "better" because they are more realistic? What about the role in these projections of "noneconomic" developments such as wars, whose occurrence may be too frequent to be dismissed as "unforeseeable"? Should predictions of the economy's trends include some systematic allowances for such factors and, if so, how are they to be made and evaluated?

Many of those engaged in making such appraisals of long-run prospects avoid the very term "forecast." They say that the main test of the goodness of their "projections" (a widely favored term) is how well these assist in public and private decisionmaking. *Ex post* accuracy certainly need not be a satisfactory indicator of this ability of the projections to help users. How well the trends in target variables for economic and business planners and policymakers are assessed over periods long enough to absorb the lags with which the relevant actions are undertaken and put into affect may depend as much on the performance of those who decide and act as on the foresight and influence of those who project. But how are we to evaluate the usefulness of the projections on this basis? The task is presumably important, but it also seems difficult and, unfortunately, hardly tackled.

These reflections indicate that analyses of comparative accuracy of long-term projections should prove instructive and are much needed—but also that they are likely to be more difficult to execute and interpret and are less conclusive than similar tests for the shorter forecasts. Such appraisals, among others, can tell us something about what the projections ought to aim for and how. For example, optimistic assumptions of low rates of inflation appear to be the main cause of underestimation in some GNP and related projections; the corresponding estimates in constant dollars are not so biased.[19] A review of five-year and ten-year projections of constant-dollar GNP accounts and labor input data from a large source of detailed estimates of this sort suggests that they are often much more accurate for the overall aggregates than for the more volatile components. The projections for total GNP, personal consumption, total labor force, and employment are encouragingly close to the actual levels (within ± 6 percent). Those for residential construction, inventory change, unemployment, and armed forces series that vary a great deal in response to cyclical factors and changes in the international situation are generally much poorer and in some cases greatly (40-60 percent) off the mark.[20]

In addition to such projections, which are numerous, some long-range forecasts have been produced with the aid of aggregate econometric models and input-output models of supply connections among the Nation's industries. If the quantity of each input per unit of output is treated as a structural constant (which implies constant returns to scale and, a particularly drastic assumption, absence of substitution among inputs when relative prices change), then a system of linear equations can be set up describing the interdependence between the outputs of the different industries by means of these "technical coefficients" of production Such a model can be used to make conditional predictions of the values of industry outputs, given estimates or projections of the "bill of goods," that is, of purchases by the autonomous sectors consumers, government, and foreign countries Problems for applications of this approach to forecasting arise mainly from autonomous and price-induced changes in technology (which can invalidate the estimates of the technical input coefficients, especially over longer spans of time) and from variations in demand and, consequently, in capacity utilization (which may be troublesome for the shorter predictions with such models).[21]

Economic growth depends on scientific development, technological progress, social innovations, the resulting changes in conditions of life, the feedback effects of these changes on the demand for and supply of advances in science and technology, and so on. These are complex sequences and interactions even on the national level, and they are further complicated by international inequalities, rivalries, and cooperation. Hence it is not astonishing that the process continues to generate many unforeseen surprises, which have both positive and negative implications.[22] Economics, long ago labeled a "dismal science" because of its stress on the limits to growth, has lately been accused of promoting rapid growth while neglecting the grave ecological damages involved in the process. The charge seems to be applicable to some writings of some economists but also unduly generalized and simplified in at least its more extreme versions.

It is just as important to consider the reverse error which pervades some recent long-range economic projections by noneconomists. These appear to revive a simple Malthusian model and overemphasize the danger of imbalances caused by growth by underemphasizing the responses to them, through changes in capital accumulation, technological improvements, and demographic variables. As a result, both the need for and the potential of economic growth (which can avoid major excesses and distortions) tend to be underestimated. In the last analysis, these errors derive from the widespread tendency to assume that the demand and supply functions of all sorts are inelastic, in particular with respect to changes in the price structure.

I shall not attempt to forecast the future of economic forecasting, but the preceding does contain some hints at what the prospects, at least for macroeconomic prediction, might be. There are undoubtedly limits to what social and economic forecasting can achieve in our puzzlingly mixed stochastic-deterministic universe, where human behavior (individual and group) is variously constrained and yet in part voluntarily decided, so that generalizations about its causes and consequences are always tentative and of uncertain validity over time. In fact, these limits are probably more narrowly drawn than a great many people presume. But for several reasons I find it difficult to believe that present-day forecasts in economics, especially those with short and intermediate spans, already approach the theoretical upper bound of perfectibility.

Surely, macroeconometric models are in part misspecified and capable of being improved in this regard and perhaps also by better estimation techniques. It should be possible to design more informative surveys of business and consumer expectations and utilize the resulting data more effectively in the models and in combination with other forecasting techniques. Current work on business-cycle and growth-cycle indicators suggests similarly that further significant advances are possible on this front. More generally, richer, better, and prompter reporting on current economic conditions could contribute very significantly to the improvement of short-term forecasts, which in turn should help raise the accuracy of longer projections. As better extrapolative techniques become available, they too ought to provide useful tools and inputs in the prediction process. Last but not least, forecasters often can "learn from past errors" and, one may hope, will.[23]

In sum, there is much need and much room for improvements of data, models, and methods used by economic forecasters. There are grounds to expect that progress will occur in its own good time, but it need not be strong and steady. High promises and expectations should be avoided here, because they are not well founded and risk needless disappointments.

Suggestions for Research

I have outlined a number of problems encountered in the wide area of theoretical and empirical study and practical activity that was surveyed in this paper. A more detailed exposition is beyond my present purpose. In conclusion, however, it may be useful to identify several broad topics for further research. At least those concerning short-term macroprediction[24] have already received serious attention in past studies, but each of the items in the brief list that follows is believed to require a much more extensive and systematic effort:

1. Criteria and methods of forecast evaluation (what standards should be applied to the different types of forecasts, why, and how?).
2. Strengths and weaknesses of different forecasting techniques (their determination will require not only comparisons of forecasts from different sources but also decomposition of forecast errors, study of how forecasts which combine various approaches compare with "pure" predictions from well-defined benchmark models, etc.). The relation between econometric and judgmental forecasting is a part or aspect of this topic, which is of special interest to many practitioners and researchers.
3. The accuracy and usefulness of forecasts as a function of the predictive span (considerable work has been done on this problem for short-term forecasts, and further analysis is particularly needed for longer projections as part of item 4 below).
4. Assessment of intermediate- and long-term economic projections—a comprehensive analysis of their accuracy and other properties.
5. The role of exogenous and endogenous factors in economic forecasting (as related to the important and much debated question of the role of exogenous and endogenous factors in economic growth and fluctuations).[25]
6. Interaction of forecasts, realizations, and policy changes—feedback effects (this has many links to topic 5; both are very complex problems that are likely to require intensive and sophisticated research techniques—theoretical analysis, simulations, new data collections, and international comparisons).
7. Strengths and weaknesses in forecasting different economic variables and processes (e.g., prices, unemployment, and inventory change are important variables that are predicted poorly compared with GNP; should they get a larger share of resources to be invested in forecasting studies?; how can their forecasts be improved?).
8. Systematic collection of a representative sample of reputable *ex ante* predictions from the main econometric model builders, professional business forecasters, etc. This involves periodic, properly designed and evaluated surveys which would prevent the gaps in the record that are frequent in data for past forecasts and that impede research.
9. Extension and improvement of information needed for more effective analysis and forecasting of economic conditions. Several interrelated data systems should be considered here, including cyclical indicators, anticipations surveys, national income aggregates, monetary, financial, price statistics, etc.
10. International comparisons of experience with different types of economic forecasting. This would extend substantially the range of the available materials and evidence and should be particularly helpful in dealing with some of the other problems (e.g., items 6 and 7 above).

This is clearly a very ambitious, though by no means all-inclusive, agenda. It involves problems that lend themselves to, and warrant, comprehensive research. The choice between the topics is also made difficult by their being interrelated (though, of course, not inseparable). In past studies, topics 1, 3, and 8 probably account for the largest share of the work done and topics 4, 6, and 10 for the smallest. Feasible research strategy may well require a more selective approach. If pressed to focus on matters of the greatest direct interest and probable return to producers and users of forecasts, I would opt for items 2, 4, 7, 8, and 9 (not necessarily in this order). If free to pursue matters of more theoretical interest and long-range returns, I would opt for 1, 5, 6, and 10.

FOOTNOTES

[1] There is no space here to provide a historical perspective on the evolution of economic forecasting. For this background, with references to literature, see Victor Zarnowitz, "Prediction and Forecasting, Economic," *International Encyclopedia of the Social Sciences*, Vol. 12, (1968), pp. 425-39.

[2] In particular, there are no totally endogenous econometric models some variables affecting economic activities are determined in large part by noneconomic or other factors, and their values must be estimated or assumed outside the model. (These are the so-called exogenous variables; see the discussion of the econometric approach below.)

[3] *The ASA-NBER Quarterly Survey of Economic Outlook* (conducted by the Business and Economic Statistics Section of the American Statistical Association and evaluated by the National Bureau of Economic Research). See below.

[4] For further discussion, see the reference in footnote 1 and V. Zarnowitz, "Forecasting Economic Conditions The Record and the Prospect," in V. Zarnowitz (ed.), *The Business Cycle Today* (New York National Bureau of Economic Research, New York, 1972), pp. 183-239.

[5] The role of random impulses in propagating business cycles has been given much attention in recent simulation studies of aggregative econometric models. See Bert G. Hickman (ed.), *Econometric Models of Cyclical Behavior* (New York National Bureau of Economic Research, New York, 1972).

[6] G E P Box and G M. Jenkins, *Time Series Analysis, Forecasting and Control*. (San Francisco. Holden-Day, 1970).

[7] As shown by the U S. Department of Commerce sample surveys.

[8] The main examples are prices of common stocks, which according to many recent studies are traded in efficient markets and follow random walks For a summary of these studies, see James H. Lorie and Mary T Hamilton, *The Stock Market. Theories and Evidence* (Homewood, Ill.: R. D. Irwin, 1973).

[9] The principal investigators were Wesley C. Mitchell and Arthur F Burns in 1938, Geoffrey H Moore in 1950 and 1960, and Moore and Julius Shiskin in 1966 The revisions and extensions of the list of indicators have been prompted by the appearance of new data and improvement of old data, the accumulation of knowledge about the series and the processes they represent, and the increases in efficiency of data processing. The reviews resulted in many significant changes, but the core of the list remained rather stable in terms of economic processes See G H Moore and J. Shiskin, *Indicators of Business Expansions and Contractions* (New York: National Bureau of Economic Research, 1967).

[10] Perhaps the most representative exposition of this idea is Milton Friedman and Anna J. Schwartz, "Money and Business Cycles," *Review of Economics and Statistics*, Supplement (February 1963), pp. 32-64.

[11] These developments are variously attributed to changes in (1) the structure and institutions of the economy, (2) economic knowledge and its policy applications, and (3) public attitudes and expectations. Some of these changes, however, seem to have at the same time strengthened the forces of inflation, which makes them partly destabilizing.

[12] See Ilse Mintz, "Dating American Growth Cycles," in *The Business Cycle Today*, as cited, pp. 75-82.

[13] A closely related distinction is between alternative hypothetical forecasts (e.g., of GNP next year, assuming 6 or 10 percent increases in government expenditures) and a single preferred forecast (that government expenditures will be up 8 percent and GNP will be such and such).

[14] In any particular period these terms of course may differ from zero, even though their expected values are zero.

[15] For references to literature and current work on the assessment of predictions in economics, see the reports cited in footnotes 1 and 4 above and also V. Zarnowitz, "New Plans and Results of Research in Economic Forecasting," *51st Annual Report*, National Bureau of Economic Research (September 1971), pp. 53-70.

[16] The following is a brief summary of selected findings, stated in general, largely qualitative terms, of reports published by the National Bureau of Economic Research since 1967. The principal authors are G. H. Moore, Jacob Minder, V. Zarnowitz, Rendigs Fels, Rosanne Cole, Michael K. Evans, Yoel Haitovsky, and others. In Europe, Henri Theil and his associates have produced, since 1958, a large body of analysis directed to both business surveys and econometric model forecasts.

[17] For example, one large collection of annual GNP forecasts shows mean absolute errors of $7.5 billion in 1957-1963 and $9.4 billion in 1964-1970, these represent 32.1 and 17.8 percent of the mean absolute annual changes in GNP during the two respective seven-year periods.

[18] Technically, these are modifications of the constant terms of the model equations. See M. K. Evans, Y. Haitovsky, and G. I. Treyz, assisted by V. Su, "An Analysis of the Forecasting Properties of U.S. Econometric Models," in *Econometric Models of Cyclical Behavior*, *op cit.*, Vol. 2, pp. 949-1158.

[19] Gerhard Colm and Peter Wagner, *Federal Budget Projections* (Washington, D.C. The Brookings Institution, 1966). The authors present annual estimates (as of February 1965) of GNP in current and in constant prices for fiscal 1965-1973 and related projections of government revenues and expenditures for 1968 and 1973. Already in 1967 there were indications that the rises in GNP and Federal spending were going to be strongly underestimated in these projections, see the review of the Colm-Wagner book by V. Zarnowitz in *Journal of the American Statistical Association*, (December 1967), pp. 1500-1502. As then noted, these errors were related to the unanticipated scope of the Vietnam War, but this did not make them insignificant or otherwise avoidable. "For it seems that 'normal' conditions are never really assured and unforeseen events seldom fail to occur. Conceivably, over a period of 5-10 years the net effects of limited wars and other such exogenous events could prove transitory and weak, but historical evidence provides no sound basis for predicting this as the most probable outcome" (*ibid.*, p. 1502).

[20] Center for Economic Projections, National Planning Association, *An Evaluation of Prior NPA Projections*, National/Regional Economic Projections Series, Report No. 67-J-2 by Morris Cobern (April 1967). As implied by the above results and noted in the NPA report, "To some extent offsetting biases in the components result in projections for the aggregates being close to the actual."

[21] Input-output analysis was developed by Wassily Leontief and first presented in 1941. For an application to long-range forecasting, see Clopper Almon, Jr., *The American Economy to 1975: An Interindustry Forecast* (New York: Harper & Row, 1966).

[22] Simon Kuznets, *Quantitative Economic Research: Trend and Problems* (New York: National Bureau of Economic Research, 1972); see particularly, pp. 66-70.

[23] Clearly this requires that forecasters develop good record-keeping habits, so that they can accumulate knowledge of their own past practices, successes, and failures.

[24] My own work having dealt mainly with this subject, I am much better acquainted with it than with the other subdivisions of economic forecasting.

[25] A closely associated set of questions concerns the relations between forecasting in economics and in other pertinent fields: technology, sociology, psychology, and politics.

Chapter 9

A Resource Allocation Tool for Decisionmaking

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and
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The Problem of Resource Allocation

The idea of applying a precise rating system to research and development was, until fairly recently, highly suspect—somehow anti-intellectual. The old stereotype of the independent researcher following his private star dies hard. But the history of the past 30 years has sounded the death knell of this romantic approach. World War II, *Sputnik*, and the Apollo moon program—not to mention the new worlds of electrons, medicine, and transportation—have all made science and technology into a big business of tremendous complexity. There must be a logical, rational way of selecting the tasks to be worked on and the resources to be expended on the effort.

As the scope of the job of allocating technical resources becomes larger and the complexity increases, more and more factors must be considered to arrive at an effective decision. The point is soon reached where even one small decision may affect the operation of all efforts—at least to some degree. When that happens, the human brain alone cannot do the job as well as is needed.

The situation is especially difficult when many research and development projects must be considered for inclusion in a fixed government or corporation resource ceiling. Priorities must be set or decisions made concerning which projects to back and which to drop or delay. Numerous efforts are interrelated with regard to time, resources required, purpose, and possible technical transfer. Choices therefore must be made with regard to the total effect. Whether a manufacturer, a service industry, a government agency, or a university laboratory, every organization must seek the greatest payoff from its resource investment.

What alternate methods are available for helping to make allocations? How do you evaluate them? The basic point is that the resource-allocation problem is usually too big to keep in one man's head. Data inputs come from areas completely outside of his control. The inputs involved multiply rapidly to the hundreds or thousands when an allocation problem is really subjected to careful analysis.

It might be useful, at this point, to back off and take a look at how these problems were usually handled in the past. A familiar approach was to take care of the "squeaking wheel." Some requested resources were cut from all estimates. Then the manager could sit back and see who complained the most. On the basis of the loudest and most insistent cries of anguish, he would then restore some of the resources withheld. When he reached his budget ceiling, he simply shrugged.

Another common approach, which resulted in fewer squeaks, was to allocate this year's resources in just about the same manner as last year's. But if the level of funding of the status quo approach is continued very long in a rapidly changing technology, the organization involved will end up in serious trouble.

One easy approach, which often made research and development managers feel most secure, was to be guided by the glorious past. For example, last year or the year before or perhaps several years ago a division or organization had a very successful project; therefore, why not fund the unit for the next 5 years on any projects advocated by it? This method eliminated the need for analysis of the proposed projects. If an individual or laboratory had a past record of success, it got whatever it asked for.

All readers are familiar with the "white charger" technique. Here the various department heads came dashing in to top management with multicolor graphs, handouts, and well-rehearsed presentations. If they impressed the decisionmaker, they were rewarded with increased resources. Often the best speaker or the last man to brief the boss won the treasure hunt.

Finally, there was the committee approach, which freed any individual manager from resource allocation decisions. The committees called the shots—increasing, decreasing, or leaving all allocations as they were. Too often the committee did not have enough actual experience in the organization or sufficient information upon which to base its recommendations. Its members, especially if it was an ad hoc group or from outside the organization, could avoid responsibility nicely since they did not have to implement their recommendations. This is not to say that committees are never useful.

Obviously, these allocation methods are neither scientific nor objective. Anyone who has lived with them knows that they must be supplanted. An increase in the number of reports and recommendations that can be demanded of every level in the research and development organization does not help the situation. More data without a system for using them only compounds the manager's problem.

The Basics of Resource Allocation

A more realistic alternative is being built by specialists in operations research. Information assembled by them can be used to assist managerial judgment significantly. This is the point where quantitative evaluation techniques enter the picture. Each major aspect of a program can be examined, first separately and then as it becomes interrelated to competing programs. Items such as timeliness, cost utility or payoff, confidence level or risk, personnel, and facilities can be evaluated by specialists in each field and the total picture made available as a basis for decision. Payoff areas can be identified and problems highlighted. Inputs can be accurately recorded, made clearly visible, and analyzed to help make the final decision.

The use of quantitative methods leads the decisionmaking process to formal analysis, much as a research project itself employs formal analytical techniques. Subjective assumptions and values of decisionmakers become apparent and can then be analyzed in an objective manner. Formalization of the decisionmaking process allows all considerations and complications of a problem to be identified and examined concretely.

Often an expert in a field is asked by management to estimate the usefulness of a project or its probability of success or completion in regard to various time and cost criteria. Men with wide experience in a field usually have no difficulty in using this kind of probability once they realize that what may seem like guesswork can lead to an objective analysis. Other criteria, such as the utility of the research to the objectives of the organization or its relevance to desired priority systems, can be rated in a like manner.

The use of subjective probabilities makes feasible the incorporation into the decision process, in a formal and visible way, of both the nonobjective and objective criteria and variables that were previously taken into account by the decisionmaker when he applied his judgment to the data available to him.

If several experts in a field were asked the same questions, their responses would very likely differ somewhat. Each has different assumptions about the conditions under which the research would be undertaken, who would be performing the research, and how the results of the research would be used in the future. However, if several experts were consulted on several specific criteria for evaluation, their collective responses would reflect the best-informed opinions on the matter, regardless of variations between individual responses.

Some people ask how a collection of admittedly subjective ideas can be turned into a reliable "objective" tool. They feel that people are fooled into thinking that they have "data" when they assign a number to an opinion or guess. Perhaps the best first answer should be that this approach is not really new but has been used successfully in other fields. High school students are admitted to universities on the basis of quantitative judgments made by teachers. These teachers grade about five subjects a year for four years of high school. The different teachers, different subjects, different tests, and different subject matter taken in high schools throughout the nation are all expressed in numbers or letter grades. Teacher opinions on how to grade, biases and prejudgements, oral recitations, and grades on nonstandardized, unstructured subject matter and tests are all injected into the conglomeration to form the individual teacher's final grade in one subject. High school grades for the 4 years are then averaged to produce one number: the high school average. Miraculously, there is a good, positive correlation between this magic average and success in college. It is recognized in most cases that this "quantitative estimate" of many judgments is the best indicator of success in college.

Since opinions and judgments are gathered and weighed by every decisionmaker in one way or another, why not formalize this process? Some method must be devised for evaluating opinions and judgments so that priorities can be most reasonably established. Somehow comparisons must be drawn between projects which may differ in cost, focus of research, and duration.

A Resource Allocation Model

Forecasting International (FI) developed a model which enables such comparisons to be made for the Processes and Effects Division of the Environmental Protection Agency. The Processes and Effects Division has a particular need for systematic allocation procedures because its situation is extremely complex. The scope of the work is very broad, involving several disciplines in the physical and biological sciences and frequently the social sciences as well. In addition, research is done in agency facilities and by outside contractors, both groups of performers being widely dispersed. Such organizational and geographical separations impose barriers to communication which add to the difficulties of resource allocation. In addition, most candidate projects, including many which must be deferred, concern urgent needs.

These factors serve to illustrate the need for a model, as well as to imply some of the properties the model must have. These properties are delineated below:

1. As a minimum the model should yield a meaningful and realistic ranking of candidate research projects. It should also produce a figure of merit for projects which takes costs into account, since the objective is to allocate resources efficiently.
2. The model must be powerful enough to handle the full range of the division's technical and organizational complexities. Projects dealing with, for example, pollution of estuaries in one laboratory must be compared with work at another site on thermal pollution of the Great Lakes.
3. The results of evaluations and the rationale behind them must be readily communicable upward and downward in the administrative chain. They should fit the frames of reference of people having broad range responsibility as well as those having project responsibility.
4. The workings of the model should be visible and comprehensible and should not require a specialized staff for operation. Managers tend to distrust—and rightfully so—hidden procedures in such a critical area as resource allocation. As a corollary, inasmuch as the model is supposed to aid management, it should take into account those factors used to control the programs.

The resource allocation model which FI developed was designed to meet the above-mentioned criteria. The methodology provides a means of relating the expected value of a research project to the funds required by the project. The model rank orders each project according to its desirability index, the ratio of the expected value of the research to the estimated cost of the same research. The index is basically a figure of merit which measures the expected efficiency of resource utilization: the higher the numerical value of the index, the greater the expected return on resources used and, consequently, the more desirable the project.

The model is not used to compute costs; it is assumed that a budget will be developed during the planning process. The crux of the model is the estimation of expected value: the model is fundamentally an algorithm or set of rules for computing expected value.

In developing the model we first identified major categories of considerations which are important for all research projects:

1. Value of the research to the funding organization.
2. Probability of successfully accomplishing the goals of the research project.
3. Project management.

The next step was the identification of subcategories in which to measure distinct aspects of a project's contribution to the three major categories. The subcategories were as follows:

I. Research Value
   A. Mission Contribution: a measure of the extent to which a project's potential technical accomplishments would contribute to the fulfillment of the Processes and Effects Division's mission. The project's responsibilities are stated in its authorization and are subcategories of Mission Contribution:
      1. Standards—a quantitative measure of the degree to which the project contributes to information for establishing pollution standards.
      2. Control Action—a measure of the degree to which the research will contribute to the control of pollution.
      3. Identity and Source of Pollutants—a measure of how much the research contributes to information about generation of pollutants.
      4. Path Persistence and Fate—a measure of how much the research contributes to knowledge about how pollutants are transported to and within the environment.

5. Human Effects—a measure of the research contribution to knowledge about how pollutants and their derivatives affect human health and quality of life.
6. Ecological Effects—a measure of the research contribution to knowledge about how pollutants and their derivatives affect the ecology.
7. Economic Effects—a measure of the research contribution to knowledge about the economic costs of pollutants and their derivatives.
8. Measurement—a measure of the research contribution to pollution measurement.

B. Problem Significance: a measure of the importance of the problem which the research attacks, as determined by the extent and intensity of the environmental stress. Three distinct subsets of criteria are used here depending on whether the proposed research relates to air, water, or radiation pollution.

C. Timeliness: a measure of the availability of research results in relation to the need for the results.

D. Field Utility: a measure of the practical feasibility of applying the research results to enforcement of environmental standards.

E. Technological Transfer: a measure of the additional value, if any, resulting from applying the research results in other than the primary area for which the work is undertaken.

II. Probability of Success

A. Technical Risk: a measure of the likely success of the technical objective of the project, usually a function of present "stage of development" of the task being evaluated.

B. Past Performance: a measure of the performing organization based on its previous track record.

C. Technical Planning: a measure of how well the technical program has been thought out, especially the extent to which alternative routes have been considered and evaluated.

D. Adequacy of Funding: a measure of whether, in the assessor's judgment, the objective can be achieved with the requested funds.

III. Project Management

A. Research Objective Achievement Plan (ROAP) Purity: a measure of the extent to which all the proposed project tasks are vital for accomplishing the stated objective.

B. General Considerations: a measure of whether the project can or should be delayed in whole or in part.

C. Management Environment: a measure of whether the project is acceptable to higher authority in the management structure.

Each of these categories could have been divided into subcategories in a fashion similar to the way mission contribution has been subdivided. We judged further divisions to be unnecessary for the purposes of this model. Such divisions could be made and additional categories could be created if circumstances changed.

Once the categories that are to be measured have been identified, the criteria for assigning numerical values to the categories can be defined. We decided that, with a few exceptions, each of the categories within the three major divisions (value, success, and management) could receive a rating of A, B, C, or D for a specific project. A was the highest rating which a project could receive in a category, and D was the lowest. Each letter rating carries an associated numerical value: A = 8 points; B = 4 points; C = 2 points; and D = 1 point.

The rationale for this scale is derived from observation about human thought processes. People think in terms of doubling or halving. For example, if someone judges light A to be appreciably brighter than light B, it is about twice as bright. This carries over to other areas where judgments are used. We reemphasize that the model is a device for quantifying and communicating subjective judgments. These values represent the judgments of Forecasting International personnel.

The criteria for the letter ratings were then specified. A program receives a letter rating in each evaluation category. The evaluation criteria are listed below.

I. Research Value

A. *Mission Contribution:* A program is rated on its contribution to each of the division's responsibilities.

1. A major increase (breakthrough) in useful knowledge of the causes, consequences, measurement, or control of pollution.
2. An important extension in knowledge.
3. A contribution which fills gaps in our present knowledge or increases the efficiency of measurement or control.
4. Analysis or evaluation of available knowledge.

B. *Problem Significance:* Separate criteria were developed for water, air, and radiation pollution programs. Only the water pollution criteria are detailed here for illustrative purposes.

The problem significance rating for a water-related project is basically a measure of the number of waterways to which the knowledge will be applicable. The basis for judgment is present in Table 1, which shows the stresses which will be applied to 22 major water basins. It was developed from information concerning the 1980 medium demands for water as estimated in *The Outlook for Water.*[1] The letters indicate the importance of demands which will be made on available water supply, and from them one may infer the kind of water pollution problem likely to be encountered. The letters A to D indicate the severity of demand. A, under city, shows that the demand for a municipal water supply relative to available stream flow in the particular waterway is high—roughly the upper quartile of such demands.

A significance rating is derived simply by identifying the water basins affected by the project, counting up the number of A's, B's, etc. and multiplying by 8 for A, 4 for B, 2 for C, and 1 for D. For example, a need dealing with pollution from sewage plants in estuaries (related to city demand) will be of concern in New England (A), the Delaware and Hudson (A), the Chesapeake (C), the lower Mississippi (D), the Western Gulf (B), the South Pacific (A), the Central Pacific (D), and the Pacific Northwest (D). The rating will be

A. $8 \times 3 = 24$
B. $4 \times 1 = 4$
C. $2 \times 1 = 2$
D. $1 \times 3 = 3$

Table 1. Water Demands

	Agriculture	Mining	Cooling	Manufacturing	City
Northeast					
New England	D	D	A	A	A
Delaware and Hudson	C	C	B	B	A
Chesapeake Bay	D	D	B	B	C
Ohio	D	C	A	B	C
Eastern Great Lakes	D	B	A	A	A
Western Great Lakes	C	B	A	A	A
Upper Mississippi	C	B	A	C	C
Lower Mississippi	C	D	B	C	D
Southeast					
Southeast	C	D	C	C	D
Cumberland	D	D	D	B	D
Tennessee	D	D	B	C	D
Lower Mississippi	C	C	C	B	D
Lower Arkansas-White-Red	C	D	C	D	D
Mid-Continent					
Upper Missouri	B	C	B	D	C
Upper Arkansas-White-Red	A	B	D	B	B
Western Gulf	B	B	C	B	B
Southwest					
Upper Rio Grande-Pecos	A	A	D	C	A
Colorado	A	B	D	D	B
Great Basin	A	B	D	C	C
South Pacific	A	A	C	A	A
North Pacific					
Central Pacific	B	C	D	D	D
Pacific Northwest	B	D	D	C	D

These ratings are then summed and transformed into a final significance rating according to the following criteria:

A = at least 101 points
B = 51–100 points
C = 26–50 points
D = 1–25 points

Some problems cannot be simply related to single environmental stresses. An example is the development of techniques for measuring and describing the condition of ecological balance. This is likely to be of significance in all waterways and for all environmental stresses. It is given an arbitrary significance rating of A.

C. *Timeliness:*
1. A tight but realistic schedule which is consistent with the urgency of the problem and other work, if any, upon which a solution to the total problem depends.
2. The scheduled completion of effort, although not optimum, is probably the best that can be achieved—or timeliness cannot be assessed.
3. The objective will be achieved far ahead of other work essential to solving the problem as envisioned by the appraiser, and a stretch-out would occasion no significant hardship.
4. The work is directed toward the solution of an existing problem, which is reasonably expected to disappear before exploratory development can be completed.

D. *Field Utility:*
1. The exploitation of accomplishment will clearly be operationally feasible and is expected to result in a major improvement in the EPA's capacity to enforce environmental standards.
2. The exploitation of accomplishment will be operationally feasible and will contribute materially to the EPA's capacity to enforce environmental standards.
3. Uncertainty exists whether the expected accomplishment will be operationally feasible when reduced to practice.
4. Exploitation of the expected accomplishment will not be operationally feasible.

E. *Technological Transfer:*
1. This project will provide significant technology transfer to another program area.
2. This project will provide useful technology transfer to another program area.
3. This project is limited in scope; technological transfer very slight or the technology is unique to this program; no technology transfer foreseen, but possible.
4. This project depends very heavily on cross-support from other areas. In other words, it is really "parasitic" in nature.

II. Success Factors
A. *Technical Risk:*
1. Essential scientific methods or component technology in hand, primarily assemblage, application, or development.
2. Capability could be obtained at the level of funding with a confidence level of at least 80 percent. (The confidence level asked for from researchers by management.)
3. Fundamental scientific knowledge is well in hand but has not yet been applied in practice.
4. Forecasts show one could not have the capability in the time required with level of funding with a confidence level of about 20 percent.

B. *Past Performance:*
1. Past performance of the performing department is excellent. (The previous work has been completed entirely without additional funds.)
2. Past performance of the performing department is good. (The previous work has been completed with little additional funds.)
3. Past performance of performing department is fair. (The previous work has been completed with no additional funds, but late.)
4. Past performance of performing department is poor. (Previous work has been completed late and with substantial additional funds.)

C. *Technical Planning:*
1. Logical approach to this technical problem; alternative approaches to the problem examined.
2. Logical approach to the technical problem, one alternative approach to the problem has been examined.
3. Logical approach to the technical problem, but no evidence that alternative approaches

to the problem have been examined.

4. Illogical approach to the technical problem.

D. *Adequacy of Funding Estimates:*

1. Estimates in the funding profile are not only reasonable but are clearly the result of considerable realistic planning.
2. Estimates in the optimum funding profile appear reasonable for the scope of work envisioned.
3. Estimates in the optimum funding profile are clearly excessive for the scope of work envisioned.
4. Estimates projected appear so inadequate that serious consideration should be given to cancellation.

III. Management Factors

A. *ROAP Purity:*

1. All the approaches are reasonable, and at least 90 percent of the approaches are germane.
2. All the approaches are reasonable, and between 75 percent and 90 percent of the approaches are germane.
3. Less than 75 percent but more than 25 percent of the approaches are reasonable and germane.
4. Less than 25 percent of the approaches are reasonable and germane.

B. *General Considerations:*

A (1 point) or D (0 points).

— Organization (is the proposing organization the right organization to do job?),
— Must the program be started this year?
— Are there efforts which may be dropped by a specific laboratory to accommodate manpower requirements ot this program?
— Can this proposed program be logically included in an existing ROAP or combined with other ROAP? Identify.
— Should this proposed program be continued next year? (For existing programs.)

C. *Environment:*

1. On-going program.
   a. Project is required by legislative mandate or is seen as desirable by higher headquarters.
   b. Project is regarded as unfavorable by higher headquarters.
2. Proposed program.
   a. Project has been requested by higher headquarters.
   b. Project was previously submitted and rejected by higher headquarters. No significantly new information was attached to warrant further consideration.

A project's expected value, which is divided by the project's estimated cost to yield a desirability index, is then simply the product of its ratings in all categories. The higher the ratings, the higher the expected value.

Some of the possible additional complexities which can be incorporated into a decisionmaking model are illustrated by the way that the value of the mission contribution category is derived. This category contains some exceptions to the A to D rating method described above. There are several steps in the calculation of the numerical value assigned to a project's mission contribution rating. The first step is performed once, and its results are applied to all projects. This step is the determination of the relative weight, or importance, associated with each of the division's stated responsibilities. The relative weights to be assigned to each of the mission components are estimated by the use of a cross-support matrix which relates each component to all others. The matrix is shown in Table 2.

The rows and all but the last two columns of the matrix list the mission elements. The cells are filled across the rows by letters designating the extent to which the element in the row contributes to the element in the column. Consider the first row. One fills it in by asking, "If I know what the standards are, how much do I know about appropriate control actions, about the identity and source of pollutants, about the path persistence, about the fate of pollutants, etc." A letter is entered into each cell indicating the analyst's judgment of the extent of support: H for high, M for moderate, L for low, and Z for negligible. Numbers are then assigned to the letters (H = 8, [illegible]=4, L = 2, and Z = 1) and summed across the rows. Raw sums are in the first number column and normalized to a sum of 100 in the second.

The analyst then rates a program on its contribution to each of the division's responsibilities. Again, the project is given a rating from A to D. The criteria for judgment are as follows:

A. A major increase (breakthrough) in useful knowledge of the causes, consequences, measurement, or control of pollution (8 points).
B. An important extension in knowledge (4 points).
C. A contribution which fills gaps in our present knowledge or increases the efficiency of measurement or control (2 points).

D. Analysis or evaluation of available knowledge (1 point).

The extent of contribution rating is *multiplied* by the weight previously calculated for the category. The total mission contribution rating is the sum of the mission contributions calculated for all mission categories.

Although he must rate a project in several categories and in accordance with several criteria in each category, the analyst can use a fairly simple form to facilitate the evaluation. The rating form contains four charts which correspond to the steps in computation: "Mission Contribution Rating," "Expected Value Estimation," "Funding Schedule," and "Desirability Index." An example of the charts is presented in Figure 1.

Each row on the mission contribution rating chart contains information about a project's relation to a division responsibility. The first number in the row shows the weight of the division responsibility relative to its other responsibilities, as calculated from the cross-support matrix in Table 2. The analyst checks the appropriate A, B, C, or D column and multiplies the numerical value associated with the letter (8, 4, 2, or 1) by the responsibility's relative weight. Thus, if the analyst checks Column B for a project's contribution to standards, the contribution would be 32 points (8 for relative weight of standards times 4 points for B rating). The contributions are summed for total mission contribution.

Figure 1 also shows expected value estimation. Here the analyst checks A, B, C, or D for each rating category. Because total mission contribution is calculated in the previous step, the numerical value for that step is merely entered at the bottom of the mission contribution column. The numerical value associated with the letter checked in each column is entered at the bottom

Table 2. Mission Cross-Support Matrix

MISSION ELEMENTS	STANDARDS	CONTROL ACTIONS	IDENTITY/SOURCE	PATHS/PERS. FATE	HUMAN EFFECTS	ECOLOGICAL EFFECT	ECONOMIC EFFECTS	MEASUREMENT	PROGRAM	SUM	% of Total
STANDARDS		M	M	Z	Z	Z	Z	M	H	24	8
CONTROL ACTIONS	L		Z	Z	Z	Z	Z	Z	L	10	4
IDENTITY/SOURCE	M	H		M	M	M	M	H	H	44	16
PATH/PERSISTENCE FATE	H	H	M		H	H	H	H	M	60	21
HUMAN EFFECTS	H	L	M	L		L	L	Z	H	29	10
ECOLOGICAL EFFECT	H	L	M	M	M		M	M	H	38	13
ECONOMIC EFFECTS	M	L	L	L	L	L		L	M	20	7
MEASUREMENT	H	H	H	H	M	M	M		H	52	18
PROGRAM	Z	Z	Z	Z	Z	Z	Z	Z		8	3
										285	100

H= 8 - High Support L= 2 - Low Support
M= 4 - Moderate Support Z= 1 - Negligible Support

FROS/ROAP Rating Form			Desirability Index	
			Rating EROS/ROAP	
Innovative Developments in Air Quality Modeling EROS/ROAP Title			FY 74	428
			Average	022
I.D. No. *21 AUS*	Prg. Elem. *No. 1A1009*	Prg. Area Title *Transport Processes*	Total	183
			No. Years	6

Mission Contribution Rating

Element	Wt.	A.	B.	C.	D	Cont.	Comments
Standards	8						
Control Action	4						
Identity/Source	16						
Path/Persistence/Fate	21		✓			84	
Human Effects	18						
Ecological Effects	13						
Economic Effects	7					72	
Measurement	18		✓			12	
Program Development	3		✓			108	
Total Mission Contribution							

Expected Value Estimation

	Value Factors					Success Factors				Mgmt Factors			Comments
	Mission Contribution	Significance	Timeliness	Field Utility	Tech Transfer	Tech Risk	Past Performance	Tech Planning	Funding	Parity	General	Environment	
A		✓											
B			✓	✓	✓								
C													
D						✓							
R	100	8	4	4	4	2							Expected Value Product: 172,000

Funding Schedule $K	FY 74	FY 75	FY 76	FY 77	FY 78	FY 79	Total
	410	400	300	200	200	150	1,000

Figure 1. Sample evaluation forms.

of the column. The values in the bottom row of the chart are then multiplied to produce the expected value.

Two additional exceptions to the general A to D rating scale are shown on the expected value chart. Under management factors, the columns for general considerations and management environment do not provide for B or C ratings. These two categories contain go/no go criteria: A has a value of 1 point, and D has a value of 0 points. The criteria that must be met for an A rating can be viewed as being absolutely necessary in order for the project to be considered at all for funding, but an A rating has no effect on expected value. Since a D rating has a value of 0, such a rating in one of these two categories automatically reduces the expected value of the project to 0.

The final computation is the calculation of the project's desirability index, the ratio of the project's expected value to its estimated funding. A project's funding schedule is taken from the project statement.

After desirability indexes have been derived for all projects, the projects can be ranked in order of their indexes. If the results are surprising to the analyst, he can examine each step of the process and determine exactly why the results occurred. Unexpected results may be attributable to a combination of factors, none of which alone can account for the results. The model is flexible enough so that different weights or scales can be easily tested, particularly when the model is programed on a computer. Several analysts can evaluate the same projects and the model can compare the results. Each step of the evaluation can be examined separately. More distinctions within categories can be introduced. Variables can be omitted. In short, the model can and should be tested and refined with use. Refinement does not equal more complexity. The more complex the rating scales and criteria, the more difficult to use the model. At some point of complexity and sophistication, a model ceases to be appreciably simpler to a decisionmaker than the reality it represents, and it becomes no longer useful or usable.

Conclusion

We are well aware of many of the omissions and weaknesses of quantitative selection or resource-allocation techniques based on the type of analysis illustrated here. It should be stressed again that the parameters generated do not yield decisions but rather information that can facilitate decisionmaking. Indeed, these techniques are merely thinking structures to force methodical, meticulous consideration of all of the factors involved in resource allocation. In other words, data plus analysis yield information; information plus judgment yield decisions.

Those of us, who are working to build these allocation systems are firmly convinced that if we had to choose between any system with miles of computer printout and the human brain, we would select the brain. The brain has a marvelous way of learning from experience and an uncanny way of pulling out salient factors and rejecting useless information. It is wrong to say, however, that one must select intuitive experience over analysis or minds over machines. They are not alternatives but should complement each other. Used together properly they can be counted on to reduce the most complex problem to an answerable question.

The fact that a computer or an adding machine may be used to facilitate data-handling should in no way detract from the basic fact that human subjective inputs are the foundation of these systems. Accurate human calculation, as opposed to use of a computer for the calculations of all the interrelationships considered, would not alter the basic principles of these management tools in any respect.

These approaches represent the latest thinking on how to use the collective judgments of technical staffs and decisionmakers in such a manner that the most logical and sound decisions evolve. The goal is the greatest payoff achieved for the resources committed—whether in men, money, or facilities. To make an incorrect decision is understandable, but to make a decision without really trying to get as much usable information as possible is unforgivable. The managers who design and work with information systems, however, must realize that the technological forecasts, quantitative estimates of project value, and other aids to resource allocation are simply planning tools.

Even this caveat, however, does not defuse critics of the whole idea —and there are some very vocal ones both in government and business. Some of the criticism is a reaction to the fear of "mechanization" of a task felt to be rightfully in the province of human evaluation. Other critics claim that building up a logical system, computerizing the output, and quantifying what are essentially intuitive and judgment decisions may insulate some managers with a false sense of security. In some way they fear management responsibility will be hard to pin down.

Systematic analysis, however, tends to force managers to consider their resource-allocation tasks more comprehensively and highlights problem areas that might easily be overlooked by more traditional approaches. At the same time, quantitative methods are considered threatening by some managers because they may tend to expose their value judgments to critical analysis by others. As the degree of sophistication of these planning devices improves, however, managers will be won over, especially when they will be able to spend more, not less, time doing real decision-making.

FOOTNOTES

NOTE: This article is, for the most part, a combination of two previously published reports. The theoretical discussion of resource allocaton draws extensively from Chapter I, "Technical Resource Allocation: An Overview" in *Technical Resource Management Quantitative Methods* by Marvin J. Cetron, et al. (Cambridge, Mass. The MIT Press, 1969). The application of the methodology for EPA's Processes and Effects Division is derived from the final report "Development of Resource Allocation Tool for Research" by Forecasting International, Ltd. (April 1973).

[1] N. W. Wolman and G. W. Boman, *The Outlook for Water* (Baltimore: Johns Hopkins Press, 1971), p. 21, 59.

Chapter 10

Forecasting And Its Impact On Policymaking

Joseph P. Martino

Introduction

A discussion of the impact of forecasting on policy-making might usefully begin with definitions of the two terms. Webster's dictionary gives the following definitions:

> **forecast**: to calculate or predict (some future event or condition) usually as a result of rational study and analysis of available pertinent data.
>
> **policy**: a definite course or method of action selected from among alternatives and in light of given conditions to guide and determine present and future decisions.

These definitions have some implications relevant to our discussion. The term "policy" implies not just a single decision, but a long-term framework which will guide a series of decisions. Moreover, the policy cannot prescribe in detail, in the present, the decisions to be made in the future, because it serves only as a guide to these decisions. Thus the policy is concerned more with the final outcome or condition than it is with the precise course by which that final condition is to be achieved. As the future unfolds and more information becomes available, decisions will be made in the light of what appears to be the best way to achieve the final outcome.

Implicit in the idea of a policy, then, is a forecast of the final condition or situation which the policy is intended to bring about, or at least to be consistent with. This forecast, however, need not involve "rational study or analysis." It may indeed be purely intuitive and may be completely implicit. An assumption hidden in the topic of this chapter, however, is that we are concerned not simply with intuitive and implicit forecasts, but instead with those which are stated explicitly and are obtained by rational study and analysis. That is, we are concerned with forecasts made deliberately, with the best methods available, and intended to have an impact on policymaking.

However, there is one caveat to this insistence on rational study and analysis as underpinning the forecasts behind a policy. This arises from the concept of "the image of the future" discussed by Fred Polak.[1] The essence of Polak's ideas, as put forth in his bo[illegible] summed up in two sentences from the foreword by Kenneth Boulding: "The image of the future. . . is the key to all choice-oriented behavior. The general character and quality of the images of the future which prevail in a society is therefore the most important clue to its overall dynamics." Polak states that the history of Western culture can be described in terms of the images of the future held at various times by Western peoples. He then proceeds to examine these images, including those of the classical Greeks, the Persians, the Israelites, and the early Christians and then those of the Middle Ages, the Renaissance, and the Enlightenment—up to modern times, with various Utopians and ultimately the Marxists.

The essential consideration, from our standpoint, is that none of these images, influential as they once were (and to some extent still are), could have been derived from "rational study and analysis" of data about the past. These images were all derived by nonrational means and were images of a society (either in this world or in some other) which was different from, and better than, anything that had ever existed before. Indeed, Polak's main concern is that none of these images of the future which once determined choice-oriented behavior in Western civilization retains any power to motivate large numbers of people, and no new images have arisen to replace them. His hope is to spark the creation of new images of the future which can provide Western civilization with the guidance these other images once provided. There is not the faintest trace of a suggestion in his writing, however, that suitable images can be derived by rational means from data about the past. Indeed, an image which is capable of capturing emotions and firing imaginations must have something of the nonrational about it.

Another view of this same concept is expressed by Gray L. Dorsey: "Looking to social facts and choosing actions on the basis of social results desired is not an effective way to guide social and governmental organization and action in the absence of a set of generally accepted values based on commitment to implications of a view of the nature of man."[2] That is, without an image of the future—a future condition or situation which is seen as appropriate for humanity and which is shared by both those who set policy and those who make decisions in furtherance of that policy—mere rationality is insufficient. A set of actions chosen on an instrumental basis, that is, such that if everyone carries them out faithfully it can be demonstrated rationally that everyone will benefit, will not in fact inspire the support needed to achieve the goals of the policymakers. Both those who make policy and those who later execute it must share a vision which is accepted not simply because mutual acceptance leads to a desirable result, but because it is seen by all involved to be desirable and worthwhile in itself.

In addition to these caveats regarding forecasting, some additional comments on policies themselves are in order. Vickers[3] lists five elements of policy-making which, he asserts, are commonly ignored in discussions of the subject, largely because they are not amenable to "scientific" analysis. These elements are the following.

1. Endurance through time. Policies are not concerned with the achievement of goals on a "once and for all" basis. Instead, they are concerned with conditions or relationships which must be maintained through time. For instance, a policy regarding the appropriate degree of education for children cannot be achieved and thereafter disregarded. It must continue through time as new cohorts of children reach school age.

2. Management of conflict. The strictures of "the dismal science" extend beyond the confines of the money economy. All values are in competition for a finite budget of resources which can be expended in achieving them. Some values are in direct conflict, moreover, because they are contradictory. Hence an essential characteristic of policy-making is dealing with and managing these conflicts, whether they concern the degree to which contradictory values will be achieved or the allocation of finite resources among complementary or unrelated values. Nor can this conflict be reduced by increasing the resources available. Increased resources simply add new options by allowing the consideration of values which could be ignored when resources were much scarcer. (That is, a man who has only enough resources to hold body and soul together must allocate among food, clothing, and shelter. If his resources are increased he not only must continue to consider these items, but also must consider allocations for "health, education, and welfare.")

3. Value adjustment. In view of the two preceding considerations, it can be seen that policy problems are solved by setting thresholds or minimum levels which are considered acceptable for each value. Policymaking is therefore an exercise in value adjustment, and it also plays an important part in value creation.

4. Modeling historical process. The policymaker is attempting to regulate a historical process which is irreversible and nonrepetitive. Each event is unique in at least one of the elements of actors, location, or time. In order to determine the probable outcome resulting from certain events, particularly from interventions on his part, the policymaker must model this process, usually by some form of historical analogy. However, such a model can never be validated in the same way that scientific models of nonhistorical (i.e., physical or chemical) processes can. Moreover, even though the policymaker has no choice but to use such a model for forecasting an outcome, strictly speaking such a model cannot be used for rigorous prediction in the same way a scientific law can be used.

5. Modeling the "artificial." Modeling historical processes is difficult enough when they contain only those factors "which would be as they are even if men were not here to observe them." However, the historical processes men are confronted with contain a significant element of the "artificial" arising from earlier policy interventions. The problem is compounded when the results of those earlier interventions contain outcomes which occurred either in place of, or in addition to, those which were intended. These artificial elements are subject to their own laws, differing from the laws which appear to govern purely natural processes (i.e., even if a historical process could be repeated exactly up to the decision point, the outcome might be different because of people's awareness of the outcome "the last time").

Having examined to some extent the nature of both policymaking and forecasting, we can now begin an examination of the relationship between the two. We will examine several models of the policy-making process, which more or less explicitly discuss the role of forecasting. These models are selected primarily because they do examine the role of forecasting. In addition, they represent a fairly wide range of views of the nature of policymaking and thus should provide a wide perspective on possible interactions between forecasting and policymaking. The models will be identified by the name of the author who proposed them, although it is not intended to imply either that the models are unique to the named authors or that the authors themselves would defend these to the exclusion of other models.

Specific Models

Davis Bobrow

Bobrow[4] has proposed a set of purposes for which forecasts can be utilized and has identified criteria by which the adequacy of forecasts intended for each purpose can be judged. Each of his purposes can be viewed as a major type or class of policy-making objective. These purposes, and the associated criteria for forecasts are as follows:

1. Social mobilization. This refers to a policy intended to mobilize support for quickly altering, or else for retaining, the current state of affairs. The forecast must be of dire consequences which will befall society if the proposed change is not adopted immediately or the threatening change is not forestalled. The forecast must be credible, the consequences significant in terms of deeply held values, and the results of the change imminent. The condition described in the forecast must seem irreversible once the immediate action is past.

2. System replacement. This refers to a policy intended to achieve a specific social system at some time in the future—a system which is radically different from that currently existing. The proposed system must be discriminated from all others (Bobrow uses the "classless society" as an example of such a system, fundamentally different from that of today and distinct from all other possible replacements for today's society). Forecasts intended to support a policy of system replacement must show that the end state is distant in time but is an inevitable consequence of a chain of consistent events. The forecast must include short-term and midterm steps which inevitably lead to the new system. Forecasts supporting this type of policy must seem credible—in terms of the consequential linking of the intermediate steps with the final goal—and the intermediate steps must seem desirable in terms of the same set of values in which the final state is seen as desirable.

3. Administrative effectiveness. This type of policy is intended to maximize the efficiency of some agency in meeting future conditions. A forecast supporting such a policy is intended to allow the optimization of current resource allocations in terms of future conditions. Bobrow states that this is the only one of his possible purposes in which accuracy of the forecast is important. This point is discussed below. In addition, the forecast must have the appearance of certainty so that it inspires confidence rather than increasing anxiety, and to be useful it must be linked to the use of policy instruments within the control of the agency for which the forecast is intended.

4. Group perquisites. This type of policy seeks to increase the resources and status of some specified group. The forecast must make the group appear critical for achieving a desired goal or forestalling an undesirable situation. A forecast supporting such a policy must credibly portray a state of affairs which can be coped with only by a subset of specialists, and one with which they can cope effectively.

5. Participatory planning. This refers to a policy under which public groups and individuals are given opportunities to evaluate various futures far enough in advance that choices between distinctly different alternatives can still be made. Forecasts supporting such a policy must portray a range of alternative futures, each of which is seen as feasible in terms of resources available and decisions yet to be made. These futures should have implications for large segments of the population, not simply for elites. The forecasts must portray the possible futures with sufficient clarity that laymen have a basis for choosing among them. Finally, the forecasts must be made sufficiently far in advance that the participatory process can function before it is rendered irrelevant by the passage of events.

6. Collective learning and adaptation. This type of policy can be applied to a large group or a small one. The objective is to permit the relevant group to respond to external events which alter the likelihood of various possible futures, thus taking these events into account instead of pursuing a predetermined program from some starting point. Forecasts in support of such a policy must cover all significant contingencies between the starting point and the desired end point.

Moreover, the forecasts must be structured so the corrective implications of new information are clear. That is, the group involved must be able to determine from the forecast what types of intermediate events must be monitored and the consequences of various interventions following specific possible events.

7. Theory development. This is not, strictly speaking, a policy objective in the same sense as the others. The objective here is improvement of theories describing the flow of events and the consequences of actions. A forecast in support of such a purpose is simply a derivation from the candidate theory, and it must be optimized to subject the theory to an empirical test. The forecast must be sufficiently specific that the occurrence of the forecast outcome can be readily confirmed or disconfirmed. That is, Popper's concept of "falsification" is relevant. If a forecast cannot be shown to be false, after the fact, then it provides no test of a theory and is useless for this purpose. Bobrow points out that forecasts useful for developing theories are likely to have little value for other purposes and vice versa.

Bobrow summarizes his conclusions about criteria for forecasts as follows: Accuracy is important only in forecasts supporting policies for administrative efficiency. For many purposes, face importance is the most weighty criterion. The infrastructure (data base, post hoc predictive success, etc.) of the forecast may be more important, for many purposes, than the actual estimate of future conditions.

Several comments are in order about his conclusions. First, I believe he is in error regarding the importance of accuracy in support of policies of administrative efficiency. He apparently assumes that there is only one possible future and that the agency in question desires to place itself in a posture of maximum efficiency within that future. However, there are always many possible futures, contingent upon decisions not yet made. An agency may well wish to tailor the future to itself rather than vice versa. It must therefore have forecasts of alternative futures available. At most one of these forecasts can be accurate, since the agency will attempt to forestall the others. This is simply the self-fulfilling/self-defeating paradox which I have discussed elsewhere.[5] In the presence of this paradox, "the future" (in the sense of a single future) does not exist, and accuracy in predicting it is not an important criterion. Accuracy is important only in those cases in which the actor has no power to influence the outcome. Almost by definition, this excludes all policymakers.

In addition to the case of administrative efficiency, the concept of tailoring the future, or selecting among alternative futures, is important for the other policy purposes as well. Bobrow mentions it specifically in certain cases but leaves it only implicit in others, as for instance the case of collective learning. Hence for all policy purposes, forecasts should include alternatives, which should be linked to intermediate events and decisions as well as to instruments of policy.

Lead time is mentioned in some cases, but again it is an important criterion for all policy-making purposes. Forecasts should be made sufficiently far in advance that there is time for the policymakers to have some influence over events. Hence the specific policy under consideration, and the time required to implement it, will lead to specific criteria regarding lead time needed in forecasts supporting the policy.

Intermediate feedback, from events occurring between the starting point and the time the results are to be achieved, is also of importance for all policy-making purposes. A forecast should include estimates of intermediate events which can be utilized to check whether the historical process is remaining on the projected trajectory or is deviating to such an extent that the policy is in serious danger of failing.

Finally, while Bobrow mentions credibility for several purposes, it is in fact an important criterion for all purposes. If a forecast is not credible, it will not be utilized as a basis for action. In particular, the linkages between specific possible outcomes and specific choices made at intervening decision points must be credible.

Erich Jantsch

Jantsch[6] has outlined a very complex scheme involving policymaking, strategy, and tactics. His scheme is depicted in Figure 1. Jantsch specifically looks upon policymaking as a process of adjusting values to make them consonant with the environment. For him forecasting takes place at three levels. At the tactical level, forecasting deals with the probabilistic assessment of activities, either those actually being carried out or those which must be analyzed prior to a decision as to which to carry out. At the strategic level, forecasting deals with the identification and even the invention of feasible activities. At the policymaking level, forecasting deals with the invention of anticipations, which Jantsch defines (after H. Ozbekhan) as "intellectively constructed models of possible futures," and which he views as roughly equivalent with de Jouvenel's "futuribles" or Herman Kahn's "alternative world futures." The inventive aspect of

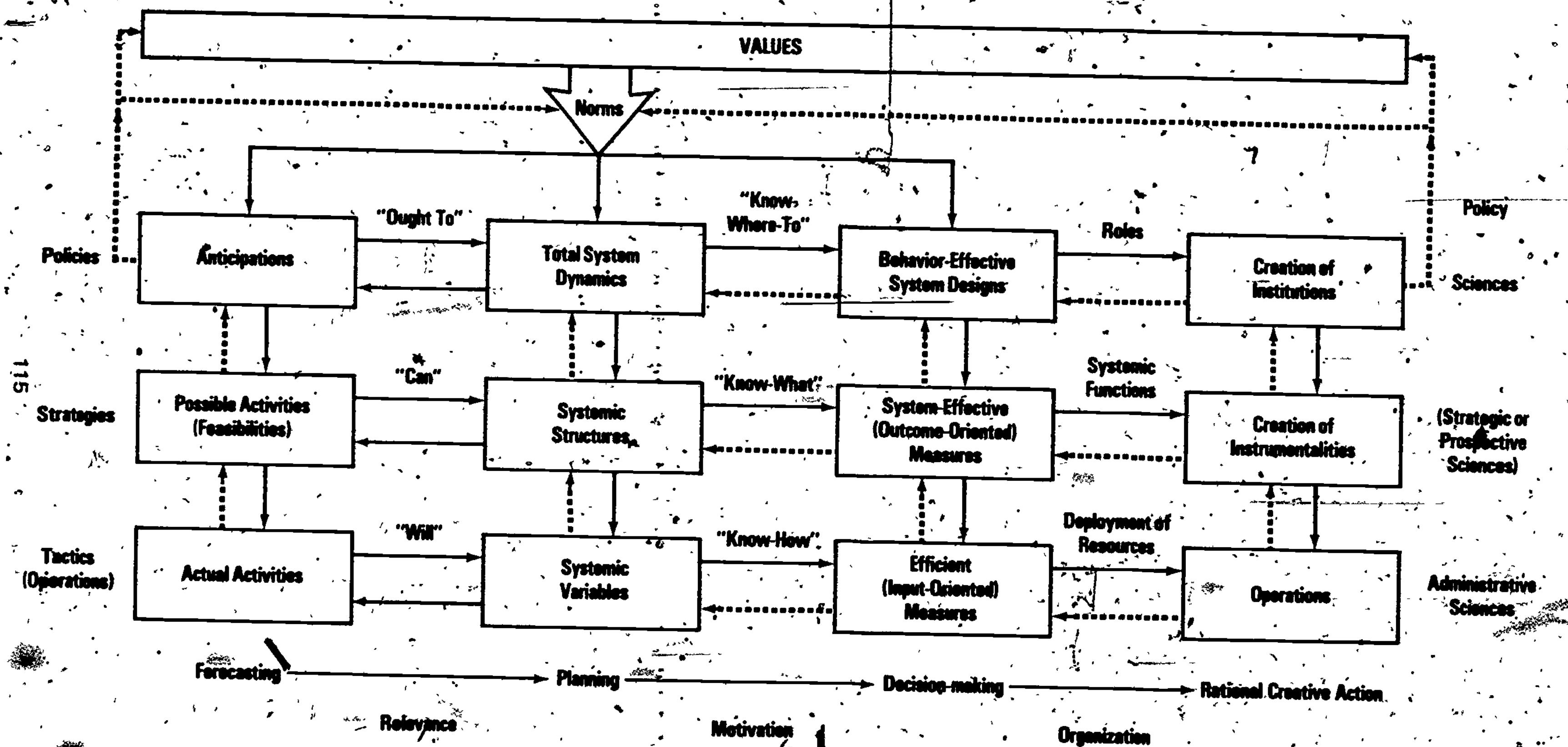

Figure 1. Structured rationalization of creative action. (Dotted arrows indicate feedback.)

forecasting is especially important for Jantsch. Moreover, it is a creative activity. The invention of anticipations cannot be done deductively. There is no algorithm by which the forecaster can exhaustively generate all possible futures. Thus Jantsch's anticipations play much the same role as Polak's images of the future. They are essential at the policymaking level of human activity, they are based on values held by humans and serve to adjust the acceptable thresholds for these values, and they represent the starting point for all choice-oriented activity.

Jantsch does not specifically identify any criteria for forecasts in his scheme. However, several are implied by his discussion. There must be sufficient lead time in the forecast to permit carrying out the policy, especially where this may require the design of new social systems with the preferred dynamic characteristics, and the creation of new institutions. Credibility of forecasts, in terms of the purported linkages between instruments of policy and the state the policy is intended to achieve, is also vital. Finally, forecasts must be capable of providing the inspiration and motivation needed to support the policies to which they give rise.

Yehezkel Dror

Dror[7] presents a model of optimal policymaking. This model includes three main stages, with a total of 18 phases. These are outlined below.

Stage 1—Metapolicymaking

1. Processing values. This involves identifying and making explicit the values held by the policymaker's constituency.

2. Processing reality. This involves identifying those aspects of reality which are relevant to the values identified above and obtaining as accurate an image as possible of those aspects of reality.

3. Processing problems. This involves determining the differences between values held and reality. These differences then constitute problems to be solved.

4. Surveying, processing, and developing resources. This involves identifying those elements of reality which can be used as resources to solve the problems identified above.

5. Designing, evaluating, and redesigning the policymaking system. This involves optimizing the policymaking system to enable it to deal with the problems previously identified.

6. Allocating problems, values, and resources. This involves assigning to subelements of the policymaking system responsibility for solving problems, maintaining values, and utilizing resources.

7. Determining policymaking strategy. This involves determining the orientations and postures of the policymakers (e.g., should they stick to incremental policymaking, or should they attempt risky innovations).

Stage 2—Policymaking

8. Suballocating resources. This involves taking those resources allocated to a single policy issue and suballocating them to the phases of the policymaking.

9. Establishing operational goals with some order of priority. This phase involves establishing goals by each of the subelements of the policymaking system and assigning priorities to them.

10. Establishing a set of other significant values with some order of priority. This involves identifying those values, not directly a part of some problem, which might be affected by solutions to one or more problems and establishing priorities among these values.

11. Preparing a set of major alternative policies, including some "good" ones. This involves delimiting the set of policies to be considered but assuring that the set includes at least one which is "good" in terms of its benefits and costs.

12. Preparing reliable predictions of the significant benefits and costs of the various alternatives. This phase explicitly involves forecasting the costs and benefits of the various alternative policies retained for consideration. Dror points out that predicting the costs and benefits of new and innovative policies is more difficult than predicting the costs and benefits of more familiar policies. The trade-off between reliability of prediction and innovativeness desired in policymaking should have been made at Phase 7, when policymaking strategy was settled. The optimal set of predictions should meet the following criteria:

a. Benefits and costs should be stated explicitly, with a probabilistic assessment of each, where appropriate.

b. Validity of the predictions should be indicated, including explicit identification of critical assumptions and sensitivity of the predictions to errors in assumptions.

c. Probability of unpredictable consequences should be estimated, including critical assumptions regarding these deviations.

d. The time horizon or cutoff should be indicated explicitly, with some estimate made of the spillover into long-term consequences, effects outside the desired sphere of activity, etc.

13. Comparing the predicted benefits and costs of the various alternatives and identifying the "best" ones. This involves search for a dominant alternative, if one exists in the set of policies under consideration or, if no dominant policy exists, identifying those with the "best" trade-offs among costs and benefits.

14. Evaluating the benefits and costs of the "best" alternatives and deciding whether they are "good" or not. It is not sufficient to determine that policy which is the best of those under consideration. It is also necessary to determine whether this policy is good enough to do the job. If at this phase the best is found to be not good enough, a search for new policy alternatives is begun.

Stage 3—Postpolicymaking

15. Motivating the executing of the policy. After a policy is selected, it must be executed. This phase involves those organizational steps needed to allocate resources, give orders, and follow up.

16. Executing the policy. This phase involves carrying out the policy which has been ordered into execution.

17. Evaluating the policymaking after the policy has been executed. This phase determines how well the policy accomplishes what it is supposed to accomplish and identifies lessons for "the next time."

18. Communication and feedback channels interconnecting all phases. The preceding outline has implied that the policy-making model requires these phases to be carried out in sequence and without deviation. In fact a policy constructed and carried out in such a manner could not be optimal. This phase, then, is not the last of a sequence but covers all communication and feedback between phases. For instance, it has already been indicated in the description of Phase 14 that if the best policy is still not good enough, a search for new options must be made (i.e., returning to Phase 11). In his description Dror gives many more examples of such feedback loops which must bind together all the phases of his optimal model. All these loops are subsumed in this phase.

In his model Dror reserves one phase explicitly for forecasting and has derived criteria for forecasts (see Phase 12 above). However, he also anticipates the need for forecasts in other phases as well. For instance, Phase 2 requires identifying not only the past and present reality but future reality also. However, Dror does not derive any criteria for good forecasts of future reality. Nor are criteria for forecasts stated in any of the other phases in which they might be needed.

One of the strong points of Dror's model is the explicit incorporation of what he terms "extra-rational" processes. He recognizes the need for creativity at various phases of the policymaking process, such as the invention of policies when no existing policy alternative is "good enough." Moreover, he states that we have considerable research knowledge about what factors stimulate creativity. Hence we should design the policymaking process so that it incorporates creativity-enhancing factors where creativity is required. The same holds true for many other extra-rational aspects of mental activity. Nevertheless, despite this emphasis on the extra-rational, Dror never explicitly calls for anything akin to Polak's "image of the future" or Jantsch's "anticipations."

Such a forecast might be implicit in Phase 1, which examines existing values, or in Phase 3, which identifies deviations between existing values and existing reality but at best it is only hinted at. In addition, in Phase 11 by implication forecasts must be used to connect outcomes with specific policies. However, Dror does not explicitly call for forecasts at this point. Thus Dror does not seem to assign to forecasting the role of "prime mover" in the policymaking process which Polak and Jantsch assign to it, nor does he assign to it the role of providing credible linkages between input and output as Bobrow and Jantsch do. For Dror forecasting explicitly plays only the role of reducing uncertainty about costs and benefits of alternative policies, once these policies have already been identified. For this purpose forecasts must meet the criteria given in Phase 12.

Graham Allison

Allison[8] has discussed the policymaking process in a framework involving three conceptual models: the "rational actor," "organizational process," and "bureaucratic politics." Each of these is intended to exemplify a particular type of policymaking.

In the rational actor model, the entire policymaking process is personified as a single actor who acts with perfect knowledge and in the light of consistent goals and values. In a complex policymaking system, it is assumed that the activities of lower level policymakers are consistent with and derivable from the policies set by higher level policymakers. Moreover, this model assumes that all the values of the rational actor can be condensed into a single payoff function, so that maximization of that single function becomes the goal of the actor.

In the bureaucratic politics model, policies are seen as the outcome of bargaining among a set of actors, each of whom is the leader of some organization or constituency and each of which has its own interests and goals. These actors do not focus on single issues but instead look at many diverse issues, opening the door to logrolling and "politicking" on individual issues. In the bureaucratic politics model, policies are not made by rational choices involving a single set of values but instead arise from the dickering and horse trading of politics.

In the organizational process model, the emphasis is on the constraints posed by organizational form. Organizations are really information-processing systems. They are designed to take inputs of specific kinds, process them through standard operating procedures, and convert them to specific kinds of decisions or actions. In particular, organizations are intended to solve well-characterized problems. Moreover, a specific class of problems tends to give rise to a specific organization to deal with that class (e.g., problems of international policy are divided into those which are handled by the State Department and those which are handled by the Department of Defense). Thus organizations tend to see the world in terms of the functions they have been established to perform. The policies they espouse tend to be those which require the functions they have demonstrated their ability to perform.

Allison does not address the question of criteria for forecasts to be utilized by each of his actors. However, his models go a long way toward explaining some of the characteristics of policymaking in government and in large private organizations. Therefore it appears to be worthwhile to frame some criteria for each of these actors.

From the standpoint of a rational actor, a forecast must meet the following criteria: it must provide a credible linkage between various possible outcomes and policy instruments under the control of the actor, estimates of the impacts to be expected on all values which are important to the actor, sufficient lead time to permit the actor to make all necessary preparations and changes in his resource allocations, and feedback from intermediate events to allow the actor to be certain his interventions are having the desired effect. Finally, the forecast must provide a sufficient range of futures that the actor can have a high degree of assurance he has located a true (global) optimum.

From the standpoint of a bureaucratic actor, a forecast must meet the following criteria: it must include many features to provide room for bargaining among various actors (indeed, there is a premium on creating options which are credible and which have "something for everybody") and it must estimate the impact of the outcome on the actor's constituency or agency, especially impacts which were not intended or foreseen by those proposing a specific policy. The lead time provided by the forecast must be sufficient to allow the political process to be carried out, especially the bargaining which may involve a multitude of parties and the packaging or bundling of unrelated issues. Moreover, the forecast must provide sufficient lead time to allow successful outwaiting of "filibusters" by actors whose own interests would best be served if a decision become moot through the passage of events. The forecast must provide a credible linkage between the desired outcome and the actions of specific actors and provide feedback from intermediate events to permit reopening of the bargaining if it appears that one part of a package deal is turning out unexpectedly. Moreover, from the standpoint of each actor, this feedback should be early enough that he retains some leverage through actions he has not yet taken in support of policies of greater interest to other actors.

From the standpoint of the organizational process actor, a forecast must meet the following criteria; it must deal with one of the class of problems the organization was established to handle, and it must break the problem down into manageable pieces in a manner which is compatible with the structure of the organization. The forecast must provide sufficient lead time for the organization to carry out the appropriate standard operating procedures and provide feedback from intermediate events. However, these events must be seen as falling within the scope and function of the organization and must be among those events which some portion of the organization is responsible for monitoring. The forecast must provide a credible linkage between the final outcome and actions by specific segments of the organization.

Summary and Conclusions

After examining the policymaking process through several models and identifying the characteristics of forecasts required by each model, we can summarize those elements which are common to several models or which seem to provide important insights even though they appear in one model only. In this section I present some characteristics of forecasts derived from the preceding analysis of the relation between forecasting and policymaking. In addition, I indicate some research problems suggested by the characteristics.

One of the first considerations is that forecasts have impact upon policymaking at several levels. At the highest level, forecasts serve as images of possible futures. These images are the prime movers which drive the policymaking process, which serve to coordinate the actions of all involved in the process, and which motivate people to support specific policies. At a lower level, forecasts serve to identify feasible actions which might be carried out in support of given policies. At a yet lower level, forecasts serve to assess the possible outcomes of various specific actions, allowing participants in the policymaking process to select those actions which are most efficacious in furthering specific policies.

At any of these levels, the characteristics required of forecasts are generally the same. However, at the lower levels there is a greater need for specificity and concreteness and also a focus on the nearer term. At higher levels, the requirement is for greater inclusiveness and for focus on the longer term. Keeping these differences in mind, however, it is still possible to discuss on a general basis the requirements placed on forecasts.

Credibility

One of the key requirements for a forecast is credibility. Without credibility a forecast is incapable of providing a basis for action. The forecast user must believe that the measures proposed can indeed achieve the desired outcomes. That is, the forecast requires face credibility. Beyond that, the forecast must inspire confidence in the correctness of its estimates of the events forecast (whether these are probabilistic or deterministic). There must be a credible linkage between the outcome desired and the actions to be taken or the policy instruments to be utilized in achieving the outcome. Finally, the sensitivity to assumptions and to errors of fact must be indicated to reinforce credibility. The credibility issue gives rise to two types of research problems. One deals with face credibility.

What internal characteristics must a forecast have and how should it be presented to make it credible to the potential user? This becomes especially important if the events forecast are radically different from those within the experience of the policymaker using the forecast. The other research problem deals with the technical issue of forecasting methodology. Which methodologies deserve credence? How much credence? Can new methodologies be devised which are inherently more deserving of credence? How can one tell whether a forecasting methodology deserves credence in a specific situation?

Range of Futures

The range of futures covered by a forecast should be sufficiently large that it provides genuine alternatives to the policymaker. The various futures must not all be minor variations on a single theme, nor should the deck be stacked by the inclusion of only one "preferred" future and several totally unacceptable alternatives. The policymaker should be forced to exercise his entire range of values and to make the trade-offs among competing or contradictory values to assure that the policy ultimately selected does indeed lead to a future which is desirable. Research problems here deal with identifying the value structures of policymakers and their constituencies as well as the technical issue of the construction of scenarios embodying different trade-offs among values.

Lead Time

From one standpoint lead time is the whole point of a forecast. The only reason for knowing about something before it happens is to allow time for preparation. Thus a forecast must provide sufficient lead time for institutional arrangements to be made, for organizational processes to be carried out, for bargaining to be completed, and for decisions and choices to be made by the public and the elites involved. From another standpoint lead time may be beyond the control of the forecaster. He may not be asked for a particular forecast until it is too late for his forecast to be useful, even though he could have provided a useful forecast had he been asked sooner. Beyond this, however, there are some research problems arising from the need for lead time and the inevitable trade-off between precision and time length of a forecast. The farther into the future the forecast must be extended, the less the precision which can be expected (e.g., in assessing the probabilities of various possible events). Research is needed on the precision to be expected from various forecasting methodologies, as a function of length of forecast, uncertainty in data utilized, etc. In addition, better methods are needed which will permit a better trade-off between precision and length of forecast.

Intermediate Events

The forecast must identify intermediate events which are to be expected between the initial intervention resulting from execution of a policy and the achievement of the state or condition which is the objective of the policy. These events provide feedback about the effectiveness of past interventions and provide error signals for changed or additional interventions. The forecast must identify the events to be monitored so the monitoring process can be established as part of the follow-up of policymaking. Research problems here arise from the uncertainty to be expected of a given trajectory through time. Does the occurrence of some event really represent adherence to a trajectory, or is it only loosely coupled to the desired process so that its occurrence comes in spite of significant deviation? Conversely, does the occurrence of an unforecast intermediate event indicate a deviation from the desired trajectory, or is it only a low-probability event which managed to occur despite the overall success of the past policy interventions? Research is needed to define the degree of deviation which can be expected under various circumstances and when various forecasting techniques have been utilized.

Completeness

The forecast should indicate impacts on all the values held to be important by the policymaker or his constituents. In addition, the forecast should indicate the space and time horizons or cut-offs it considers and estimate the degree of spillover beyond these cut-offs. Research problems here deal with what have been called "unknown unknowns," that is, things the forecaster does not know and does not realize he does not know, as contrasted with "known unknowns"—those things he is aware he does not know. Research on forecasting techniques is needed to reduce the incidence of unknown unknowns by identifying where specific techniques have blind spots or tendencies to break down. Once a forecast is needed it is too late to identify unknown unknowns. The forecaster should already be aware, as a result of prior research, of the pitfalls inherent in the methods he is using so that he may take appropriate steps to bypass them.

Clarity

The forecast must have clarity sufficient to provide a basis for decision. If the forecast is not clear about what events are and are not expected to occur, or if the identity of the events themselves is unclear, the forecast provides no help to the policymaker who must choose among alternative policies. Lack of clarity may arise from incompetence or deliberate ambiguity on the part of the forecaster. Barring this, however, research problems exist both in forecasting methodologies that are capable of distinguishing among events which are superficially similar but which are perceived by the policymaker as having highly different value structures and in methods of communicating forecasts to the policymaker so that he understands the value implications of different possible events. This latter is particularly important because the forecaster and the policymaker tend to come from different backgrounds and to use different technical jargon, hindering communication.

Structure

The structure of the forecast should be compatible with the structure of the policymaking system so that the overall policy issue can be decomposed into discrete subpolicies assignable to specific subordinate elements. That is, the structure of the forecast should match the structure of the organization which must comprehend and use the forecast. Research problems exist here in cataloging types of organizational structure and the effect of the communication process in various organizational structures on the use of forecasts. Ideally, research in this area would enable the forecaster to tailor his forecast so that it was isomorphic to the policymaking system which will have to use it.

Image

An "image" forecast must be capable of motivating and inspiring those who will execute the policy and those whose support (active or passive) is needed to execute the policy. Research problems here involve identification of the values of various publics and elites and the trade-offs these elements have among the values they hold. This information can be used not only to predict likely support or opposition among various groups but also to tailor images which provide a satisfactory degree of appeal to wide segments of the population. Put in a less manipulative way, this will allow tailoring policies which indeed respond to the aspirations of significant segments of the population.

FOOTNOTES

[1] Fred Polak, *The Image of the Future*, Elise Boulding (trans.), (San Francisco: Jossey-Bass, 1972).

[2] Gray L. Dorsey, "Constitutional Obligation," in J. Roland Pennock and John W. Chapman (eds.), *Political and Legal Obligation* (New York: Atherton Press, 1970).

[3] Sir Geoffrey Vickers, "Commonly Ignored Elements in Policymaking," *Policy Sciences*, Vol. 3 (1972), pp. 265-266.

[4] Davis Bobrow, "Political and Social Forecasting: Purposes, Criteria, and Recent Emphasis," in Walter A. Hahn and Kenneth F. Gordon (eds.) (New York: Gordon & Breach, 1972).

[5] Joseph P. Martino, *Technological Forecasting for Decisionmaking* (New York: American Elsevier, 1972), p. 12.

[6] Erich Jantsch, "From Forecasting and Planning to Policy-Sciences," *Policy Sciences*, Vol. 1 (1970), pp. 31-47.

[7] Yehezkel Dror, *Public Policymaking Reexamined* (San Francisco: Chandler, 1968).

[8] Graham Allison, *Essence of Decision, Explaining the Cuban Missile Crisis* (Boston: Littke, Brown, 1971).

Chapter 11

Problems in Futures Research*

Ida R. Hoos

Gertrude Stein once consigned a place to instant oblivion by her comment, "There is no there there." Similarly, although with less nicety of dispatch, futurologists have succeeding in disposing of time. For those of us in whom the dizzying kaleidoscope of the past half-century has inspired the question, "Is it now already?" they have a ready answer: "The future is now." This compression of the time element should simplify their task, because that which is here and now, i.e., the present, should be considerably more amenable to study and understanding than the unfathomed future. But the semantic collapse of the time dimension does not carry over into the logic or the logistics of the futurologists. When we examine their concerns, we find them delving into the "post-now" with little attention to the obstacles and methodological deficiencies that impede a surer understanding of where we are *now*, let alone where we are going.

This is a serious oversight, because the future is longer by far than the present and the mistakes are greater by many orders of magnitude. The difficulties stem from two main interrelated problems: (1) the easy assumption but lack of a scientifically verifiable technique for studying that which has not happened and (2) the dearth of reliable data, the *sine qua non* for "good" research. This chapter will concern itself mainly with the second problem—it is my contention that the information base, how it is derived and used and by whom, is critically important in understanding social processes, whatever their time frame: past, present, or future. And a review of all three temporal foci casts much more doubt than light.

Such being the case, this chapter starts with its conclusion, viz., that given the present state-of-the-art of "futures research," the classic example of the Greek sibyls, who produced deliberate doubletalk, may be the wisest, safest, and most desirable in the final analysis. As for criteria for "good" futures research, there are none identifiable now; it is doubtful that there can be any, and it might be healthier for the future if there were not any anyway.

History and "Facts"

If accuracy be the criterion, then even past and present are subject to interpretation. "The facts," says Edward Hallet Carr,[1] "speak only when the historian calls on them: it is he who decides to which facts to give the floor, and in what order or context." Carr quotes lines spoken by a character in a Pirandello play: "A fact is like a sack; it won't stand up till you've put something in it." Understandably, then, there can be as many views of the past as there are historians, who try to document what has happened, and historiographers, who try to make sense out of it all. For Spengler[2] the dynamic was clear, the course determined. He scanned the past and traced the life course of the various cultures. For ours he foretold destruction, mainly because of technology, which, he argued, had altered the face of the earth and made high priests of its engineers. But, he promised, the machine, "the real queen of this century," will succumb because the last conflict is at hand—the conflict between money and blood. "And so the drama of a high culture—that wondrous world of deities, arts, thoughts, battles, cities—closes with the return of the pristine facts of the blood eternal that is one and the same as the ever-circling cosmic flow." With one direction, obligatory and willed, we "have the freedom to do the necessary or nothing." In other words, we are doomed if we do and doomed if we don't!

Toynbee disagrees. Although he discerns uniformity in the disintegration and dissolution of civilizations, of which he identifies 21, he nonetheless argues that the cyclic view would, if taken seriously, "reduce history to a tale told by an idiot, signifying nothing."[3] He attributes the demise of earlier civilizations to their failure to meet the challenges of their time but contends that modern man *could* achieve the purposeful enterprise, the divine plan, except for one thing—the formidable

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technology that has linked all mankind under the menacing mushroom cloud of total annihilation. Where once the "eggs of humanity were happily distributed among many baskets," so that some could remain intact even while others got broken, now they are all vulnerable to the same technologically induced dangers.

Sorokin sees technology not as the driver but as a manifestation of a prevailing *Zeitgeist*.[4] Forty years ago, long before anyone thought of using Michelangelo's David to advertise men's clothing or putting the Mona Lisa under a hair dryer, Sorokin provided us with detailed characteristics of the Sensate Culture, which has been used by Kahn and others as the mold for the future.[5] His term, *Sensate*, was literally applied to indicate that only that which was presented to the sense organs is real. Sorokin said that in this type of era the great cultural values of the past would be degraded: "Michaelangelos and Rembrandts will be decorating soap and razor blades, washing machines, and whisky bottles." In the Sensate Culture, values are empirical and material, always relativistic and lacking in sacred, eternal imperatives. Its pervasive goal is control over nature and other men; science and its system of truth dominates.

As Sorokin reviewed earlier cultures which had gone through this period, he looked for trends and foresaw crisis: "The boundary line between the true and false, the right and wrong, the beautiful and the ugly, positive and negative values, will be obliterated increasingly until mental, moral, aesthetic, and social anarchy reigns supreme." "Freedom will become a mere myth for the majority and will be turned into an unbridled licentiousness by the dominant minority." "Governments will become more and more fraudulent and tyrannical, giving bombs instead of bread, death instead of freedom; violence instead of law; destruction instead of creation." "Divorces and separations will increase until any perceived difference between socially sanctioned marriages and illicit sex relationships disappears." "Quantitative colossalism will substitute for qualitative refinement, the biggest for the best; a best-seller for a classic . . . technique for genius; . . . 'operational manipulation' for an enlightening intuition." "Suicide, mental disease, and crime will grow. Weariness will spread over larger and larger numbers of the population."

No doubt there are many historians who would question Sorokin's "facts"; certainly his "law of immanent causation" has not won the universal acclaim and acceptance that was his dream. But this is not to belittle his accomplishment—and without the sophisticated tools which are the *sine qua non* of today's futurologists. One cannot but be impressed with the aptness of the fit of the Sensate Culture for our "point in time"!

Accepting Carr's dictum that "the belief in a hard core of historical facts existing objectively and independently of the interpretation of the historian is a preposterous fallacy;"[6] we are faced with the ineluctable conclusion that accuracy is not a useful criterion in judging the historian's craft. Indeed, forging the facts of history has long been a respectable pastime, exceeded only in popularity by the posthumous debunking of events and defrocking of personages. Any doubting Thomas can sing as did Sporting Life: "It ain't necessarily so!"[7]

Troublesome with respect to the past, matters of fact become acute as we approach the present. There are, first of all, such epistemological considerations as how we know what we know. Walter Lippman, long ago, in an insightful essay on "stereotypes," pointed out the impossibility of attaining pure objectivity. Myrdal suggested that at best we can only acknowledge our bias. In recent years, social scientists, with their ready acceptance of verbal and conceptual droppings from the hard sciences, have happily adopted the "Heisenberg Principle" for their own, the vulgate being that the experimenter himself is inevitably in the experiment and plays a crucial role in its outcome. Of course, this is nothing new, but it sounds so much more impressive when expressed in the language of physics. (Long observation on this matter suggests a rule: the "softer" the science, the more prone it is to this kind of *Bourgeois Gentilhomme* behavior.)

Moving from the philosophical to the practical, we come face to face with the problem of information in our time. In our zealous pursuit of data, we have all but forgotten that *data* are not, as the Latin origin suggests, *given* but are actually *gotten*. Our almost fanatic reliance on data points up a faith not justified, an attribution of reliability more wishful than real; or it may perhaps be a conscious, cynical trade-off on the assumption that anything is better than nothing. Much has been written about the data explosion, the mythology of the more the better, and the technocratically derived nonsense that an "information expert" is someone equally capable of handling the machines and what goes into them.[8] We are the new Frankensteins, and we have created a monster—the information system. First we rationalize it by justifying its "feasibility." Then we build it, with an appetite exaggerated by our expectations of its performance in our service. As a consequence, we must keep feeding it. Its digestive capacity being greater than our consumptive capabilities, we soon find ourselves innudated.[9] Finally, we are devoured and our purposes are subverted—the whole enterprise an information-gathering and processing exercise having little relevance to the task at hand.[10]

Data, Documentation, and the Research Function

No matter what techniques for forecasting the future are utilized, much will depend on the reliability of the data on past and present. The past, we have seen, is a vast and rich reservoir in which *fact* turns out to be a fishing expedition. What we know officially about the present is derived from the body of facts and figures on which public policy is based and decisions made. Although mindful of the slips 'twixt cup and lip—the vagaries, vicissitudes, and downright perversity of the factors bearing on decisions in the public sector—I nonetheless deem it important to consider the mechanisms through which the government gathers its information. Important now, this matter becomes crucial later in this chapter when *social indicators* are discussed. There is, to begin with, the Federal statistical system, at the core of which are the agencies whose primary mission is the preparation of statistics about people, organizations, and activities. The greater portion of Federal statistics comes not from statistical but from operating agencies and relates more to government than to private matters. Thus not only are there the descriptive series of economic and demographic indexes, such as the Current Population Survey, the gross national product, the Consumer Price Index, birth and death statistics, and the like, but also there are the wider range of statistical matters pertaining to weapons evaluation, social experiments, agricultural field trials, and reliability measures for NASA's calculations, as well as prediction of medical costs. In 1971 a President's Commission on Federal Statistics completed an evaluation of the present performance of the Federal statistical system and issued a two-volume report.[11] Some of the material presented is merely descriptive:

> Data . . . arrive in federal files in various forms. Basic data are generated by the federal government, in joint federal-state operating and statistical programs, and in a great many state and local service, regulatory, and law enforcement activities. At each level of government, data are gathered by the agencies and through contracts with private firms and colleges and universities. Data originated by the federal government are obtained as observations supplied by respondents on their persons or activities, as direct measurements and counts made by government enumerators, and as internal records arising out of the operations of agencies. Some data received by the federal government from original collections sponsored by states and localities are received as copies of primary unit records, and other data from similar original collections arrive in federal agencies as summaries for the different jurisdictions. Although some editing and processing of data gathered by the federal government are done by federal agencies, these functions are also performed by the states and localities on much of the data originally gathered by them, with different degrees of intergovernmental coordination. Federal holdings also include considerable collections done outside government. *Thus, many hands, public and private, federal and nonfederal, participate in collecting, editing, summarizing, and processing the data held in federal records. There is enormous diversity in the sources and characteristics of data used by the federal government.* [Italics added][12]

Even this brief selection, although uncritical in its intent, suggests possibilities for enormous unevenness in quality. One of the members of the commission, William Kruskal of the Statistics Department at the University of Chicago, subsequently commented, "Much of that huge cloud of statistical thought and action, a cloud that suffuses all government activity, is not carried out by people called statisticians, or trained as statisticians. Much of it is not regarded as having important statistical components. Consequently, much of it is of poor quality."[13] Kruskal noted that a great deal of Federal statistics, whether or not it is specifically so labeled, is performed under contract by private firms and is of highly variable quality. With present contracting and review procedures unlikely to effect improvement in professional standards, he proposes this as an item of top priority for the Committee on National Statistics, an activity of the Division of Mathematical Sciences in the National Academy of Sciences/National Research Council.

However necessary such a step might be, it is far from sufficient to overcome the complex of factors working against "improvement" of federal statistics as now conceived. One stumbling block is the quantity—and quantitative—orientation of the whole enterprise, the more so as whatever gets gathered must fit into a prefabricated "system." Possessed with the urgency to achieve compatibility, especially in view of increasing interaction between levels and layers of government, and obsessed with notions of machined efficiency, committees have time and again been caught with their trained incapacity showing.[14] The National Data Center of ill repute in the 1960's keeps resurfacing under such guises as *Reorganization* or *Coordination*, with convenience of handling and processing the predominant desideratum. Not surprisingly, the 1971 Report recommends for decisions regarding statistical programs a formal cost-benefit approach:

> In determining any year's budget allocations, the ideal procedure would be to compare the costs and benefits of every proposed expenditure and support only those for which the benefits exceed the costs.[15]

This method is suggested even though accompanied by the caveat that "the comparison of costs and benefits cannot really be made unless it is possible to specify the benefits of the program in a measurable way and, particularly for statistical programs, that is difficult."[16] On this score, Eckler[17] reminds us of another point worth reiterating here: "Efforts to systematize the measurements of benefits result in giving undue weight to minor factors that can be readily quantified and ignoring major elements that are hard to measure." Anthony Downs[18] called this horse-and-rabbit stew!

Quantomania has encouraged premature (and even spurious) precision. For example, a U.S. Department of Agriculture report to the Office of Management and Budget dutifully produces a benefit-cost ratio of 1164:1 for research on live poultry handling and a ratio of 928:1 for research on scab-resistant white potato varieties! Such precision on the part of the USDA is all the more miraculous when considered in light of that agency's calculations in the 1973 wheat sale to the Soviet Union. Along with the notorious information lags and errors that crystallized the trade with Russia and catapulted U.S. food prices, we have evidence of an underestimate by agriculture department experts of 60 percent on farm prices of wheat. By setting the figure at $2.49 a bushel, the government paid wheat farmers a $475-million subsidy, unwarranted because the USDA's own action, to export vast quantities of wheat to Russia, drove the price to $3.99!

Similar instances of the hardening of soft data are the more frequently found as the cost-benefit calculus becomes mandatory as the rationale for government funding. The consequence is that public program planning comes to involve criteria like high yield or a favorable benefit-cost ratio and, by hook or by crook, it *is* achieved! Sometimes a figure is offered to meet a deadline and becomes quoted in several places; then, by apparent corroboration, it is crystallized into firm input.[19]

Another obstacle to "improvement" of federal statistics lies in the very nature of bureaucracy, its first order of business being self-preservation and its first law of procedure being pursuit of the path of least resistance.[20] Consequently, although high-minded commissions and consultants may prescribe ways to upgrade the enterprise, the prospects for fundamental change are very dim. Regulatory agencies have shown themselves dilatory to the point of sloth and procrastination in taking steps to assure the accuracy of the information they process and provide. Scrutiny as to how the Food and Drug Administration, the Federal Trade Commission, and the Environmental Protection Agency get their information, what they do with it, and whose purposes it serves uncovers example after example of error, miscalculation, and sometimes downright deceit.

With regard to the FDA, new evidence emerges with every Congressional investigation. The Intergovernmental Relations Subcommittee of the House Government Operations Committee reported in December 1973 that the FDA had continued to allow dosage of the hormones diethystilbesterol (DES) in animal feeds, to increase growth of beef cattle and other food animals, even though evidence was accumulating that DES was capable of causing cancer in some animals and that traces of the hormone could not be kept out of the food products. Moreover, the subcommittee report accused the agency of being tardy in warning physicians that use of DES as a drug in pregnant women was unsafe.

On the other hand, the FDA took precipitate action with respect to cyclamate artificial sweeteners, which Americans were consuming at the rate of 16 million pounds per year. In October 1969 cyclamates were banned in foods because evidence showed that they had caused cancer in rats. However, regulations did permit their sale as nonprescription drugs, a decision reversed some 10 months later. On the strength of the 1958 Delancy Amendment to the Food, Drug, and Cosmetic Act, then HEW Secretasy Robert Finch was forced to prohibit cyclamates, even though there was not then and has not been subsequently evidence of their having caused cancer in human beings. Representative L. H. Fountain, heading an investigation by the Intergovernmental Relations Subcommittee,[21] found that Finch did not want to eliminate completely sales of products already containing cyclamates. Coincidentally, an FDA official mentioned in his testimony that California fruit growers had just ended the canning season and that a significant portion of the fruit had been canned with cyclamates. It was shortly after the ban that the *Washington Post* reported that HEW was planning legislative strategy for the elimination of the Delancy Amendment. By rechristening cyclamates as a drug instead of a food additive, the agency demonstrated its own brand of strategic administrative expediency and circumvented its own normal procedures of scientific evaluation. Fountain regarded this as a subterfuge for evading the law, "discouraging . . . at the time we are trying to impress upon our young people the importance of law and order."[22]

An abortive effort to construct an information system for the German Federal Health Bureau points up the complexities of managing information relating to drugs.[23] First, the designers soon learned that the bureau's mission, i.e., thorough and conscientious evaluations, was virtually impossible. Clear-cut policies were lacking, and *information* lay at the very heart of the obstacles. With some 60,000 drugs on sale in Germany, an unknown number available in other countries and likely to be imported, and both categories increasing rapidly, the number of drugs to be monitored was tremendous. Second, drugs are not readily classified and catalogued. Single chemical compounds are related to one another in complicated ways, indicative of their complex molecular structures. Their relations in terms of medical effects are even more intricate. Third, effectiveness is not a binary scale but contingent upon the disease under treatment. A highly effective preparation can produce serious side effects, even the risk of death. Fourth, if the pharmaceutical firm eager to market the drug is the primary source of information about it, *effectiveness* as a criterion may be overstated.

Other sources of information about a drug are difficult to classify and evaluate, because they are heterogeneous and variable as to authoritativeness, e.g., medical and pharmaceutical journals, clinical reports, experimental results, rumor, etc. Most important, current information about a drug may and often does prove to be far less significant than what is discovered through use, through combination with other drugs, and through correlation of information with as yet undetected sources, particularly with the advance of analytic and measurement technology.

Information on crime is surely no less suspect, in spite or because of expensive police information networks with grids so fine as to ensnare potential jurors along with criminals![24] Crime in the streets is the tub-thumping theme, and much is made of figures on robbery, aggravated assault, and the like. A few years ago, then Acting Attorney General Robert H. Bork cited a 3 percent rise in what the FBI rates as "serious crime" (murder, rape, robbery, and the like) and made the following remarkable nonsequential pronouncement: "This increase in serious crime in the nation means that all of us must continue to support our local law enforcement agencies in their efforts to combat crime in the streets."[25] However, if one were to examine how the Safe Streets appropriations have been spent, one would be struck with their subversion to the busy work of data-gathering and processing, all rationalized by the notion that this is the way to "fight crime."

Crime statistics being notoriously what the police report and definitions of crime being at best legalistic and at worst tautological, information systems are pouring forth a flood of data on purse-snatchers and muggers. And from this we have derived a raft of sociologistic correlations between crime and poverty, poor housing, unemployment, and other deprivations. As a consequence, apologistic measures have been the popular mode of response. Implicit here is the notion that amelioration of his lot would keep the felon from "maturing his felonious little plans" or "the enterprising burglar from aburgling" to paraphrase the Sergeant in *The Pirates of Penzance*.[26]

Forgotten in the pother over tactics for waging war on poverty and crime in the streets is the possibility that crime may correlate with gross national product, that dealings in narcotics, security fraud, and buying of political candidates and high officials to ensure special interest are all manifestations of big business. A New York City police consultant warns that crime today is "computerized, financially sophisticated, and has mastered the arts of systems analysis. Using computers, crooks are setting the sights of major crime at new highs," he observes.[27]

Because the old-fashioned crook is relatively easy to count and characterize, the system has concentrated on him. But how do our statistics reflect organized crime, especially the kind that involves high-level officers and police? How do our criminal justice information systems yield useful data on hijackers who have been known to cause sovereign nations to capitulate under threat of massive destruction and massacre? How about the Arab terrorists who immobilized a vital transportation nerve center, like London's Heathrow Airport, by threatening to shoot down airliners with Soviet-built surface-to-air missiles[28] or who bully the United States by threats to drop bombs on missile installations, atomic energy plants, and other vulnerable sites—all for a good cause? Before long we may have to decide whether we will, like some governments, accept terrorism as a method for implementing a policy, national or international. Perhaps we will exempt from the crime category such activities as lawbreaking to gain some socially acceptable purpose, like electing a president, discrediting a political adversary, or topping a potentially obstreperous foreign government. In so doing, will we adopt the accommodating "interpretation" that accords to terrorism the dignity of expressing dissent that should be respected or to outright corruption the legitimate status of a technique for rallying support for some cause? Whatever happens, the crime information in our files will have little relevance; and improvement, as envisioned through coordination, may be just more of the same old bureaucratic voodoo to conjure reorganization into appearing like a new vision.

The census figures are not sacrosanct. This 180-year-old institution, created by Thomas Jefferson and anointed in the First Article of the Constitution of the United States, has been criticized for its coverage, its gaps, and its propensity for unwelcome snooping. Because the decennial census was, however, supposed to be a source of reliable and impartial information, it has served as the basis for many kinds of official planning. But its information can have serious repercussions, for example, as in the case of the alleged undercount of Blacks in the cities. The National League of Cities once tried to get a Secretary of the Treasury to provide additional Federal funds on the argument that the data in the 1970 census were false and the consequent decisions inequitable. The league claimed that there was an undercount of Blacks of 7.7 percent, or 1.88 million persons, more than four times the undercount rate of 1.9 percent for whites.[29] This discrepancy would directly affect revenue-sharing, with Newark, New Jersey, losing $.5 million. In similar fashion, local welfare rolls are cut and officials make political mileage out of their "management reforms" only to disclose *later* that the decline stems from errors in classification, recording, and reporting.[30]

Doctoring the data is not an uncommon practice in the field of medicine. The files of Medicare are full of services not rendered, drugs improperly administered, and a gamut of other abuses which, taken gullibly and in the aggregate, would convey a distorted picture. But some misstatements are venial and due to bad reporting rather than evil intent. Such, for example, is the information on kidney transplants and the comparative mortality record of large versus small hospitals which comes from statistics gathered by the Joint Registry on Human Kidney Transplants in Chicago of the American College of Surgeons and National Institutes of Health. Commenting on the report issued in the *Journal of the American Medical Association*,[31] Belding H. Scribner, a world-renowned kidney specialist, said that the report reflected a falsely low death rate in small centers. "All the nonreporting centers are small, and my guess is that the worse their results, the less eager they would be to volunteer to report them to the transplant registry, even though the report is kept confidential."

Information on unemployment is obfuscated by the popular ploy of redefinition. Sometimes the jobless are counted as those who are actively seeking employment, a simple semantic expedient that automatically excludes the hundreds of thousands who have stopped investing in the futile effort of reporting to their local Department of Human Resources (formerly known as the State Employment Office). A series of artices[32] by the former chief of the U.S. Bureau of Labor Statistics and now vice-president of research, National Bureau of Economic Research, suggests a new statistic. Called "index of unemployment severity," this figure and the analyses based on it reach the conclusion that the state of the economy in general and the severity of unemployment in particular were not then as bad as some critics maintained.

Similar semantic games may be played to promote the cause of special interest groups. The Cost of Living Council, for example, withheld information submitted by the country's 700 largest corporations, i.e., those with an annual revenue of at least $250 million. On the ground that the figures required were not "numerically identical" with those supplied by the SEC, the council kept data on costs and profits from the very public that the gathering of them was intended to serve. This was the situation that prevailed until a lawsuit, brought by the Consumers Union, ended in a ruling by a Federal court that the Cost of Living Council had been withholding the information illegally.[33]

After reviewing even these isolated instances of contradiction and confusion, can we seriously expect bureaucracies to improve their practices vis-a-vis data? I think not. In the game of musical chairs in Washington, it is likely that the persons called on to assess and revise are the very ones who had a hand in the establishment of the enterprise. Any intelligence postmortem is likely to be conducted by persons closely related to the ongoing endeavor. Such intelligence coroners tend to accept old assumptions and otherwise avoid boat-rocking. If truth hangs in the balance, no one recognizes it, for the show must go on.

Still another impediment to improvement of information stems from the nature of bureaucracy and its quest for the path of least resistance, as suggested in the foregoing paragraphs, and derives nurture from it. Here I refer to the strangle-hold that PPB (planning-programing-budgeting) and its complements and components, cost-benefit analysis, and the like, have had on the very philosophy of government. These "sophisticated tools of management," introduced into the Department of Defense by then Secretary Robert S. McNamara and developed conceptually and operationally by his assistant Charles J. Hitch, were credited with having won the Battle of Britain and the War on the Potomac; they certainly contributed to the mythology of methodology that still pervades government.

But their triumph was mostly one of supersalesmanship, for Senator Proxmire's committee later disclosed that the Pentagon, far from representing a paragon, was, on the contrary, an example of gross mismanagement and that the very system singled out as the triumph of utilization of the "total system procurement concept" was, instead, an egregious instance of spectacular cost overrun and productive short-fall. Nonetheless, program budgeting managed to survive and, indeed, flourish—its banner loyally transplanted on civilian soil by Whiz Kids who soon occupied important posts in agencies and in peripheral "research institutes." On August 25, 1965, President Lyndon B. Johnson proclaimed program budgeting (PPB) as the prescribed method for Federal planning, with wishful statements about this system's having "proved its worth many times over in the Defense Department" and now bringing to each department and agency "the most advanced techniques of modern business management."[34] PPB, he rhapsodized, would "illuminate our choices" of programs to bring the Great Society closer to all the people:

> For example, how can we best help an underprivileged child break out of poverty and become a productive citizen? Should we concentrate on improving his education? Would it help more to spend the same funds for his food, or clothing, or medical care? Does the real answer lie in training his father for a job, or perhaps teaching his mother the principles of nutrition? Or is some combination of approaches most effective?

Such faith was patently naive; PPB could not, by its very nature, "illuminate such choices." All it accomplished was to cast government program planning into Procrustean forms so that there were few choices that *could* be made. Everything became part of the cynical numbers game; and everyone had to play the game in order to survive. For every agency, division, and program, the *apologia pro sua vita* was cast in cost-benefit terms, and where figures were lacking, they were dutifully conjured.

So pervasive has been the influence of PPB that even in death its grip is strong. Although a June 21, 1971, memorandum from the OMB marked its official demise in Washington, the hinterlands are still busy adapting their procedures, and they can be expected to continue until at least the end of this century. Moreover, and more important, PPB has refused to die without issue. Its legacy lives on, for example, in the form of PIE (Planning-Implementation-Evaluation), a suspiciously similar permutation in which the "evaluation" of programs is in quantitative, "output," "production" terms. Consequently, the data generated are no different from and no better than those associated with PPB. What is important, with respect to this factor preventing an eventual improvement of information, is that the need ultimately to satisfy the OMB influences directly the kinds of research that will be encouraged, who will be the recipients of grants to support the research, what form research results make take in order to be acceptable, and whether the research will continue to receive financing.

Under the present circumstances the superprecious benefit-cost ratio, no matter how preposterous, will carry more weight than a thoughtful exposition of factors considered relevant or research conclusions that elude the measuring stick. During the past decades, as universities have become increasingly dependent on Federal funding, the academic community has had to resort to survival tactics. They have had to go through the ritualistic motions of "producing" in the format required while at the same time preserving the integrity of their research. If reconciliation is impossible, they have one of two choices—to capitulate or to perish, because grantsmanship now has been institutionalized as indicative of productivity in university calculations, too. Education having become big business, it is subjected to the management techniques that were spun off by the Department of Defense. If scrupulosity prevents a faculty member from producing as prescribed, the funding agency can turn to an ever-ready "research institute" or private think tank, eager to oblige with a "quick-and-dirty" job.

Another impediment to improvement in Federal information is couched in the hidden agenda of government activities. These vary from time to time and are subject to the style and whim of a particular administration. The fact of their existence prevents access to information and controls its use. We can know them only by their signs and manifestations. Use of the "classified" label is the first and most obvious of these. Standard practice ever since the rubber stamp came into being, this method of imposing secrecy has been used to the point of overkill. A best seller on the subject[35] documents the ways in which "secret" and "top secret" have been used for varying purposes by successive Administrations. The case of Daniel Ellsberg and the "Pentagon Papers" highlights some of the problems. Subsequent inquiry into abuses of the "classified" label were intended to open old files and clear them of items no longer secret. Although considerable declassification was the intent, the only apparent result has been refinement of an already overburdened structure of classifications.

A more recent case involves the current "energy crisis" and former President Nixon's message to Congress on September 10, 1973, to urge that the Navy's Elk Hills petroleum reserve in California be opened to the oil industry for development. Representative John Moss's testimony before the Senate Commerce Committee criticized the Department of the Interior for its failure to safeguard the public interest and showed it instead "to be an adjunct of industry in such a manner as to constitute a public disgrace." Similarly, the Navy and the Justice Department were excoriated. The U.S. naval commander who was deputy director of naval petroleum and oil shale reserves resigned his commission rather than support the administration's position. Lieutenant Commander Kirby Brant is quoted as having told a Congressional source, "I have told my last lie."[36]

Sporadic moments of "operation candor" on the part of the former President did not offset the deceptions and doubletalk of his loyal henchmen and White House spokesmen who turned out to be less reliable sources of information than usually unreliable sources, like the underground press. Secrecy keeps any number of boondoggles going. The label of secrecy also serves as an arbitrary device to keep research results inaccessible.

The secret agency works in inscrutable ways through various channels. The institution of the Scientific Advisory Committee is a typical example,[37] with the Surgeon General's Scientific Advisory Committee on Television and Social Behavior as a case in point. This panel was set up with the support of the National Institute of Mental Health to investigate the impact of television violence on the behavior of children. The broadcasting industry was allowed to review and veto the appointment of potentially hostile critics and, at the same time, was given prominent representation on the panel. Blacklisted through this procedure were seven candidates whose professional credentials were impeccable but whose research results suggested that viewing of televised violence was not a catharsis for pent-up aggressions. Commenting on the appointment process, James J. Jenkins, chairman of the Board of Scientific Affairs of the American Psychological Association, is quoted[38] as saying, "It looks like an exemplar of the old story of the 'regulatees' running the 'regulators' or the fox passing on the adequacy of the eyesight of the man assigned to guard the hencoop." One of the committee's own staff members, Douglas A. Fuchs, senior research coordinator for the million-dollar project, observed that "the scientific independence of this study has obviously been subverted to some kind of political consideration."[39]

Silencing the critics can be accomplished in many ways, as the Center on Corporate Responsibility learned. This small public interest group conducts research and litigation to improve business behavior in such areas an environmental protection, minority hiring, and product safety. The Internal Revenue Service denied the organization tax-exempt status, a death blow since this deprives it of contributions from tax-exempt foundations and donations by individuals. The IRS action has been interpreted[40] as a manifestation of discriminatory practice, in that corporations are allowed to treat their tremendous expenditures on lobbying as tax-deductible business expenses. "American tax and corporation laws have in fact created a dangerous imbalance between corporate and public interests. In the harsh light that Watergate has shed upon the way special interests are able to influence political decisions, Congress and the courts need to reexamine tax laws and rules that apply so unequally to business organizations seeking to further their self interests and to groups seeking to further social interests."

Futures Research—Prolegomena and Perspectives

Forecasting of various kinds and in numerous guises constitutes a considerable category in the "research roster" of the 1970's. A particular favorite appears to be science and technology forecasting, perhaps because these are relatively less complicated by human, social, and political vagaries. One might expect, then, that barring such developments as secret weapons, the present state-of-the art vis-a-vis science and technology would be clearly accessible for appraisal and the future at least logically conceivable. Quite the contrary. We find that shelves of books, specialized journals, and high-level conferences from Santa Monica to Stockholm have not yet brought us even to the elemental stage of having achieved a usable definition, to say nothing of an understanding, of the dynamic for and direction of change. Involved here are the complex interrelations between science, technology, and public policy, with causality not a link in an endless chain but rather appearing in circular guise.[41]

The National Science Foundation undertook to develop a set of indices, which it said "would reveal the strengths and weaknesses of U.S. science and technology in terms of the capacity and performance of the enterprise in contributing to national objectives."[42] The aspirations were lofty; such indicators were presumably to assist in improving the allocation and management of resources for science and technology and "in guiding the Nation's research and development along paths most rewarding for our society."[43] This verbal macrame soon revealed itself to be patterned after the scrimshaw that decorates proposals and other less noteworthy documents. The National Science Board labored and produced a catalogue of current research and development activities. Listed were such items as "Science and Engineering Personnel," containing such unenlightening entries as "current pool," "enrollments and degree production," and "supply and utilization." Instead of supplying the promised "indicators," the report poured forth a deluge of detail, not all of it particularly pertinent, on funds, manpower, and equipment. It then concluded that present limitations in data and methodology and the current paucity of necessary indices limited the conclusions that could be drawn concerning the quality and effectiveness of our scientific and technological effort!

Other researchers in the field publish articles on equally dogged exercises that explore the obvious and produce tepid conclusions. A typical article takes as a null hypotheis the proposition that information processing technology and applications are not advancing more rapidly than predicted by the forecasts, analyzes a set of selected forecasts, and produces the following incestuous validation:

> *Although the data are insufficient to provide conclusive evidence,* the results suggest that information processing technology and applications are advancing more rapidly than predicted by the forecasts. Various long-range forecasts are in general agreement. Information processing will be of increasing importance for future forecasts both as a subject and as a tool for processing data. [My italics.][44]

At meetings, accounts of science and technology futures research take the form of elaborate paradigms and schematized displays, most of which are platitudes cast in the future tense. Wallowing as our generation still finds itself in the murky, uncharted currents of assessment of technology,[45] we just may be unready to predict future trends. Indeed, Lanford[46] finds that we have not yet even reached the point where classification and standardization of terms has been accomplished.

Certainly, if the technological response to the current "energy crisis" can be taken as a clue to our capability to assess and predict in this one specific area, then the demonstrated state-of-the-art is in a sad state of disarray. In the United States the "Project Independence 1980," which former President Nixon defined as "a series of plans and goals set to insure that by the end of this decade Americans will not have to rely on any source of energy beyond our own," is based on wholly improbable assumptions. The givens that were taken for granted cannot be gotten. We cannot by 1980 build more nuclear power plants than are already on the drawing boards, produce any substantial amount of synthetic crude oil from coal or oil shale, or expect substantial development of solar and geothermal energy. Moreover, there still remain serious doubts as to (1) the safety of nuclear power reactors and (2) our capacity for storing and disposing of the vast amounts of fissionable by-products of such operations.

Professor David Inglis warns[47] that we should not put too much credence in the frequently cited argument that there have been no calamitous accidents associated with the 30 reactors which have been producing power for years. This, he says, does not prove that they are safe, but only that "if we are to have a thousand or so reactors in the future we will probably not have more than a few calamitous accidents per year. Engineering assessment of the precautions taken is more reassuring than that, but the claim that the nuclear plants are safe is a matter of hope, not experience." The problem of energy is multifaceted, with technology, economics, resource allocation, and social goals interacting and with no consensus about any of these factors. The situation is so confused that former President Nixon on January 19, 1974, felt impelled to deliver a statement over the radio on the credibility of the "crisis" itself. It is the view of a professor of nuclear engineering at MIT that part of the difficulty stems from the lack of a rational, long-range energy policy. He observes that "the Federal Government has developed little capability to develop data on fuel demand

and resources and has been content with petroleum-industry data."[48]

The "technology forecasters" have shown only plodding pedantry in their preoccupation with dredging up definitions. Dodging real-life concerns, they have been engrossed in pencil-and-paper games, the intellectual doodling dignified by names derived from the Greek. Where knowledge was called for, they have given us what the French so aptly term *fumisme*.[49]

Their fanciful flight put us in the twenty-first century before we learned to live out the reality of the twentieth. Note, for example, the contribution of "experts" in a Xerox survey:[50]

> Ground Transportation: Tubes underground with propulsion by air or magnetos will take care of many mass transportation needs inter- and intra-city. In many cases high-speed above-the-ground monorails or tubes will accomplish the same results. Computer control will virtually eliminate the possibility of accidents. Hook-on conveyor type transportation will be used for individual vehicles and will operate at high speed in safety and comfort by eliminating the decisions and discretion now prevalent in personal driving. An individual will watch 3-D television or play cards while traveling at high speeds. Energy for these vehicles and their control devices will be through nuclear and magnetic means as well as solar and fuel cells, all of which will operate with great efficiency and the power to replenish themselves. Included herein are many new devices to operate beneath the sea.
>
> Air Transportation: We will be sending material and people around the world strapped into devices like space ships which will be similar to our present guided missiles and rockets but on a huge scale and with much more sophisticated controls. For short jaunts a magnet-controlled luxurious flying saucer type device will be prevalent.

Energy, the very element that turns out to be crucial in this excursion into the never-never land, was dismissed in cavalier fashion as though practically a given. Actually, international events of the past few years have brought the entire world to the realization that its energy sophistication is at low level, and that despite the great show made of nuclear, solar, and even laser technologies, we are still vitally dependent on fossil fuels.

With continuation of the energy crisis, many of the props of our social universe—prosperity, equality, justice, and humanitarianism—are tottering. In the vacuum that has been created by what can only be attributed to technical lag in failing to assess this critical aspect of industrialized civilization, politics, propaganda, and pressure tactics have become the prime forces.

If our forecasting techniques for technology were so weak that the past few years' rediscovery of the wheel caught us unprepared, with the result that foreign manufacturers enjoyed a bonanza and road planners had to improvise bicycle lanes on highways and bridges; that the revival of the long-defunct Iron Horse as a mode of transportation found the minimal services of Amtrak totally inadequate to meet the demand; that the triumph of aircraft, "the King of the Fleet," the 747 jumbo jet that caused airports to relocate and rebuild their facilities almost overnight could become the dinosaur of the Jet Age because it requires 40,000 gallons of fuel for every hour it flies; that the key symbols of American affluence, Cadillac and Continental cars, could become such a drag on the automobile market that manufacturers have been forced to close down production and lay off hundreds of thousands of workers, with the usual ripple effects on the total employment picture—if not a glimmer of these important occurrences appeared in the crystal balls of the forecasters—can we count on them to foretell with any degree of credibility the future of man's social world?

For an answer we must first look at the methods and tools in use, then at the tangible record, and finally at the forecasters themselves. As to the first, we find an eclectic gallimaufry, borrowed from a heterogeny of disciplines and possessing none of its own. Consequently, anyone who cares to do so can profess expertness. Not surprisingly, since systems analysis is the predominant paradigm[51] of our technocratic era, we find "the systems approach," now appearing in the future perfect, prominently in vogue. Think tanks, East and West, and East-West international research institutes, as well as ever-growing satellites, all purport to study, forecast, and design the future, with this technique curiously and ubiquitously applicable in all these discrete usages.[52]

The systems approach attacks the future in much the same way as it deals with the present. However, we find the same tools and techniques used now without the constraints imposed by real-life tests in the present. Possessed with the zeal to appear scientific, the futurologists have gratefully glomerated fragments of borrowed methods in an eclecticism that bespeaks that lack of an intrinsic discipline. Typical are the references made to the work of L. A. Zadeh, an electronics engineer who has been studying the use of linguistic variables in place of or in addition to numerical variables in systems analysis.[53] Zadeh's notion of "fuzzy sets" seems to have brought to fuzzy thinking a respectability through language far in excess of its contribution to conceptual clarification. What is especially significant in this adoration of Zadeh is peculiar selectivity. The fuzzy algorithms are accepted and cited with enthusiasm; studiously avoided, however, is the very basis for his contention that "the conventional quantitative techniques are intrinsically unsuited for dealing with humanistic systems or, for that matter, any system whose complexity is comparable to that of humanistic systems."

Zadeh's entire approach rests on "the principle of incompatibility," which appears nowhere in the citations of his futurologistic followers. The essence of this principle is that "as the complexity of a system increases, our ability to make precise and yet significant statements about its behavior diminishes until a threshold is reached beyond which precision and significance (or relevance) become almost mutually exclusive characteristics. It is in this sense that precise quantitative analyses of the behavior of humanistic systems are not likely to have much relevance to the real-world societal, political, economic, and other types of problems which involve humans either as individuals or in groups."

Because, as with systems analysis of current phenomena, *data* are the *sine qua non*, a prime difficulty has been that of a data base. For this futurologists have relied heavily on "social indicators," which was a legacy of the Johnson Administration and represented an attempt to duplicate in the social sphere that which had been accomplished with respect to economics, viz., a system of indicators that would contribute to social accounting just as economic indicators describe the behavior of the economy. Much has been written about *Toward A Social Report*,[54] which was intended to lead to a regular social report on the Nation similar to the Annual Report of the Council of Economic Advisers. Although its conceptual weaknesses far outweighed its strengths, the report remains a kind of landmark and has served as a springboard for much more of the same, and for critical analyses.

One important basis for criticism should be the notion of emulating the economy. Popper[55] reminds us that the analogy between the two kinds of reports was misleading in that the economy could draw on a genuine, full-scale model provided by Keynes. No such model of the social system exists, and the countless partial models are simply inadequate imitations. There is, moreover, the matter of predictive power. For all the well-developed tools of the economists, tremendous divergencies appear in the forecasts. In its 1973 year-end analysis of business conditions,[56] the Morgan Guaranty Trust Company of New York warned that current forecasts of economic developments in 1974 "rest on much flimsier foundations than usual." Reviewing its previous year's performance, the report said, "What actually happened in 1973 bore little resemblance to what had been anticipated. Most important of all, the optimism of forecasters that inflationary pressures would remain reasonably moderate proved to have been totally unfounded."

The annual meeting of the American Economic Association in December 1973 was described as an assembly "of a penitent and humble band of economists," penitent about their failure to forecast the inflation that had enveloped the economy in 1973 and humble about their ability to see what lies ahead in 1974.[57] The prevailing theme came forth unequivocally in the opening remarks by Walter W. Heller, the association's incoming president: "Economists are distinctly in a period of re-examination. The energy crisis caught us with our parameters down. The food crisis caught us, too. This was a year of infamy in inflation forecasting. There are many things we really just don't know."[58] In his lecture to the American Mathematical Society, Paul A. Samuelson, a Nobel Laureate, made this pithy comment, "Economists should be humble, for they have a great deal to be humble about!"[59]

Suffering from all the drawbacks and deficiencies presented in our earlier discussion of information, social indicators as envisioned by those who aspired to become social accountants are additionally burdened by fuzzy conceptualization, faulty assumptions, and feeble credibility. The very notion of a value-free system of social statistics is patently impossible. In an enlightening and comparatively little-known document,[60] the U.S. Department of Health, Education and Welfare acknowledged this impossibility, but by that time the movement had taken on a dynamic of its own and has subsequently gone into business for itself, with foundation grants, government projects, and the usual propulsion mechanism at work. Along with pointing out the inadequacies and inappropriateness of social indicators as a device to accomplish the purposes set for them, the report offers some interesting *caveats*:

> The very labeling of data as "social indicators," with the normative implications which labeling carries, may be misleading in its implications. Indeed, the omission of items which cannot be

measured is itself a major bias—often the most important facts about social condition are qualitative, derivative, or interactive.[61]

One might also add, with an *amen* to the above, that not isolated facts but issues in their complexity might be more meaningful as indicators than mere countable dribs and drabs. But because ideas, much like spurious figures, become legitimated by reference and repetition, the notion of social indicators has managed to attain such an aura of respectability that even Daniel Bell,[62] no neophyte in these matters, makes wishful remarks about their possible efficacy in the context of his forecasting.

Inevitably, consideration of criteria for "good" futures research brings us to an assessment of the futurologists themselves. Who are they? What are their credentials and claims to credibility? Here, oddly enough, we can find few definitive answers and instead must depend on observation. As we stated earlier, futurology as we see it is no "discipline" but an agglomeration of derived activities, including anything from mathematics to public relations. A gravid "Encyclopedia of the Future"[63] has a roster, we are informed in the advance publicity, of some 65 "eminent authorities" already committed to contribute and 45 more to come. Their articles will appear thematically in five volumes titled, "Politics and Economics," "Science and Technology," "Society," "The Individual," and "The Arts and Leisure." "Members of the Hudson Institute in Paris, the European office of the internationally known 'think tank' headed by the economist Edmund Stillman, chose the list of authors and their topics and will provide intensive editorial direction."

Perhaps herein lies the clue to the identity of futurologists—they are people selected by Herman Kahn and his cohorts as authorities to set forth, in articles favored with Hudson Institute's own brand of "intensive editorial direction," their thinking about the unthinkable and perhaps the unthought of in their fields. But this would account for only the *future* experts on the future. Of the *present* experts on the future, it can be said that their expertness is so likely to be positively correlated with distance in time and space from the here and now that they remind one of the character on Ko-Ko's famous "little list"—the person who had a predilection for "all centuries but this and ev'ry country but his own." With systems analysis the prime stock-in-trade of these adventurers into the unknown, we might share the further deprofessionalizing note offered by an aerospace engineer: "There are two occupations," he said, "for which previous experience is not necessary. One is streetwalking, and the other is systems analysis." Such is the terrible state of the futuribles.

Having begun this paper with its conclusion, it is only right that I conclude with its beginning. I said then that, given the present state-of-the-art of futures research, the classic example of the Greek sibyls, who produced deliberate doubletalk, may be the wisest, safest, and most desirable in the final analysis. If accuracy is a criterion for prediction, then who the futurologist is, what he stands for, and what his values are become crucial. Intrinsic in the matter of being right is the now-familiar and generally recognized element of the self-fulfilling propensity. But there is more. Under the influence of B. F. Skinner, psychologists are flocking backward, like lemmings to the sea, to the behaviorism of the first quarter of this century. Ignoring all the moral and ethical implications and cynically contriving an argument with his own version of a "humanistic" basis, Skinner is the leader of a new movement calling for the development of a social technology.

> What we need is a technology of behavior. We could solve our problems quickly enough if we could adjust the growth of the world's population as precisely as we adjust the course of a spaceship, or improve agriculture and industry with some of the confidence with which we accelerate high-energy particles, or move toward a peaceful world with something like the steady progress with which physics has approached absolute zero (even though both remain presumably out of reach).[64]

Experimentation in behavior modification is going on in unwonted places. Platt tells of progress in his laboratories at Michigan.[65] Others are displaying their own research results on this latter-day conditioning of responses, all in pursuit of new "scientific" dimensions and in violation of what surely would be ethical standards of the profession if any manage to survive. Nor have sociologists stood firm as a bastille in defense of humanism. Instead, displaying a *vade mecum* that indicates the insidious grasp of the dominant paradigm, sociologists are being exhorted[66] to "revitalize their discipline" by "cross-fertilization" with "engineering, architecture, systems sciences, and experimental psychology" where social technology is being developed. An impassioned polemic castigates sociology for being an "unexciting discipline." Sociology is urged to recognize "the rich field of information being produced by experimental psychology." (This turns out to be a rediscovery of rats, rodents, and our winged friends, the pigeons.)

But also, and more ominously, there are emerging "external reinforcement contingencies" or social intervention techniques, otherwise known as "reinforcers," that can virtually assure that a future—once having been established as feasible and, by some calculus, desirable—will occur. And lest there be any obstacle, there is also a prescribed *G*uarantor *Of D*ecisions, "a superordinate authority," which can make the future to order and, perhaps by no mere coincidence, the futurologist right.

FOOTNOTES

[1] Edward Hallett Carr, *What Is History?* (New York: Knopf, 1962), p. 9.

[2] Oswald Spengler, *The Decline of the West*, Vol. II (New York: Knopf, 1929), pp. 506–507.

[3] Arnold J. Toynbee, *Civilization on Trial* (London: Oxford University Press, 1946), p. 14.

[4] P. A. Sorokin, *Social and Cultural Dynamics* (Boston: Porter Sargent Publishers, 1957), pp. 698–701. This is a condensation of the distillation from Sorokin's four larger volumes, *Social and Cultural Dynamics* (Englewood Cliffs, N.J.: Bedmenster Press). Their contents were familiar to Sorokin's Harvard and Radcliffe classes in social and cultural dynamics during the 1930's.

[5] Herman Kahn and Anthony J. Weiner, "The Next Thirty-Three Years: A Framework for Speculation," *Daedalus*, Vol. 96, No. 3 (Summer 1967), pp. 705–733.

[6] Carr, *op. cit.*, p. 10.

[7] Song in George Gershwin's *Porgy and Bess.*

[8] Ida R. Hoos, "Information Systems and Public Planning," *Management Science* (Application Series), Vol. 17, No. 10 (June 1971), pp. B-658-672.

[9] "Defense Intelligence Organization Criticized," *Aviation Week & Space Technology* (August 3, 1970), p. 17: "Washington. Competing interservice intelligence activities are collecting and processing so much information, 'it is difficult to find out what is important,' according to Gilbert W. Fitzhugh, chairman of the Blue-Ribbon Defense Panel."

[10] A case in point is the NIH program on alcoholism, which started out commendably enough with consortia of health services approaching the problem as a complex of physical, psychological, and social maladjustments. With the allocation of a contract to a corporation to devise a "management information system," the whole endeavor deteriorated into a data-handling exercise. Ironically, since the patients are "captive," i.e., referred to treatment by police and courts, they represent only the lowest socioeconomic strata. The "information" they yield is mostly redundant, extremely limited, and virtually useless as an index to alcoholism, which still remains a serious and persistent problem. A proper "information system" would have included the medical data of private practitioners, but their files would not fit the pigeonholes and punch cards of the system devised by the "information experts," because doctors are likely to record the diagnosis as "cirrhosis of the liver" or some such related disease.

[11] *Federal Statistics*, Report of the President's Commission, Vols. I and II (Washington, D.C.: U.S. Government Printing Office, 1971).

[12] *Ibid.*, Vol. II, pp. 45–46.

[13] William Kruskal, "The Committee on National Statistics," *Science*, Vol. 180 (June 22, 1973), pp. 1256–1258.

[14] Edgar S. Dunn, Jr., "The Idea of a National Data Center and the Issue of Personal Privacy," *The American Statistician*, Vol. XXI (February 1967), pp. 21–27.

[15] *Federal Statistics, op. cit.*, Vol. 1, p. 107.

[16] *Ibid.*, p. 108.

[17] A. Ross Eckler, "Some Comments on *Federal Statistics*" (Report of the President's Commission on Federal Statistics, Vol. 1), *The American Statistician* (April 1972), p. 13.

[18] Anthony Downs, "Comments on Urban Renewal Programs," in Robert Dorfman (ed.), *Measuring Benefits of Government Investments* (Washington, D.C.: The Brookings Institution, 1965), pp. 342–351.

[19] Professor Leonard Merewitz cited these examples to demonstrate a similar point in a recent report.

[20] For this point I am indebted to Jack N. Schuman.

[21] "Report on the Regulation of Cyclamate Sweeteners," U.S. House of Representatives, Intergovernmental Relations Subcommittee of the Committee on Government Operations, Washington, D.C. (October 1970).

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[25] "Violent Crime Up 3% for 9 Months," *The New York Times* (December 28, 1973).

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Forecasting When The Future Is Known: The Case of The Soviet Union

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Doctrinal Background

Most of us know Communism in any of several forms: as an overarching field of study ("scientific socialism"); as a theory of history and of social change ("historical materialism"); as a theory of reality and of cognition ("dialectical materialism"); as a global political movement ("the socialist revolution"); as a doctrine for the forceful transformation of precapitalist and capitalist states into socialist states ("Marxism-Leninism" or similar phrases); as a guide to revolutionary strategy and tactics ("the party line"); as a blueprint of specific achievement in avowedly Communist countries ("the plan"); and as a unique configuration of personalities and institutions that embody, express, interpret, and control these various kinds of thought or action ("the soviets," "the party," "the party leadership," and so on). These and still other guises that could be mentioned differ from each other in important ways, and none by itself actually defines "Communism"; but they are all alike in at least one essential respect: each rests upon a claim to the same special and infallible knowledge of the future.

This body of knowledge is special because it is limited, not so much in scope as in time. That is, in subject it ranges as widely as to include some very large features of the future development of all socio-economic systems in the world, while in time it moves outward only to that vague but never too remote point at which these systems will have achieved full classlessness, *sans* "dictatorship of the proletariat," *sans* party, *sans* state. The knowledge is infallible because, in the first instance, it is "scientific" (i.e., it is said to follow from a thorough analysis of conditions which are objectively rooted in concrete experience) and, second, it is impossible to show that it is not scientific (i.e., the rules of analysis and discourse are apparently such that contradictory results are inadmissible).[1]

A few examples will illustrate the species of foreknowledge falling in this category. First, there are statements of opportunities that can and will be taken. Thus Engels writes in 1882:

> Since the historical appearance of the capitalistic mode of production, the appropriation by society of all means of production has often been dreamed of, more or less vaguely, by individuals, as well as by sects, as the ideal of the future. But it could become possible, could become a historical necessity, only when the actual conditions for its realization were there . . . . This point is now reached.[2]

Similarly, the draft of the Program presented by Khrushchev at the 22nd Congress of the Soviet Communist Party in 1961 declared: "The world capitalist system on the whole is ripe for the social revolution of the proletariat."[3]

We might observe in passing that the fact that this statement refers to the same "objective conditions" described by Engels almost a century before, and yet is weaker, would not be taken by a Communist as an indication that it was at all suspect. Apart from doctrine, there is a long tradition among Communists that the timing of a *revolution* cannot be forecasted, even though the event itself is inevitable. Thus Lenin wrote in early 1917 that "revolutions . . . for which the date and the chances of success can be told in advance, never happen,"[4] and in 1918 he ridiculed a prediction that "the imperialist system" (i.e., the most advanced capitalist countries) would begin to collapse later that year:

> This formulation is . . . childishly inaccurate, despite the scientific pretensions. It is natural for children to understand science to mean that it can determine in what year, spring, summer, autumn and winter, the "collapse must begin." These are absurd efforts to ascertain what cannot be ascertained. No serious politician will ever say *when* this or that collapse of a "system" "must begin."[5]

A second kind of indisputable knowledge about the future describes methods that must and will be used in the achievement of Communism. For example, the notion was long prevalent (and may still be) that war between the Communist and non-Communist states is inevitable, and it is still asserted that the transition from capitalism to socialism can occur "only" through revolution.[6]

A third kind of knowledge involves tactics—specifically, the conditions for their success. In the strongest statement of its type, Lenin once wrote: "Nothing but an objective account of the sum total of all the mutual relationships of all the classes of a given society without exception, and consequently an account of the objective state of development of this society as well as an account of the mutual relationship between it and other societies, can serve as the basis for the current tactics of the class that forms the vanguard."[7] This call for perfect information was viewed by Lenin as a strict inference from "the general principles of [Marx's] materialist-dialectical outlook." The fact that Communists also believe that such full knowledge is not attainable would make this statement look foolish indeed were it not at least partially offset by two other facts: first, the "vanguard" class (i.e., the proletariat) has its own vanguard, known as the party; and, second, the party believes that it cannot err if it holds fast to Marxism-Leninism. All future decisions, therefore, will be correct, provided only that they are based on the then-current version of this philosophy. This idea was most blatantly stated in the notorious *History of the CPSU (Short Course)*:

> The power of the Marxist-Leninist theory lies in the fact that it enables the Party to find the right orientation in any situation, to understand the inner connection of current events, to foresee their course and to perceive not only how and in what direction they are developing in the present, but how and in what direction they are bound to develop in the future.[8]

The final kind of knowledge of the future bestowed by the various forms of Communism is the most important of all: definite assurance of how the foreseeable future will turn out. Said Lenin at a Soviet Congress in 1921: "The new society . . . is inevitable. Early or late, twenty years earlier or twenty years later, it will come."[9] The idea has been repeated so often that it is a commonplace, sometimes stated matter-of-factly:

> . . .the question arises: Can Communist society be brought about? Our answer is "yes." About this the whole theory of Marxism-Leninism offers a scientific explanation that leaves no room for doubt. It further explains that as the ultimate result of the class struggle of mankind, such a society will inevitably be brought about.[10]

—sometimes exuberantly:

> It is now becoming more and more clear that the end of the sway of capitalism is drawing near . . . and that capitalism is a system that has outlived its age and is bound to perish. The future is ours! The future is for Marxism-Leninism! The future is for communism![11]

From time to time, one not only hears about the certainty of the event, but also is given an idea of when it will occur. Thus, in 1952 G. V. Kozlov wrote that "the second half of the 20th century will produce the complete victory of communism throughout the entire world."[12] The draft Program of the 22nd Soviet Party Congress in 1961 was extremely vague about the advent of Communism in the world at large ("sooner or later"), but rather specific for the Soviet Union:

> The present generation of Soviet people shall live under Communism. Socialism has trimphed in the Soviet Union completely and finally. Within two decades the Communist society will on the whole be built in the U.S.S.R.[13]

Inevitable victory on a national or global scale carries with it, obviously, a number of other profound changes that can also be foreseen with absolute certainty. The most significant of these, from a doctrinal point of view, are the many rewards to the individuals who are there as it happens. All of these rewards flow from the fact that the exploitation of man by man will have ceased; the principle, "from each according to his ability, to each according to his needs," will have come into force. No exhaustive listing of the tangible consequences of this development has ever been attempted, of course. But some specifics have been enunciated. For example, in December 1964, the entire issue of *USSR: Soviet Life Today* (a magazine later renamed *Soviet Life*), was devoted to answering questions frequently asked by Americans about Communism. Prepared by a team of experts under the editorship of S. G. Strumilin, the replies are not only authoritative, but candid and often highly eloquent. From them, we can glean many insights into the future that Communism predicts for individual citizens. Among the certitudes are the following, all of which are quoted (the numbers in brackets identify the pages):

—An abundance of all the good things of life will be free and at everyone's disposal [5]. Trade, money and credit will gradually disappear [5-6]; no money will be used [12]. Nothing will limit the needs of the individual except his own good sense and moral values, his self-respect and his respect for the interests and needs of the other members of society [12].

—Full social, economic and cultural equality of all members of society, i.e., . . . the complete absence of classes and social grouping [6]. [A] woman will be . . . independent in every respect, the equal of her husband where her opportunities for creative work are concerned [29].

—Differences will be eradicated between city and village, between manual and mental work [6].

—Science will no longer be harnessed to military ends . . . Science and production will really be joined in pursuit of their common goal—the satisfaction of the continuously growing requirements of the people. New scientific findings and inventions will be almost immediately applied in industry and agriculture, while a grateful population will pay tribute to the researchers and innovators responsible [7-8].

—All accounting—economic calculations, planning, statistics and economic efficiency—will be rated in kind and in units of contributed labor, i.e., working time [6]. The gauge of people's needs will be provided by statistics of consumption and the sum total of individual orders [12].

—Planning will become more complicated. [But] the sorting and summarizing of the millions of orders from consumers, the choice of the best technological methods, the most efficient allocation of resources, the deployment of plants, the balanced development and coordination of industries, the drafting of the entire system of indices, operative and statistical calculation and control, etc., will all be done by cybernetic computers and information machines manned by an army of highly skilled personnel [12].

—The special group of people professionally engaged in the management of public affairs will disappear. Each individual will devote some part of his time to civic work [4].

—State activity . . . will lose its political character. [There] will still be bodies in charge of the national economy or the planning of scientific research . . . But these will not be state bodies since their activity will be determined exclusively by economic, scientific, and other such considerations, not by political considerations [4].

—There will be far more leisure time . . . . People will have to give less and less of their time to production—four, three, two, or perhaps less hours [13].

—Work . . . will be purely voluntary, nothing compulsory about it [13]. No one, [moreover,] will be chained to a single vocation. With his comprehensive and very thorough . . . training, the future citizen . . . will be equipped to move from one occupation to another when he so chooses [56].

—The need for any kind of coercive laws will disappear, as well as the need for maintaining the state apparatus. It will be replaced by the public self-government of free people, and the laws will turn into customs and habits that will have become the standards of communist morality . . . . Disputes and disagreements between people or conflicts between society and certain individuals . . . will be resolved purely by persuasion [13]. The courts, the police, the organs of state security and the army . . . will disappear completely [4]. [While] there will . . . be occasional breaches of the rules of society . . . they will be individual moral lapses of no great social importance. Many of the crimes so common today will be unknown . . . . Other crimes will be inconceivable to people with the ethical concepts of the future, or will simply be senseless . . . . Personal morality will become the accepted regulator of social relations and will take over the job now done by law and administrative coercion [30]. Conflicts will be settled with dignity and kindliness. [And] a growing role will be played by friends and comrades—by the collective as the educator of the citizenry, as the "deliverer" of man from his personal imperfections [28].

—The future family will lose its present *economic* importance. [Love,] mutual respect and spiritual kinship . . . will be the only foundation for the family . . . . People will marry only for love. [But not all marriages will last, and there will be] greater freedom to invalidate marriages [29]. Society will undoubtedly take a greater part in supporting and bringing up the younger generation than it does today [but parents will not] renounce the joy of communing with their children [or] give up their role of initial mentors [29].

—Man's hunger for knowledge and his eagerness to master new skills will be as spontaneous and natural as his need for work, rest or sleep. Every man will have limitless opportunities to develop and use his abilities. Communism will at last make it possible for the human personality to flower [13].

—Religious conceptions of life and nature, of man and his place, role, and purpose, as well as other superstitions and prejudices, will disappear . . . . All this will be replaced by a sober scientific and materialist view of life. There will be only one faith in communist society—faith in man, in his energy, his labor, in the unlimited creative potentialities of his free spirit, in his intelligence armed with the all-conquering force of knowledge [29].

—Communism will do away with class, national, religious and racial antagonism . . . . Such vices as greed, cupidity, hypocrisy, egotism, brutality, and licentiousness will be gone. But this does not imply that . . . tastes and views will not differ [or that] communist society will . . . be static [30]. There will never be an end to pioneering thought and action [51]. No matter how harmonious and free we imagine our fully constructed communist society to be, history will undoubtedly confront its people with new problems. This is the guarantee that mankind will never be deprived of the fascination of exploring the unknown, of the joy of creation, the thrill of struggle [56].

In short, when Communism is achieved, "the struggle for individual existence disppears . . . . The whole sphere of the conditions of life which environ man, and which have hitherto ruled man, now comes under the dominion and control of man, who for the first time becomes the real, conscious lord of nature, because he has now become master of his own social organization . . . . The extraneous objective forces that have hitherto governed history pass under the control of man himself. [And] man himself, more and more consciously, [will] make his own history."[14]

These universal propositions about opportunities, techniques, tactics, and outcomes comprise a literally foreseeable future that is, in its own way, as chiliastic, comprehensive, and redemptive as any ever cooked up by a St. Augustine or a New Utopian.[15] Nonbelievers will find it equally as dubious or absurd, though for our purposes that fact is irrelevant. Nor does it matter that this image of the future is accepted as true by millions of persons in the socialist countries and elsewhere. What is of interest here is that this future, augmented by a political lore and fired in a crucible of awesome experience, is shared as a starting assumption by the specialists in forecasting and planning in these countries—and by their employers. Presumably, the consequences for the theory and practice of forecasting, in all of its dimensions, are significant. With reference especially to forecasting efforts in the United States, where whatever visions that may be given are certainly not taken as final, we should expect to find that the Communists entertain quite different preferences among techniques, standards of evaluation, perceptions of theoretical difficulties, criteria of implementation, constraints on choice of subjects to be forecasted, and so on. And this is indeed the case, as we may see by examining some of the literature on forecasting from the Soviet Union.[16]

Twentieth-Century Attitudes

The first and most important point that emerges from this literature is also the most obvious one: The role of forecasting and the value of any particular forecast is determined by the larger theoretical context provided by doctrine—including specifically (but by no means exclusively) what the doctrine proclaims as inevitable in the future.[17] This seemingly innocuous observation has exceedingly wide-ranging implications. For example, it means that normative forecasting, if possible at all, is possible only in the short term and only insofar as it answers the question, "What might be the next best step or two in the inexorable march toward Communism?" All the rest must be purely exploratory, since the ultimate outcome is not only known, but is a function of past and present forces which, in their essential thrust, cannot be diverted.[18]

Far more noteworthy, however, is that the subordination of forecasting to doctrine carries with it the consequence that forecasters must be subservient to the interpreters of doctrine, principally the party apparatus and, within it, the party leadership. The importance of this relationship is at least two-fold. First, the party is committed to its own past as a major guide to future policy; thus, "scientific" forecasting, while certainly not deprived of the opportunity to make a substantial positive and independent contribution to the advancement of Communism, must contend with both the competition and the threat posed by party history. Second, because the doctrine espoused by the party is so thoroughly futures-oriented, the party cannot avoid sanctioning the practice of forecasting, nor indeed would it want to, though forecasters are automatically put on notice that the sanction can be limited or expanded at the dictate of the regime. Let us examine these points a little more closely.

In the Soviet view, the party continues to exist because it has passed the test of real-world experience; survival certifies correctness, particularly the correctness of tenets expressed by the leadership in the earliest days of revolutionary activity. Thus, the very history of the party assumes a unique stature in helping to shape current decisions, plans, and outlooks. Indeed, the party tradition and the Marxist doctrine tend to come together at this point, the former being taken, in part, as the "objective" expression of the latter. While the tradition cannot be the sole influence on policy—after all, mistakes have been made, as each successive party chief has eventually said about his predecessor or his recently purged associates—its role is central. To some significant extent, it may even be formalized, as a number of Western analysts have tried to do.

One of the more ambitious attempts was made by Nathan Leites in a series of books concerned with identifying and organizing all of the inherited ideas about broad political strategy that serve to inform the "spirit" of the Soviet leadership and, thus, to determine many of its attitudes and much of its behavior. These guiding ideas—"political" in both a philosophical and an instrumental sense—could be found, in Leites's opinion, in the statements of the leadership itself, though not always explicitly expressed. In the aggregate, they constitute what Leites called the "operational code" of the Bolshevik elite.[19] Though this code has limited value as a device for predicting specific actions of the Soviet Union, Leites believed that it could be used in forecasting general patterns of development and the motives that may be at play.[20] In the present context, however, the code is worth reviewing for what it tells us about possible party beliefs regarding forecasting per se. Among the principal points in Leites's discussion of this question are the following four:[21]

1. There are no accidents. As Stalin put it in 1950, "History never does anything of moment without some particular necessity." Yet even on this basic issue, there is an apparent contradiction; in Leites's words, "According to one Bolshevik feeling, any given event is either inevitable or impossible; according to an opposed feeling, there are a variety of possibilities for any given situation . . . . The determinist tendency is apt to prevail with respect to the past in general; and to the future with regard to overall lines of development and the actions of others. The indeterminist tendency prevails with respect to details of the future and, particularly, to acts of the party itself." This would appear to be consistent with the point made earlier, that the clearer and more fundamental and long-lasting the outcome, the greater the necessity for the forecaster to adopt an exploratory approach.[22] In any case, he probably must be committed, in principle, to the determinist attitude.[23]

2. Control of the future, at least in the sense of not being surprised by it, is crucial. Leites' statement of this point is as follows: "The alternative to controlling is being controlled. Being controlled by the Party is intensely preferred; being controlled by 'history' is accepted; and being controlled by enemies dreaded." Forecasting can help provide a measure of control, but so too can an awareness of history and a knowledge of doctrine. Forecasters must have both, a fact that marks a fundamental difference between Soviet and Western forecasting.

3. All actions are "forced" by "objective conditions," even those of the party. But true freedom—which consists in acting in accord with an understanding of this necessity, rather than being overwhelmed by it[24]—is made possible by the party, which by leading in the development of doctrine is closest to such an understanding. Indeed, all those who do not accept the doctrine can only be "dragged along" by history. One implication of this view is that the party will accept a forecast about a favorable situation for an "enemy" only if it can be shown that the enemy's choices leading to this situation are forced upon him by "conditions." No less important is the implication that the party looks with greatest favor on forecasts about actions that might be taken by the regime which show, on the basis of full analysis, that there is only one possible choice. As Leites puts it, the elimination of alternatives permits the leadership to dispense with "the usual and perilous operation of ascertaining which is the only correct one and which will lead to disaster (for all will, except the only correct one)." Another major implication is that the party is very much opposed to the practice, attributed by the Bolsheviks to the non-party intelligentsia, of "inventing," which Leites describes as the practice of "deciding upon a course of action originating in one's thoughts rather than in the external situation." The thrust of this objection is against old-fashioned utopian socialism, but it no doubt has a current application for Soviet forecasters in limiting imaginative excursions into the realm of futures that might themselves be "invented." Certainly no such thinking has as yet appeared from within this group—a fact that should, of course, be taken merely as an indication that the weight of party tradition and the strictures of doctrine are willingly accepted by these forecasters.[25]

4. As we have already seen, there is much that is known and can be known about the future. At the same time, the party leadership has always maintained that some aspects of the future are not predictable. Earlier, for instance, we indicated that the precise date of the occurrence of a revolution falls in this class. But Leites and others marshal a good deal of evidence that has the effect of showing that this is a relatively trivial example, for also included are at least the following seven kinds of potentially very useful information: (a) the rate ("tempo") of progress or of development toward ultimate outcomes; (b) the specific paths that will lead to these outcomes; (c) the specific steps that will be taken on these paths; (d) the type, number, and duration of the setbacks and obstacles that will have to be overcome in taking these steps; (e) the degree of support (morale) and the character of that support (mood) among peasants and workers for the party program; (f)

the extent to which the party itself will seize upon the opportunities opened by "objective conditions," and (g) the extent to which the goals sought by the party (the "subjective" aspect of a political choice) will actually be attained (the "objective" aspect). If, given all of these unknowables, one senses a fundamental similarity between the difficulties facing forecasters in the USSR and in, say, the United States, he would be wrong. The similarity is only superficial, as the U.S. forecaster may readily determine for himself by substituting "client" for "party" and trying to explain why he faces difficulties comparable to these seven. His answer will surely differ from the one given by the tradition of the party and shared by the Soviet forecaster.

In short, it runs as follows: To grant these unknowables is not to renounce the belief that the world is fully intelligible; indeed, this remains a basic conviction. But in the search for this intelligibility, one must start somewhere. Nothing significant is given all at once. Specifically, a science like historical materialism is no more established in a twinkling than is a science like physics. At the beginning it may well be possible, however, to isolate basic concepts of lasting value (e.g., the atom, the class) and to define general laws governing the system in question. To get to the point where one knows not only the laws, but also what Stalin and others have called the "zigzags," requires, in the first place, the accumulation and analysis of data and the continued refinement of details of theory, *building always on what is already known.* As it was put in the 1930's:

> [Because this theory] is a science it does not and cannot stand still, but develops and perfects itself. Clearly, in its development it is bound to become enriched by new experience and new knowledge, and some of its propositions and conclusions are bound to change.[26]

In the eye of history, the needed theory has hardly begun to be developed, especially in the social sciences. The wonder, then, is that so much has already been discovered—not that so much is presently unpredictable.[27]

At the same time, however, it must be remembered that a development is produced by the "relation of forces" and that in cases like the seven mentioned, the actual relationships can often be discovered only in the final phases of the "struggle." In such circumstances, analysis can at best give an insight into the relevant probabilities.[28] Because action itself increases knowledge, forecasting is bound to be weak in unprecedented situations. More generally, forecasting cannot hope to succeed in dealing with ever more specific problems if the forecasters are not themselves participants in the "struggle." In a rather profound sense, if they are not part of the solution, they are part of the problem.

To the Westerner who sees a particularly telling contradiction between the admission that objective conditions are only partially predictable and the claim of the party that its line is "correct," the answer would appear to be that the two are compatible if the line is on roughly the same level of generality as the conclusions established beyond all doubt by the doctrine, or if the line can be deduced irrefutably—that is, to the satisfaction of the party—from these conclusions. Where these conditions do not obtain, there should be no line, because statesman, bureaucrat, and forecaster alike are back in the realm of "science." That is, the process of grasping the unpredictable is to be viewed as *experimental*—just as the whole effort of the Soviet Union itself used to be viewed. Recently, the Soviet historian Medvedev has reaffirmed this answer by recounting how Lenin "never tried to project some strictly defined line, the least deviation from which would be considered a 'left' or 'right' opportunistic deviation"; in this regard, Medvedev also quotes with approval H. G. Wells's comments after his meeting with Lenin in 1920:

> Lenin . . . whose frankness must at times leave his disciples breathless, has recently stripped off the last pretense that the Russian revolution is anything more than the inauguration of an age of limitless experimentation. "Those who are engaged in the formidable task of overcoming capitalism," he has recently written, "must be prepared to try method after method until they find one which answers their purpose best."[29]

Julian Huxley's observations following a visit to the USSR about a decade after Wells are especially worth recalling:

> Russian affairs . . . have a political aspect, and a patriotic aspect, and an aspect of fanatical devotion which may perhaps be called religious. But under one aspect, and that perhaps in the long run the most important, Russia remains a scientific experiment, and the only one ever yet carried out in such a field and on such a scale.
>
> If the visitor to Russia is to appreciate the meaning of this experiment, he must first of all have some appreciation of the meaning of experimentation in general. For a scientific experiment implies that you are putting a question to nature. [Hence, to assess what is being attempted in the

USSR, the visitor] ought to be judging, first by the direction in which events are moving instead of by their precise state at present; and secondly, by the scientific efficacy of the experiment—whether or not it has been properly planned so as to give a real answer to the question put to it, and not merely a jumble of confused half-answers, each capable of a dozen different interpretations.[30]

It may be argued, following Leites, that what the party permits, the party tends to require. If doctrine permits experimentation, then practice demands it. Unquestionably, the most important historical manifestation of this conviction is to be found in the Soviet emphasis on planning, because a plan, if it properly "puts its question," not only incorporates an inspiring vision of a future that can be attained on the way toward the ultimate future, but it also defines the conditions of "struggle" and establishes unambiguous measures of the planner's foresight, the administrator's realism, and the success of the "experiment." Accordingly, the rise of large-scale economic planning in the Soviet Union—a tendency that began early but was formalized with the series of Five-Year Plans inaugurated in 1928—was accompanied by a growth in forecasting.[31] And it is in precisely this context—but only in this context, with all the explicit and implicit constraints we have reviewed—that Soviet forecasting first received party sanction.[32]

Thus, while Soviet forecasters can and do track their genealogy back to Marx and Engels—who, in the words of Bestuzhev-Lada, "carried out an ideological struggle not only against utopianism but, first and foremost, against the agnostic approach to problems of the future common at the time among the leading bourgeois philosophers, who claimed that it was impossible to make scientific prognostications of social processes"[33]—the practice of forecasting did not become formalized or legitimized in Soviet decision-making until concrete social experimentation—i.e., planning—emerged in the 1920's. It is probably impossible now to recreate the feelings of shock, derision, or excitement that were expressed by outside witnesses to this astonishingly presumptuous and unprecedented kind of futures thinking. "To most bourgeois scientists," says Bestuzhev-Lada, "the very idea of such a possibility seemed fantastic."[34] Nevertheless, the work was begun, and the Soviets can rightly boast of being the first to approve the attempt (and institutionalize it) on a national scale.

Almost immediately, however, forecasters and planners ran into some of the questions and presuppositions hinted at earlier in this chapter. There were, for example, debates in the 1920's over such issues as whether a plan differs from a prediction. Because the Bolsheviks believe that socialist man is master of his own destiny—or can be, once he has come to understand the inner necessity of conditions—the upshot was that the two were judged to be very much alike, at least in principle. Scientific prediction of events and conditions, insofar as they could be foreseen, thus became intermixed in theory with socialist intention. But the actual situation prevailing then (and even today) required that a distinction be made, based on the fact that all men were not as yet "free." Their energies, therefore, needed to be channeled, and this was to be the main function of the plan. Prediction, being limited to specifying certainties about the future, had to take second place in "this phase" of social reconstruction. As it was put at the Second All-Union Congress of Workers from Planning Organizations in March 1927:

> If our plans were concerned only with foreseeing the objectively inevitable, irrespective of the will of the economic subjects, which is exactly like the conditions of spontaneous development in capitalist countries, then we indeed should have based everything upon scientific forecasts of the future. In that case, there would not have been any point to plans. It doesn't make sense to construct a plan for the rising and setting of the sun or for the development of the crisis of capitalism, since these things take place in their own time and without any plan. But under Soviet conditions, a plan is expedient and necessary in order to concentrate the collective will of the economic leaders and workers of the whole country upon one or another set of tasks . . . . We consider the focal point of every plan to be not prevision; but rather assignments and instructions.[35]

Among other issues confronted during this period were a number that have come back into prominence in the last 10 years, including the question of the extent to which forecasters should make comprehensive socio-economic projections rather than more narrow economic predictions alone, and the question of the time horizon that should be considered in making these projections. Since social forecasts seem to become more useful as the time frame increases, these issues are clearly interrelated, and, for doctrinal reasons already discussed, the expectation of Soviet forecasters was that the large view would be approved. But it was not, although some interesting work had been begun:

> . . . in the 1920's we not only were not lagging behind the West in the scale and—the main

thing—the level of social prediction, but, on the contrary, in many respects were even leading. But then, as is known, the theory of the "very next step" triumphed in our country. Anyone who attempted to look too far into the future was quite summarily returned to the present.[36]

This "next step" attitude—easily justified in the tradition—set the tone for all of the Soviet forecasting that was done up to the 1960's, and much of the work since then. While its immediate practical advantages were obvious, it also had the effect of retarding the development of perspectives and methodologies that have characterized the growth of the futures movement in the West. In this sense, as one Soviet writer has said, forecasting has come to the USSR today as a "belated echo from the U.S.A."[37]

Between the 1920's and the 1960's, especially during Stalin's 30-year rule, the practice of forecasting and the sanction accorded this work were in constant jeopardy, as were the planners and forecasters themselves. Whether or not one agrees with Medvedev's assessment that Stalin "never was a true Communist,"[38] there is no disputing how Stalin viewed himself. And the actions he took regarding planning and forecasting are not patently inconsistent with either the doctrine or the party tradition. For example, he continued the idea that plans should be framed not only in terms of what is likely to be accomplished (the "base line" projection), but also in terms of what could be accomplished, given maximum will and effort, plus favorable circumstances in areas outside of Soviet control. Like others, including Lenin, he also believed that the principal task of the party was to minimize the time required to achieve ultimate goals, and hence he believed that the optimistic projection was the one to be pursued.[39] And he certified the position that plans were directives toward approximate rather than absolutely necessary goals: "The Five-Year Plan, like all plans, is merely a plan, accepted as a preliminary approximation, which must be refined, changed, and perfected on the basis of experience."[40]

Indeed, it was against this background that forecasting, as opposed to prediction, first came to be considered as a decisionmaking tool, a means of choosing among alternative plans by evaluating their consequences.

As a matter of hard reality, the Stalinist era also crystallized another aspect of Soviet "scientific prognostication": the dangers of being wrong. In theory, these dangers were that errors could mean serious setbacks, perhaps even annihilation, for the USSR, which in turn could mean a major reversal for the cause of Communism in the world at large. From the forecaster's point of view, however, the dangers were quite different. They included the risks of being charged with "wrecking"; with no longer being a "real" Bolshevik (i.e., one who has special insight into the future); and with actually favoring whatever has been foreseen, however deleterious to the regime and no matter how objective the method used. No one in the Soviet Union could have failed to notice that all of these accusations were made against the persons eliminated in the infamous purge trials of 1936–1938.[41] There was also the still more insidious (and unpredictable) danger of somehow having committed an *unspecified* mistake. In this way three successive heads of Gosplan lost their lives—as did millions of others, of course.[42] It would be quite surprising if these risks, or the remembrance thereof, had not inspired certain indelible feelings of guardedness and conservatism among Soviet forecasters, as they most assuredly did among others.[43]

According to Jantsch, the first break with the "next step" policy, which had set the tone, the time horizon, and the scope of earlier work, occurred in the late 1950's when a State Economic Council was created, independent of Gosplan, to prepare a 20-year economic forecast for the 1961–1980 period.[44] This may be true, considering that findings from the council's report were in fact incorporated in the Program of the 22nd Party Congress in 1961, that the council itself was absorbed into Gosplan at the same time, and that Gosplan has since ordered 15-20-year forecasts from subordinate institutes. But the record of conferences, book and journal publications, and official pronouncements suggests that the notion of longer-term and wider-ranging forecasting did not begin to take hold until about the mid-1960's. Indeed, if this record can be taken as the measure, the rise of futures studies in the Soviet Union was for all practical purposes contemporaneous with that in the West.[45]

Writing in about 1967, Bestuzhev-Lada observes that "more than 20 large international symposia" on scientific forecasting have been held in "recent years"; while these are not listed, the earliest one he mentions dates from 1961, the next from December 1966, and then a handful from 1967, which seems to have been a key year of activity.[46] Other sources point to the major Prognostic Symposium held in February 1967 in Prague, organized by the Council of Mutual Economic Assistance (COMECON),[47] and to a meeting in December in Kiev on problems of forecasting.[48] In the same year, apparently, the Research Committee on Social Prognostication of the Soviet Sociological

Association organized the first of a series of "permanent seminars" on a large variety of substantive and conceptual issues pertinent to social forecasting.[49] And, of course, 1967 also saw the first World Future Research Conference, held in Oslo and attended by several Soviet experts on forecasting. A milestone may have been reached in May 1968 at the All-Union Conference on Improving Planning and Economic Management. Among the conclusions from this meeting was a "stricture that . . . five-year plans be worked out within the framework of a system of long-range plans."[50] Subsequent conferences have served to develop themes from the 1966-1968 period.

Much of the same pattern is revealed in the chronology of publications. Thus, while the proceedings of the 1961 conference mentioned above were published, and popular books on forecasting began to appear thereafter,[51] it was not until the mid-1960's that serious studies going beyond the realm of strictly economic forecasting started to appear in larger than usual numbers. Among them were survey volumes,[52] attempts to bring forecasting into the management of science,[53] and several cookbooks or reviews of forecasting techniques.[54] Works such as these seem to have helped spark, in turn, a greater public interest in forecasting, so that popular scientific, literary, and other kinds of magazines began to report on the results of studies and to devote regular columns to "various concrete aspects of the future of the Earth and mankind."[55] They also may have helped to create an interest in the subject within wider academic and research circles. Whereas Bestuzhev-Lada could complain in 1967 that "the candidates of science who have defended dissertations on the problems of scientific foresight can be counted on the fingers of one hand,"[56] the situation began to change fairly quickly. In 1971, for example, the field of economic forecasting alone accounted for a dozen dissertations on subjects that included flows of public transportation in cities, demand in tourism, migration from rural to urban areas, consumer demand, and the role of forecasting in research and development management in U.S. corporations.[57]

Dramatic evidence of the growth of interest in forecasting is provided by plotting the publication date of references cited in a work like Bestuzhev-Lada's *Window into the Future,* as is done in Figure 1. The graph on the left presents data on Bestuzhev-Lada's footnote references in this book; the graph on the right does the same for the items in his bibliography. (Not all of the same titles appear in the two.) Even allowing for the fact that authors usually try to include references to the most current work, it seems clear from these plots that Soviet (and non-Soviet) publications on forecasting first began to appear in increasingly large numbers in the 1964-1966 period.[58] This view is corroborated to some extent by an analysis of bibliographic citations in the Czech study, *Civilization at the Crossroads,* as is shown in Figure 2.[59]

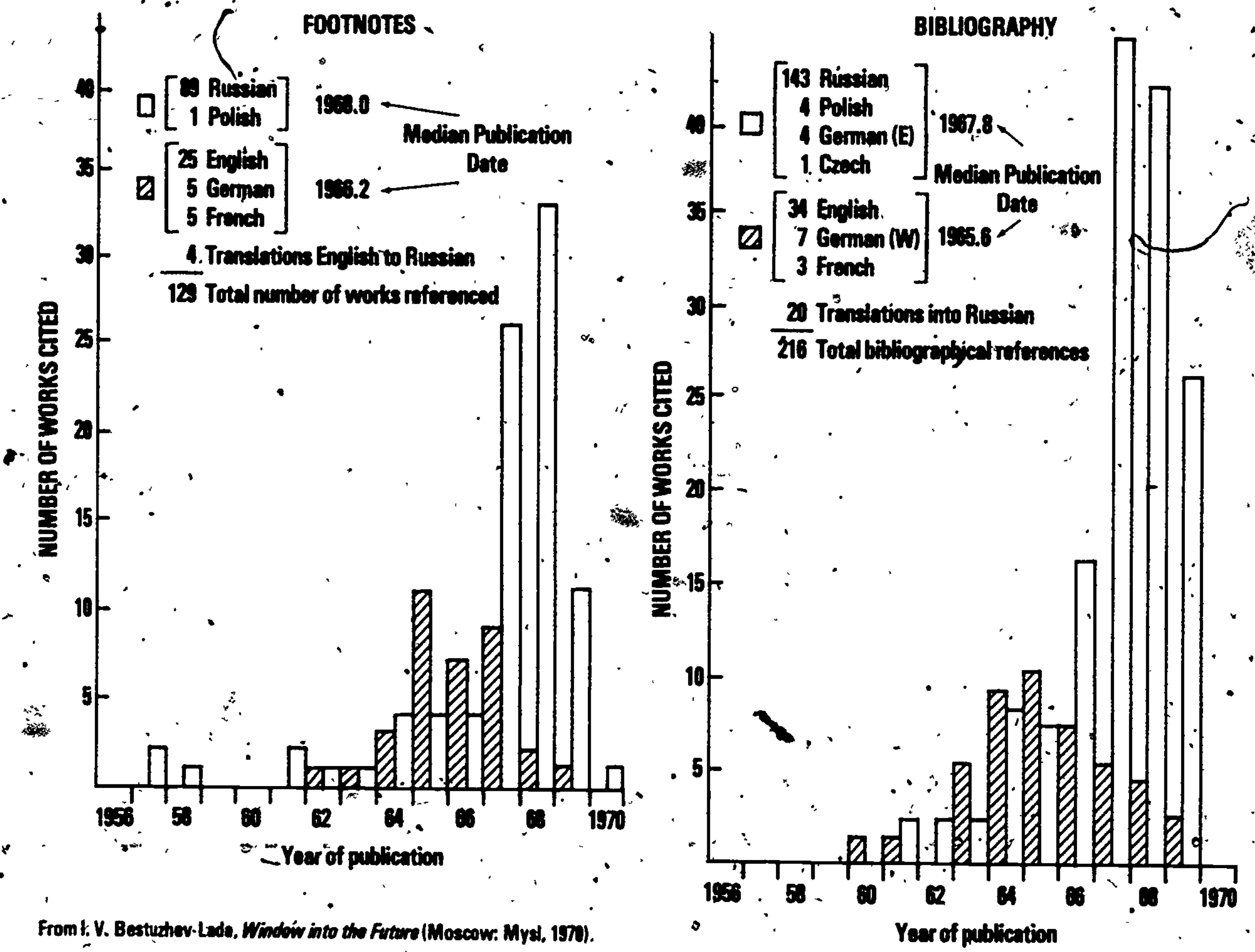
Neither conferences nor publications would, however, be possible without assertions by the top leadership declaring a need or support for forecasting. And it would seem that such statements began to appear in earnest in late 1964 through early 1965. According to Schroeder:

> The current forecasting craze . . . apparently had its genesis in a speech that Premier Kosygin made to Gosplan officials in 1965. Stressing the importance of scientific and technical progress, he said, "Can we, in projections of the national economy, ignore substantiated forecasts relating to the future? No, we *cannot.*" Forecasting was institutionalized by providing as part of the Eighth Five-Year Plan a "State Plan for Highly Important Scientific Research ' that included a comprehensive plan for working out socio-economic forecasts and forecasts of technological developments for 1971-75 and beyond.[60]

In the same 1965 speech, Kosygin also said, "The discussion of scientific prognoses must precede the working out of plans of development of national economic branches . . . . We must have at our disposal scientific forecasts for the development of each branch of industry, so as to give, in due time, the green light to everything advanced and progressive, and we must know the direction in which the plan should be worked out."[61] This cautious but encouraging statement was followed by others that also tended to give Soviet forecasters the sanction necessary to look at more subjects over a longer time horizon, though still only within the context of national economic planning.

Of special importance was the 23rd Party Congress (1966), which "formulated the question of the need for substantially raising the scientific level and broadening the range of socio-economic planning."[62] As a direct result of guidelines and programs enunciated at this congress, both theoretical and applied research on forecasting were "remarkably expanded," with dozens of groups coming into existence in Moscow alone for this purpose.[63]

In October 1968 the Party Central Committee and the USSR Council of Ministers issued a decree requiring, in particular, that Gosplan, the State Committee for Science and Technology, and other agencies



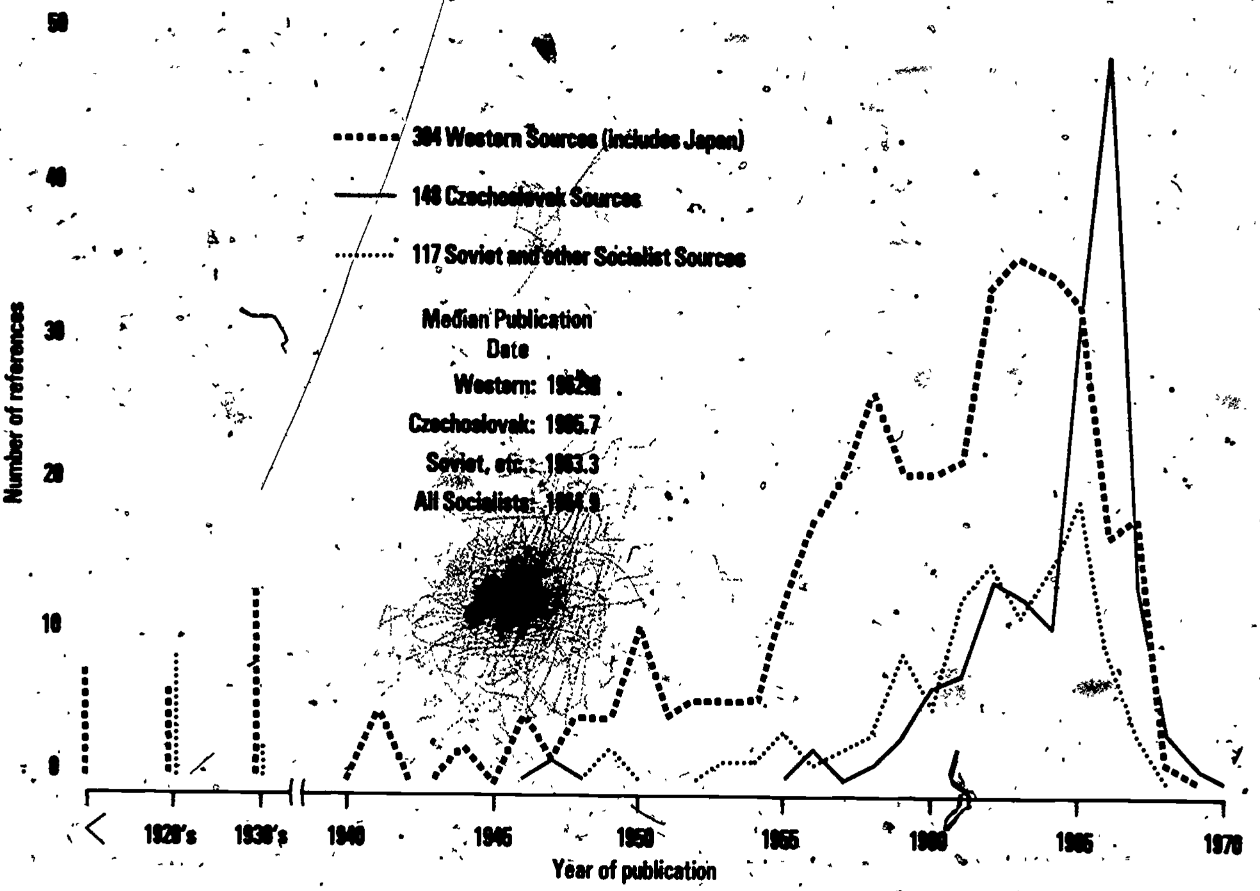
From I. V. Bestuzhev-Lada, *Window into the Future* (Moscow: Mysl, 1970).

Figure 1. Distribution of bibliographic references in Bestuzhev-Lada's study, by date and area of publication.

"henceforward" prepare "long-term forecasts (for 10, 15, or more years) . . . for the most important problems in the development of the national economy."[64] The scope of this decree is larger than it might seem at first glance, since, in the words of its title, it was aimed at "increasing the efficiency of the work of scientific organizations and accelerating the use of scientific and technological achievements." As such, its significance may lie in bringing together and approving many different strands in earlier forecasting work, including the development of methods appropriate to the newer tasks.[65]

With the 24th Party Congress in 1971, at which both Kosygin and Brezhnev emphasized the need for long-range plans, the party sanction of forecasting was apparently extended to the widest bounds. The final report declared that long-range planning of the national economy requires "forecasts of population growth, economic needs, and scientific-technical progress,"[66] and it stressed that improved planning must necessarily rely on "a more precise study of social needs, scientific forecasts of our economic potential, and a complete analysis and evaluation of different options as well as their immediate and long-range consequences."[67]

At least as interesting as the expanded charter of Soviet forecasting are the reasons why this expansion has occurred. Bestuzhev-Lada provides a safe, conventional explanation. In general, he says, the growth in the number and scope of futures studies in the USSR (and elsewhere in the world) can be attributed to the recent availability of better forecasting methods; to the emergence of many new nations, all seeking rapid economic advancement; to the threat of nuclear warfare;



R. Richta, *Civilization at the Crossroads* (Prague: International Arts & Sciences Press; 1969).

Figure 2. Distribution of bibliographic references in Richta's study, by date and area of publication.

to the growth in technology; to the global population explosion and the problem of providing enough food; to the accelerated pace of social change; and to the continued rise of "the world socialist system, which pushed into the foreground the question of the trends and prospects of the contest between the two world social systems." Specifically, however, the growth of forecasting in the USSR has been prompted by practical concerns in the doctrine and in the tradition:

> The successful solution of the complex tasks of socialist and communist construction in the Soviet Union and the fraternal countries, of the economic, scientific and technological competition with the world capitalist system, and of the struggle against the aggressive policies of imperialism, calls for the improvement of scientific Marxist-Leninist prognostication of the long-range prospects of development of the modern world. The effectiveness of the long-range economic programs and the political strategy of the socialist countries depends on how complex [i.e., comprehensive] and reliable these forecasts can be.[68]

We see here both an international and a domestic concern, each with an economic and a political dimension. But why did these concerns manifest themselves just when they did—that is, by 1965 at the latest? The answer is not given in the Soviet forecasting literature; however, it can be pieced together from other sources, which seem to make the rise of forecasting an almost inevitable consequence of, and reaction to, the "adventurism" and "harebrained schemes" of Khrushchev. On the international side, there appears to have been genuine alarm, especially among the military, that what might be called Khrushchev's "countdown diplomacy" regarding such areas as Berlin and Cuba was based on ignorance of the actual nature of Soviet capabilities and, hence, the actual possibilities open to the USSR for current and future maneuver. Matters so important, including the growing schism between

China and the Soviet Union, could not be left to the whim and the bluff of a single person.[69] Prudent action requires good plans, and good plans require good forecasts of emerging situations, intentions, and physical capabilities.

The same is true, but with a vengence, on the domestic side, where the perennial and central economic problem of the USSR has been the conflict between centralized planning and local autonomy in carrying out the plans. This problem has been described deftly by Alex Nove:

> While centralized planning overburdens the organs charged with carrying [the plan] out, decentralization—the obvious remedy—proves completely unworkable so long as planners' instructions are the principal criteria for local decisions. The modest attempt to dissolve authority to territorial economic organs, in 1957, was inevitably followed by renewed centralization. Within the system as [it] is, only the center is in a position to know the needs of industry and society at large, since these are not transmitted by any economic mechanism to any territorial authority. The latter is therefore unable to foresee the effects of its decisions on the economy of other areas, and, in the circumstances, decentralized decisionmaking must lead to intolerable irrationalities . . . Thus decentralization is both indispensable and impossible.[70]

Much more serious than the failure of the 1957 experiment was Khrushchev's apparent doctrinal heresy in 1963-1964 of permitting the so-called Liberman reforms, which seemed to open the door a crack to the profit motive as the needed "economic mechanism." Though only 150 industries were involved, the Liberman experiment drew worldwide attention, prompting surprise in the West and charges of "revisionism" from China. What was not given much attention was the fact that at the same moment the traditional system of centralized, short-term, and strictly economic planning in the USSR was on the verge of collapse. V. M. Glushkov warned in September 1962 that the economy might not survive unless the then current methods of planning were not totally changed. Indeed, his estimate was that if the planning bureaucracy continued to insist on having perfect foreknowledge of all the factors that should be taken into account in the plans, the number of planners would have to increase 36 times by 1980, at which point the entire population would be included. Nor would computers help, since the number of relationships to be considered was so large that, again according to Glushkov, a million computers operating around the clock at 30,000 operations per second would require several years to work out a plan for the total economy.[71]

To complicate matters even further, it appears that Khrushchev had lost the support of the people for his economic plans. The fabled Penkovskiy notes that "everyone laughs at him, especially at his slogan: 'Let us catch up with and surpass America.' The people are smart, and they immediately responded with a joke:

> In production of milk,
> We have overtaken America,
> But in meat we have failed:
> The bull's penis got broken."[72]

Against this background, Khrushchev's removal was almost guaranteed, as was his replacement by a leadership that was resolutely professional, orthodox, and faceless. It may also have been foreseeable that the new leadership would, among other things, sanction newer, more systematic, and more comprehensive approaches to planning and forecasting, since the improvisations and piecemeal efforts of the Khrushchev years had failed to survive the "struggle." In any event, today's interest in futures research in the USSR can almost certainly be dated from the fall of Khrushchev in 1964. Indeed, on October 17, 1964, the very day after Khrushchev was turned out of office, *Pravda* fired one of the opening shots in a virulent campaign against his leadership by referring to his failures in dealing properly with the future: "The Party of Lenin is an enemy of subjectivism and inertia in communist construction. Wild planning, premature conclusions, hasty unrealistic decisions and actions, boasting and blather, overpreoccupation with administration, unwillingness to take account of the fruits of science and practical experience . . . are alien to it." And it was during the initial work on the first major post-Khrushchev initiative, the 1965 economic reform proposals, that the new approaches found their initial expression:

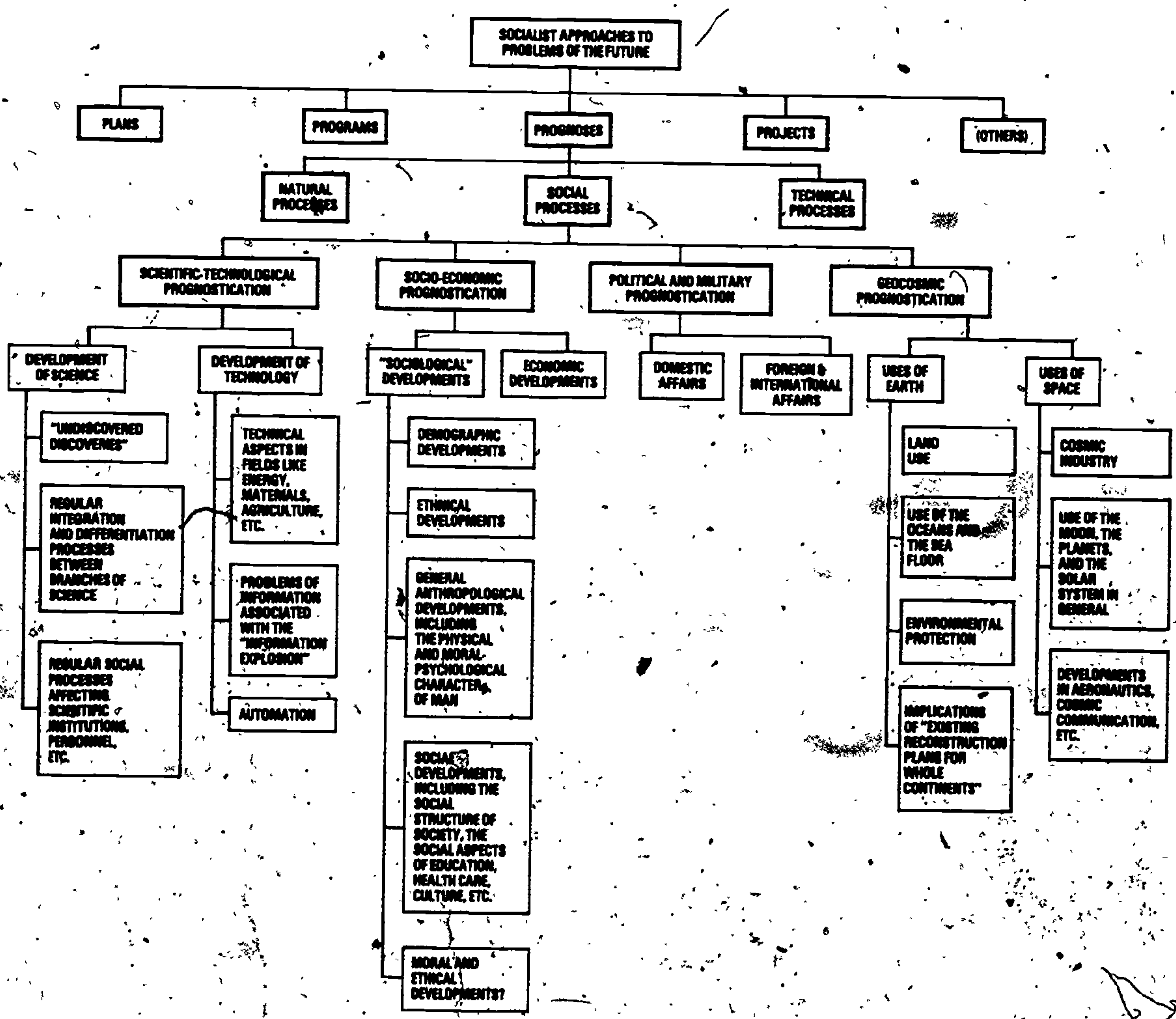
> Soviet planners became convinced that the key to improved economic performance lay in developing much more "scientific" bases and methodologies for centralized planning. The call for more "scientifically-based" plans involved two major ideas. First, long-term forecasts of scientific and technological developments should be made in some detail, and second, all parts of the plan should be based on projections (forecasts) of economic and social variables made with the use of modern mathematical and economic models. The

planners perceived that the Soviet economy was not participating in the on-going, world technological revolution and evidently believed that if accurate forecasts of technology were made, the plans could take them into account, and the USSR's track record in this area would be improved.[73]

Approaches

What are these approaches, and what methods of forecasting follow in their wake? Standard answers to these questions have not been developed yet in the USSR, but there is enough overlapping among the various discussions to permit an attempt at classification. Thus, insofar as approaches are concerned, three distinct but interlocking perspectives can be recognized.

First, there is what we might call the *substantive view,* in which the scope and the problem of forecasting are structured in terms of the kinds of subjects to be forecasted. This straightforward approach is illustrated in Bestuzhev-Lada's "Social Prognostics Research," written in about 1967 (see Figure 3). In this



Derived from I. V. Bestuzhev-Lada, "Social Prognostics Research in the Soviet Union," in Robert Jungk and Johan Galtung (eds.) *Mankind 2000* (London: Allen and Unwin, 1969), pp. 300-302.

Figure 3. Bestuzhev-Lada's classification of the types of prognostication.

scheme, forecasting can be concerned with any of three fundamental types of processes: natural (e.g., earthquakes; eclipses), technical (e.g., the speed of computers, fuel requirements for aircraft), or social (e.g., the interactions of man and man, man and society). The first category under social prognoses, "scientific-technological prognostication" has come into such prominence since the mid-1960's that it will be considered at length below.[74] The entries in this category, however, should be self-explanatory, except for "undiscovered discoveries," which is simply a shorthand for the forecasting of future developments in areas of science that are developing rapidly. (For example, given the current level of work on cancer, one might safely forecast that a cure will be discovered, though its nature and the date of its discovery cannot be known precisely.) Generally speaking, the subcategories under each of the four principal types of social forecasting are to be taken merely as examples; hence, the lack of entries for "economic developments," "domestic affairs," and "foreign and international affairs" is not significant.[75]

An interesting indication of the relative importance of these categories in Soviet research is provided by sorting the bibliographic entries in Bestuzhev-Lada's 1970 *Window into the Future* under the four main headings. On this analysis, "scientific-technological" prognostication accounts for about 25 percent of the titles; "socio-economic," for about 50 percent; "political and military," for 10 percent; and "geocosmic," for 15 percent.

The second perspective can be called the *presuppositional view;* it divides the forecasting enterprise in terms of the attitudes or expectations or motives that underlie the forecasts that are made. Bestuzhev-Lada recognizes eleven such kinds of presuppositions that govern "approaches to the problems of the future of the world and of mankind"; these he groups into three general categories: negative, intermediate, and positive.

1. *Negative Categories*

Presentistic approach: holds that both the past and the future are essentially the same as the present.

Agnostic approach: allows, in principle, for the possibility of a different state of affairs in the future but denies the possibility of knowing anything definite about the future.

Nihilistic (finalistic) approach: reflects the conviction that there is no future—that the end of the world is at hand.

2. *Intermediate Categories*

Religious approach: holds that the future is determined by supernatural forces (divine providence), that it can be foreseen by fortune tellers and prophets, and that it can be influenced by prayer and magical means.

Fantasy-oriented approach: involves arbitrary, primarily artistic representation of the future, largely involving supernatural forces, derived from folk tales and also original fantasies (but excludes science fiction).

Utopianistic approach: involves arbitrary representation of a desired future (not necessarily based on a belief in divine providence), which subjectively seems to be scientific (based on knowledge, not faith), but which objectively turns out not to be based on a scientific understanding of the laws of natural and social development. (Similarly, there is an anti-utopian approach which presents a picture of an undesirable future.)

3. *Positive Categories*

Intuitive approach: involves foreseeing or anticipating the future on the basis of intuition—one's subconscious experience allows him to make more or less accurate conjectures.

Philosophical approach: involves predicting the future as a result of natural and social development within the framework of what is known about the laws of development from the simple to the complicated. Other philosophical concepts turn out to be idealistic or vulgar-materialistic (e.g., the concept of regression from a golden age, the concept of the cyclic nature of history, and other metaphysical concepts of non-dialectical progress).

Prognostic approach: involves systematic scientific research into the prospective development of one or another phenomenon or of progress on the basis of known concrete laws of natural and social development—concretization of the philosophical approach.

Constructive approaches: involves active efforts to shape the future through *planning, programming,* making *projects,* and finally *managing and controlling* processes.

Science fiction approach: involves artistic interpretation of the future based on data acquired by means of other positive approaches.[76]

It should be noted that the "prognostic" and the "constructive" approaches (as well as the Marxist mode of the "philosophical" approach) provide the only scientific link with the *substantive* classification scheme illustrated in Fig. 3. It should also be noted that because these approaches are characterized in terms that have special meaning in the jargon of Communism, the forecasting efforts of non-Communists are thereby implicitly but automatically relegated to one or more of the remaining eight or nine categories.[77]

The third perspective on forecasting can be called the *procedural view*. Whereas the substantive view indicates the *objects* of the forecasts and the presuppositional view indicates the *beliefs* behind the very act of forecasting, this third way of thinking about forecasting appears to be based on a recognition that verbally similar forecasts can arise in different contexts and serve different ends. Accordingly, it divides the business of forecasting into categories reflecting the general research strategies which are available to the forecaster, *qua* forecaster, and which will determine not only the choice of specific methods to be used (singly or in combination), but also the place and function of the results in the much larger context of *all* efforts to understand and control the future. Important among these strategies are the approaches associated with particular disciplines, notably economics, though it is difficult to find a Soviet forecaster who considers himself a contributor to the futures movement and who, simultaneously, declares that he is merely practicing economics or sociology or anthropology. Almost always he identifies himself instead with one of the cross-disciplinary research strategies that have achieved some prominence in the USSR. In this, of course, he is very much like his U.S. counterpart.[78]

Five such strategies—all but the first of them ultimately derived, apparently, from the Western literature, but with a predictable twist—have been frequently discussed by Soviet officials and researchers:

1. *Prognostics.* This hybrid is as close as the Communists come to an equivalent for "futures research"; indeed, Western authors sometimes mistakenly interpret the terms as synonymous. But as the name of a *procedural* approach, "prognostics" is customarily defined by the Soviets as the science of forecasting in accordance with the doctrine (specifically, in accordance with Marxism-Leninism), and then only insofar as the resulting forecasts are needed in order to formulate a plan, a program, or an actual project. Under the prognostics approach, then, it can even be said that a subject is investigated only to provide data and insights necessary as inputs to the remaining four strategies.[79] In any case, the now well-established opinion, which has grown up over the last ten years in the USSR, is that prognostics, in this sense, is indispensable in planning, as was suggested earlier in this chapter.

2. *Operations research.* This approach is defined by Soviet specialists in many ways, but it might be characterized generally as the discipline or science of analyzing the relative short- and long-term implications of alternative plans, programs, or projects.[80] More rigorously, it can be viewed as a wholly quantitative, cross-disciplinary approach in which carefully defined problems of the future are analyzed according to these steps: establishing a particular operational goal (e.g., maximization of output for a fixed cost, or minimization of cost for a fixed level of output); specifying key parameters and constraints; developing a model; and then running the model in order to rank-order alternative policy variants against the operational goal.[81] No doubt these steps will sound familiar to Western analysts; the novelty of the OR approach in the USSR is that its ultimate objective is "to develop decisionmaking algorithms for *any* situation."[82]

3. *Decision theory.* In this context, decision theory is an approach that embraces analytic efforts to model and forecast the decisionmaking of management in attempting to evaluate or carry out plans, programs, or projects. Decision theory has a formal and an informal part. The formal part is concerned with problems or portions of problems that can be quantified and handled by computer; examples include "control of technological processes, scientific approach to management, etc." The informal part is concerned with "problems where the preferred possibility is less apparent, and the final judgment in making the decision is left to man. At present, the most advanced decisionmaking systems are designed as joint man-machine systems; however, a goal for the future is to formalize the entire system of decisionmaking and to develop mathematical methods to such an extent that it will be possible to apply computers to the solution of higher-level problems which now can be solved only by man."[83] Even with reference to its ultimate aspirations, however, decision theory would appear to hold minor interest for today's forecasters in the USSR; the approach seems to be implicit in much of the discussion of national planning models and national data banks, but even there it does not receive great emphasis.

These first three approaches have been viewed as parts of a single process,[84] as shown in the diagram on the next page.

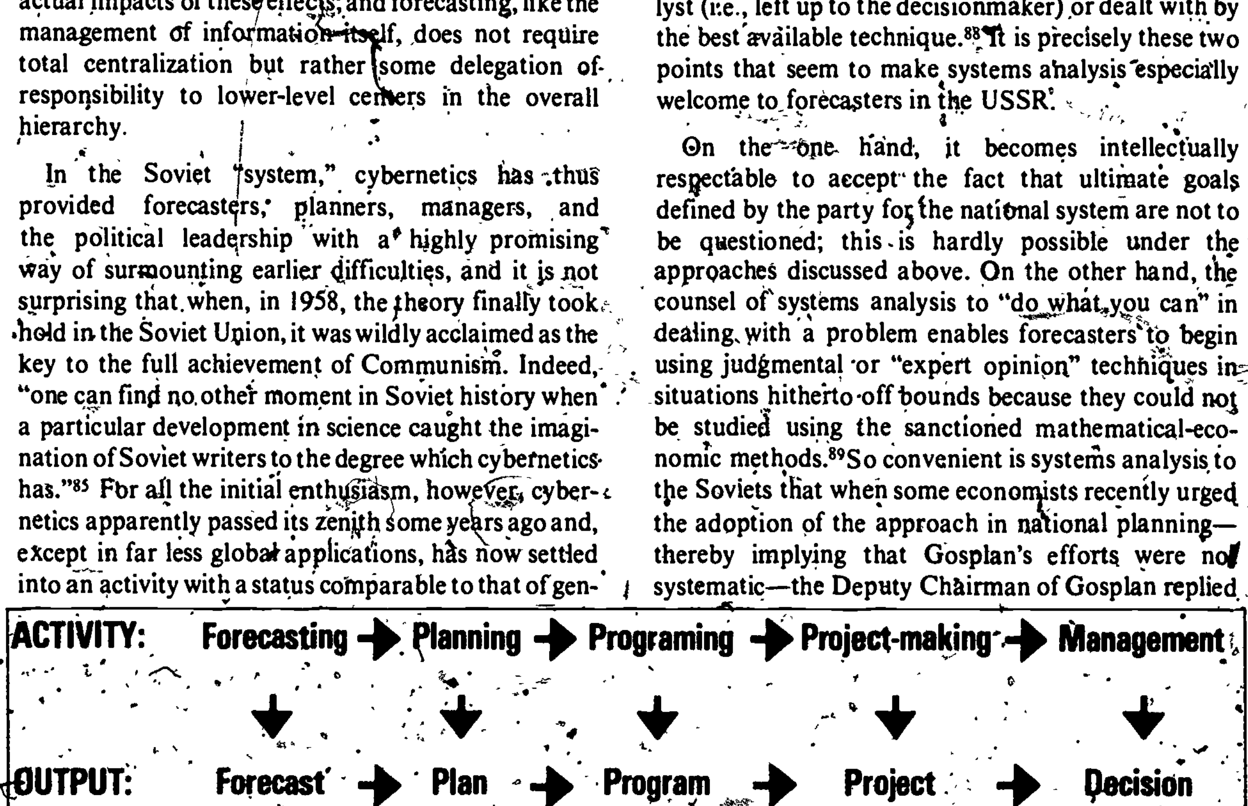
4. *Cybernetics.* As a discipline or science in the

USSR, cybernetics is a marvelously variegated activity that draws upon the skills of mathematicians, philosophers, economists, biologists, physicists, psychologists, and others, including what we call information theorists and computer scientists, all of whom are concerned with the study of how information is or can be used to regulate systems and to delay as long as possible their inevitable decline into total disorder. (The classic example of a cybernetic system is a home heating system under control of a thermostat. But much more to the point in the Soviet case is the national economy, where rational control is presumably to be achieved by centralized monitoring of deviations from a centrally prepared plan.) As an approach to forecasting, cybernetics would appear to offer several advantages which other approaches do not: future states or conditions of the system being studied must, in the theory, be described analogically or probabilistically, rather than exactly, thus obviating the need for perfect foreknowledge. Similarly, there is no need to attempt to anticipate all of the second- and higher-order effects of particular actions, since this can better be accomplished through continuous feedback on the actual impacts of these effects; and forecasting, like the management of information itself, does not require total centralization but rather some delegation of responsibility to lower-level centers in the overall hierarchy.

In the Soviet "system," cybernetics has thus provided forecasters, planners, managers, and the political leadership with a highly promising way of surmounting earlier difficulties, and it is not surprising that when, in 1958, the theory finally took hold in the Soviet Union, it was wildly acclaimed as the key to the full achievement of Communism. Indeed, "one can find no other moment in Soviet history when a particular development in science caught the imagination of Soviet writers to the degree which cybernetics has."[85] For all the initial enthusiasm, however, cybernetics apparently passed its zenith some years ago and, except in far less global applications, has now settled into an activity with a status comparable to that of general semantics or general systems theory in the West. The reasons—theoretical, ideological, and practical—are not as significant as the simple observation that no important Soviet writer currently holds that forecasting is best pursued from the cybernetics approach. In fact, while every Soviet forecaster has undoubtedly been imbued with the concepts and principles of cybernetics, not one of the authors cited in the bibliography for this chapter gives any prominence to this approach; almost none of them even uses the word.[86]

5. *Systems analysis.* The systems-analytic approach, in the Soviet Union and elsewhere, tends to draw together the main threads from the other approaches, while avoiding their weaknesses. As such, it is probably the most important research strategy for forecasting, though only in the last few years, after much confusion about what the phrase referred to, has the phrase and the approach itself begun to come into fashion.[87] (It has still not achieved the currency and reputation that has attended cybernetics.) But it is important to note that difficulties are avoided under systems analysis because, typically, they are either ignored by the analyst (i.e., left up to the decisionmaker) or dealt with by the best available technique.[88] It is precisely these two points that seem to make systems analysis especially welcome to forecasters in the USSR.

On the one hand, it becomes intellectually respectable to accept the fact that ultimate goals defined by the party for the national system are not to be questioned; this is hardly possible under the approaches discussed above. On the other hand, the counsel of systems analysis to "do what you can" in dealing with a problem enables forecasters to begin using judgmental or "expert opinion" techniques in situations hitherto off bounds because they could not be studied using the sanctioned mathematical-economic methods.[89] So convenient is systems analysis to the Soviets that when some economists recently urged the adoption of the approach in national planning—thereby implying that Gosplan's efforts were not systematic—the Deputy Chairman of Gosplan replied



in a long article that "the USSR has now, and always has had, a 'systems' and 'program' approach to planning."[90] It is particularly in the development of what the Soviets call "a system of optimal economic functioning" (SOEF) that systems analysis has achieved greatest favor:

> One of the most important problems in the creation of SOEF is to promote the systems principle of planning at all levels and in all elements of the national economy. [To date,] considerable progress [toward this goal has been scored] in the elaboration of the methodology of macroeconomic forecasting and the methodology of constructing dynamic interbranch and interregional balances and of making demographic and labor forecasts, as well as forecasts of the population's living standards . . . . In our view, the basic direction for implementing the systems principle in planning lies in [developing models that] can be used to analyze the functions of the management at all levels in the economic hierarchy and to synthesize the optimal interaction among them within the framework of a single theoretical approach. [Through this work, plus experimentation, will come SOEF, which] envisages the coordination of a complex of long-range forecasts, a "tree of goals" in national development, and comprehensive long-range programs for the attainment of the most important goals with the single national plan for the development of the country's national economy.[91]

Behind this soporific prose are several truly significant messages: SOEF is of the very first importance; SOEF cannot be attained without a *full* range of forecasts; and these forecasts cannot be prepared scientifically or with any guarantee of completeness unless the work is accomplished within systems analysis.

This discussion of approaches gives us, then, what is a three-dimensional array of perspectives that can be adopted by the Soviet forecaster, as indicated in Figure 4. This figure suggests not only that the Soviet forecaster can find a proper strategy for any particular problem he confronts, but also that he can immediately classify any non-Communist forecast and thereby evaluate it. What the figure does not show, but we have considered in these pages, is that the relative merits of the acceptable approaches have shifted and continue to shift. Also missing is another dimension: time. It is perhaps sufficient to remark that while the Soviets recognize different time horizons for forecasts—short term (1–5 years), medium term (5–20 years), or long-term [illegible] more years)—they have little to say about how

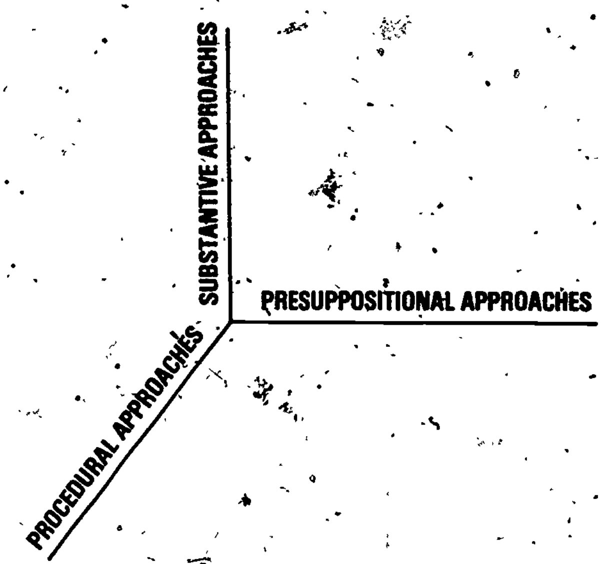


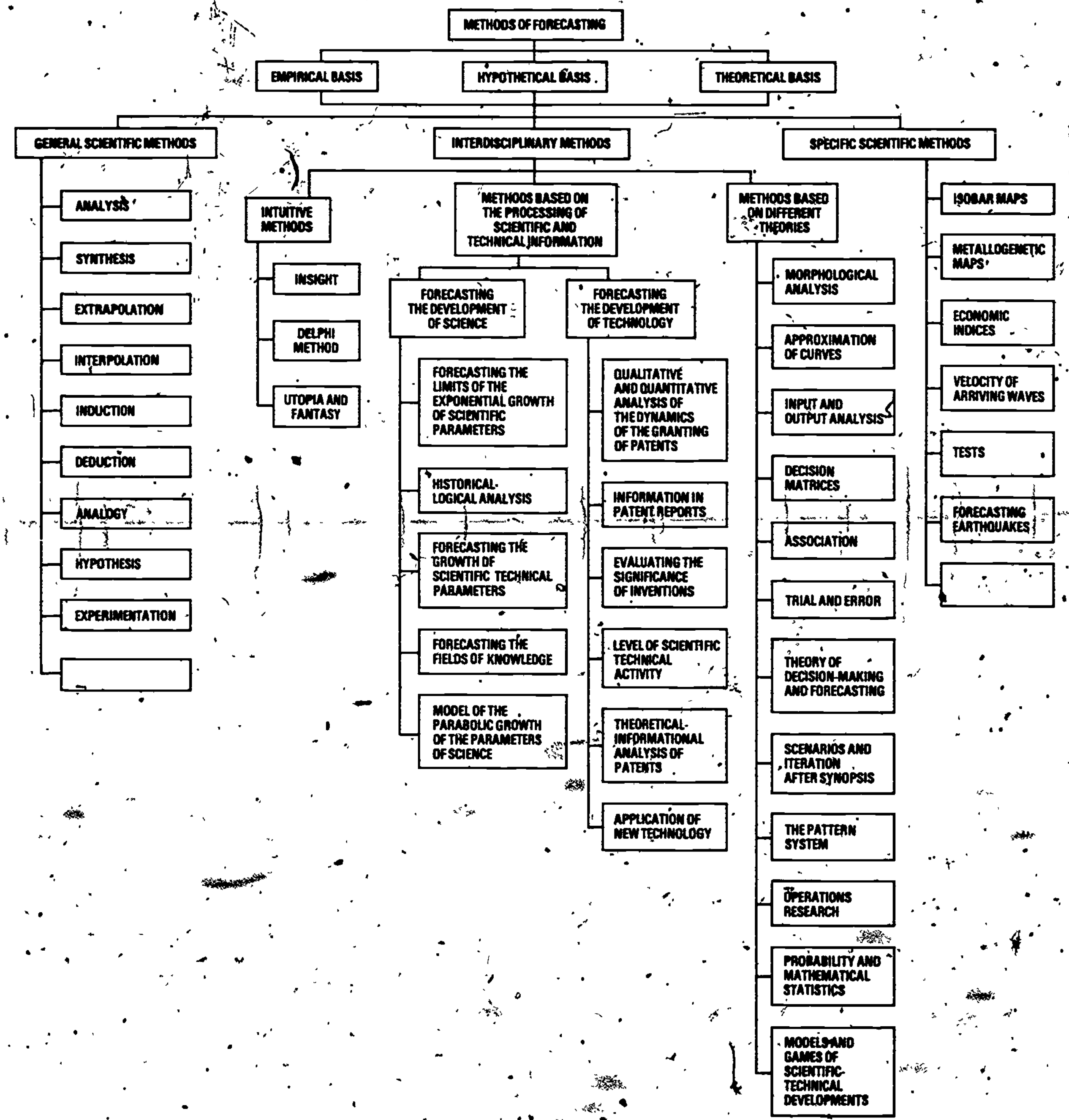
Figure 4: Soviet perspectives on forecasting.

these horizons should affect the choice of research strategy or the location of a problem in its proper strategy space.

Methods and Applications

Like futures researchers in the West, Soviet specialists in forecasting have sometimes found their progress blocked by a "prevailing chaos in published forecasting methodologies and in . . . terminology."[92] As early as 1967, Bestuzhev-Lada could seriously say that "about three hundred special methodologies" had been devised for social prognostication alone,[93] and a year later Glushkov and Dobrov could assert that 100 methods existed just for use in forecasting the course of the scientific-technological revolution.[94] Given this paralyzing abundance of techniques—and the numbers should probably be taken as real, even though no one has ever actually tried to list all of the items—it is not surprising that the Soviets prefer to speak in terms of *types* of techniques.

Out of several competing taxonomies that have been published, we might look briefly at the one devised by Bestuzhev-Lada.[95] This scheme, shown in Figure 5, is noteworthy in several respects: it attempts to be complete, but it explicitly allows for new types (by leaving several empty boxes);[96] it makes several unexplained and apparently quite arbitrary assumptions (e.g., that "analogy" is a "general scientific method" and that "trial and error" is a theoretically grounded "interdisciplinary method," but that neither should be consid-



Quoted from I. V. Bestuzhev-Lada, *Window into the Future* (Moscow: Mysl, 1970), pp. 66ff.

Figure 5. Bestuzhev-Lada's classification of forecasting methods.

ered as an "intuitive method"); and it confuses techniques with approaches (e.g., "operations research," one of the *procedural* approaches, and "forecasting the fields of knowledge," one of the *substantive* approaches, are shown as methods). Equally significant, however, is that this classification generously provides a place for almost every technique that has achieved prominence in the forecasting literature, East and West. Thus, Delphi, PATTERN, scenario writing, and morphological analysis are not only given separate mention, but are placed on the same level as such a well-established technique as input-output analysis—and this despite the fact that none of these methods has been used extensively in the Soviet Union. Their inclusion may simply be a signal of awareness; more important, however, it may also be a sign that by 1970 the desire and the opportunity to experiment with rather exotic Western techniques had a fairly widespread endorsement, thanks principally to the party's insistence on having better, more comprehensive, and longer-range forecasts.[97]

The question of superabundant alternative methodologies can be radically simplified by examining actual practice over the last 10 years in the USSR. On this basis it is clear that the kinds of techniques in formal and general use are few, though a bit more than there were before the Central Committee granted Khrushchev's "request to be relieved of his duties." Far and away the most deeply imbedded and widely used are the mathematical-economic methods (including input-output analysis, linear programing techniques, and simulation models); the supremacy of these methods was not attained without much controversy, but it is now complete, at least in terms of expended effort. Much further down the scale are the following types: extrapolation (mathematical and, where unavoidable, subjective); "expert opinion" or "expert evaluation"; "informational analysis" (including especially the study of patents); "historical-logical analysis"; and various "charting techniques" (including relevance trees). All of these methods are used in both short-term and longer-range forecasting; for our purposes, however, only the latter applications need be discussed, since it is here that the problems of comprehensive forecasting are thrown into greatest relief. To be as brief and yet as tangible as possible, let us consider only the mathematical-economic and the expert opinion techniques.

Mathematical-Economic Forecasting

The nature, place, and role of these techniques in the Soviet scheme of things cannot be understood apart from work that has been underway since 1964 to implement a planning system which is, in its breadth and daring, every bit as awe-inspiring to outsiders as was Lenin's GOELRO Plan for the electrification of the USSR. (Like GOELRO, it has also been derided as "grandiose," "visionary," and "utopian.") As originally proposed by V. Nemchinov and developed in its first specifics by a group of analysts that included Glushkov and Fedorenko, this system envisioned the following:[98]

1. A total restructuring of the national information system for economic and other kinds of planning data.
2. Creation of a unified state network of computer centers.
3. Routinization and algorithmization of day-to-day processes of administration and control.
4. Creation of a dynamic national economic optimizing model for continuous planning. (This is to be the heart of the system.)
5. Development of the mathematical methods required to solve specific problems regarding the general model.
6. Development of a concrete administrative and planning apparatus, including institutions and behavior rules, consistent with the general model.

Efforts toward these goals have been halting, because of bureaucratic in-fighting; the inescapable need to overcome ideological problems (e.g., how subversive are non-Communist modeling techniques?); basic theoretical and computational questions (e.g., what criteria of optimality are to be used?); and procedural bottlenecks (e.g., the allocation of resources to the program, the training of personnel, the availability of hardware, and the debugging of software).[99] Nevertheless, research has proceeded and, indeed, has been expanded. The SOEF scheme mentioned earlier appeared in about 1966 as a major elaboration of the basic idea. And in mid-1972, the Institute for Problems of Organization and Management, a group within the State Committee for Science and Technology which had been specially formed to implement the national computer network, indicated that its assignment had been broadened to develop a "Statewide Automated System for Collecting and Processing Information for Planning and Administration (OGAS)." According to Schroeder:

> . . . as a part of this project the Institute is drawing up a plan for the location of a statewide network of computer centers and a general plan for the construction of a statewide data transmission systems. [Additionally,] OGAS is conceived as having a number of key functional subsystems, [among them] an automated system of plan calculations (ASPR), an automated system of [planning] norms (ASN), an automated system of state statistics (ASGS), an automated system for managing supply (ASU MTS), an automated system of standards and metrology (AIUS), an automated system for processing price information (ASOI), and an automated system for management of scientific-technical progress

(ASUNT). In addition to the national subsystems, there are to be subordinate "line" automated systems of management (ASU's) for republics and ministries (OASU's) and also for enterprises (ASUP's).[100]

Merely naming the systems suggests the scope and complexity of this program. Since forecasting is by no means the focus, it should be clear that, in this context, particular forecasting techniques, no matter how extensively used, are best to be perceived as auxiliary tools—as means of achieving a certain kind of answer, which, if acceptable, will have value only as an input to a larger whole. The point can be stated more crisply: Forecasting was sanctioned in the 1920's to the extent that it contributed to planning; nothing has changed now, except the definition of planning.

Even at this early stage of work on SOEF and OGAS, mathematical-economic techniques have been used with some success in the USSR. For example, the Institute of Economics and Industrial Production of the Siberian SSR Academy of Sciences has devised an interlocking set of computer models for describing and optimizing the relationships between national and regional economic development.[101] Similarly, a two-stage model was constructed to explore the possible economic development of Armenia. Despite some difficulties in handling prices, it was judged that the models reflected the structure of the economy satisfactorily, though they were unable to take sufficient account of external influences.[102] In general, according to another author's review of results over "the past 10 years" in building and applying such models, the record "shows that their use makes it possible to reduce current and capital outlays on the production of output by an average of 10% to 15% as compared with the plan variants calculated by traditional methods. But, needless to say, the gain . . . is not restricted to this. These models make it possible to solve problems of economic balance on an improved methodological basis, to analyze the various directions of the development of the national economy, to compute a large number of variants of economic planning decisions, and to select the best one."[103]

Common to all Soviet publications on mathematical-economic modeling is not only a statement of success, but also an indication of steps yet to be taken; these include development of a richer and more quickly updated data base, the introduction of better means for handling "the influence of social factors on production," and the invention of "a sufficiently reliable method for translating forecasting results into guidelines for planning." Perhaps most important is the procedural necessity to put less emphasis on building models intended primarily for solving discrete, one-time planning problems, and to turn greater attention toward the creation of "synoptic" models of the Siberian or Armenian sort. Whatever the weaknesses of these "systems-oriented" models, they nevertheless "mark the beginning of a new period in the development of economic model-building" in that they are directly supportive of the national planning program outlined earlier.[104]

Of specific mathematical-economic forecasting techniques, the one most closely associated by Western observers with the Soviet Union, and the one that the Soviets themselves seem to believe they have developed furthest, is input-output analysis. The history of I/O in the USSR is interesting, if only because it tends to recapitulate some ideas about forecasting discussed earlier in this chapter. For example, as in the case of cybernetics, when I/O first achieved prominence, it achieved *great* prominence. This occurred as a result of work performed in 1961, which involved constructing an empirical (i.e., retrospective) I/O table for the Soviet economy in 1959 and then using the principal coefficients from this table to build an experimental *planning* table for 1962. The results of this effort were very encouraging, as compared with expectations based on methods then in use: not only did the I/O table reveal potential roadblocks in production and indicate where resources could be diverted from intended uses to increase overall output, but the results were produced faster and apparently with less manpower. The immediate conclusion was to incorporate I/O in all subsequent planning.

From that point on, however, the bloom began to fade, as analysts discovered the difficulties of obtaining adequate statistical data and input norms, and of communicating the results of the analyses to decisionmakers. Thus, experimental planning tables for 1963 and 1964–1965 were attempted, but little was heard of them. The table created for 1970 was billed as "the first step in the practical use of this method for the construction of a five-year plan"[105] True to the spirit of the futures movement, the analysts also used this table to develop some 15–20 variants of the plan, from which they drew two "which best correspond to the political and economic tasks of the forthcoming five-year plan and also to the material and labor resources which will be available."[106] These they presented to the political leadership; neither variant, unfortunately, was well received.[107]

As of early 1968, according to one Soviet economist, "It would be no exaggeration to say that not a single important decision in current or long-range plans has been taken on the basis of construction of I/O balances."[108] Having made a commitment to the technique, however, the Soviets refused to abandon it. Indeed, academic and bureaucratic insistence on perfecting it grew. The extensive work that followed, particularly at Gosplan, led to the preparation of planning I/O tables for each of the years in the Ninth Five-Year Plan (1971-1975). Much the same claims were made for these tables—no doubt accurately—as were made for the 1962 table, and, like the 1970 table, these tables were described as "a decisive step" toward the application of I/O in practical planning.[109] Soviet specialists are now well aware, of course, of the strengths and weaknesses of I/O, both as a forecasting method and, as a mathematical-economic modeling technique that can contribute to SOEF or OGAS. And out of the 1970 experience, they have probably also come to appreciate more deeply how I/O, like any forecasting method that concerns itself with the entire "system," can threaten the leadership's prerogatives, particularly in goal formulation. It remains to be seen, however, how I/O will be fine-tuned in response to these perceptions.

Expert Opinion Methods of Forecasting

The gradual but ever-wider expansion of the charter granted to Soviet forecasters has inevitably been accompanied by relatively modest though increasingly serious efforts to develop, test, and incorporate judgmental techniques in forecasting, especially longer-range forecasting. Why has this happened? Though the extraordinary impact of the 1964 Gordon-Helmer report on Delphi, so influential in the West, was felt in the Soviet Union, very little of the new interest in opinion-based techniques should probably be attributed to this source.[110] Rather, the evidence suggests that Soviet appreciation of this class of methods was inspired principally by the unique character of the problems the forecasters were being asked to address, and by the approaches they were obliged to adopt in addressing them. Specific arguments that have been offered for the use of judgmental methods include the following:

—The farther one goes out in time, the more the uncertainties tend to multiply, and the greater the reliance one must place, therefore, on expert scientific opinion to cope with these uncertainties. Western observers will find this enthymeme quite comfortable, but they may wonder about its precise force and meaning in the Soviet context, where doctrinal and traditional beliefs would seem to imply precisely the opposite. Almost certainly the conclusion is meant to hold only for developments in narrowly defined areas (e.g., the future of shoes, ships, and sealing wax), including very precise lines of development in Communist society itself, such as the uses of leisure time or the creation of new art forms.

—Cross-disciplinary forecasting is necessary in some cases and, where it is, it requires reliance on expert judgment. This is due, in part, to the fact that while specialists in the conventional disciplines *could* prepare long-range forecasts, they have not done so.[111] Other workers, therefore, must make the attempt; this necessarily involves persons from other fields, who must estimate what otherwise could be computed. But a more compelling reason is that the range of subjects to be covered in a comprehensive, cross-disciplinary long-range forecast can be so large that, in the present state-of-the-art, only judgmental techniques provide a means of integrating the different kinds of forecasts.[112]

—Various aspects of the future can be dealt with only probabilistically and, in some cases, the needed probabilities can be derived only judgmentally. This is true both in the short term (where one may be practicing normative forecasting) and in the long run (where prognostications are exploratory).[113]

—In initially choosing forecast objects from all of the possible topics, trends, and future states of affairs, judgment is not simply useful but is in fact the only means available. There is no other way to start. Moreover, given practical constraints on time and resources in the forecasting effort, it is probably necessary to determine the relevance or ultimate value of these initial choices by making "preliminary forecasts" by judgment.[114]

—Similarly, "a forecast which has only a single object to be forecast on a single level of the national economy is, in the final analysis, always based on methods of expert opinion and extrapolation, inasmuch as the initial information regarding the future for making calculations with mathematical-economic models can be obtained only by such means. However, in making forecasts based on hierarchical interdependencies of a system of models, expert opinion and extrapolation of past tendencies are required only in the first stages of analysis."[115]

—Cutting across these earlier arguments is the additional and somewhat surprising notion that "forecasting would scarcely supply meaningful and objective information about the future if the most prestigious specialists did not participate in the work and if the points of view of different scientific schools and design trends were not adequately represented in the hypothesizing of complex forecasts."[116] Indeed, one writer goes so far as to assert the advantages to "the whole scientific community" of taking formal account of opinions that are distinctly off-beat.[117]

In general, the advantage of judgment and, hence, of expert opinion techniques in forecasting is that because human intuition can bypass "the methods of formal logic,"[118] it is "the sole source of information which . . . is ahead of what is contained in ordinary scientific information."[119] Therein, of course, also lies its main defect, because almost nothing is presently known about how to ascertain the reliability of substantive judgmental forecasts.[120] At the moment, since formal use of these techniques can no longer be avoided, one acceptable solution to this difficulty is to see to it that not only are the estimates themselves elicited, but also the reasons that stand behind them.[121] In a sense, the reasons can be the forecast; a recurrent theme in the literature is that expert opinion methods like Delphi hold greatest promise in situations where the most one can reasonably expect to obtain is a purely qualitative assessment of the future.

Among the expert opinion techniques occasionally mentioned by Soviet specialists are applications of utility theory,[122] a method called "simulation" which appears to be much like what is called operational or manual gaming in the United States,[123] Delphi itself,[124] and surveys and polls.[125] One of the few distinctive methods developed by the Soviets that has been described in any detail is the "collective expertise approach" or "the method of collective expert assessment,"[126] or CEA for short. This method—actually, a procedure or even a generalized study design—seems intended to serve as an alternative to Delphi. In fact, because one of its major goals is to highlight differences between well-defined groups of experts, it resembles in spirit the SPRITE approach developed by the Business Planning Group of Bell Canada.[127] The uniqueness of CEA is that it involves a combination of specific opinion techniques which, together, define the approach. In order, the steps in CEA and the submethods used are as given in Table 1.

The amount of effort implied by even this synopsis of CEA could be staggering. Unfortunately, the papers on CEA fail to answer several obvious questions: How many experts can or should be involved? How, precisely, do they interact? In face-to-face discussions? Through questionnaires? How many rounds of estimation are required for various types of estimates? Is anonymity preserved? How are disagreements between groups of experts resolved? Are any of the estimates derived as probabilities? How long does CEA take? Where and in what form are the final, analyzed results intended to be used in the planning or decisionmaking process? Indeed, has CEA ever been tried? As yet, there are no indications that it has. On one point, however, the advocates of CEA are clear, and their position is markedly different from an attitude common among Western advocates of expert opinion tech-

TABLE 1.

Step	Objective	Method
1. Preforecast orientation	Preliminary forecast of basic trends in the area of interest in order to "formulate the general aim" for the area.	Trend extrapolation
2. Construction of an "aims-means" matrix	Comprehensive assessment of the degree to which each identifiable set of relevant actions would contribute to the achievement of the "subaims" comprising the overall aim.	Cross-support matrix. (Distinctively, here each set of actions is expressed in terms of trends. The action set is the set of all known relevant trends!)
3. Formulation of "tables of expert assessment"	Assessment of (1) the relative importance of pursuing each set of actions; (2) the relative importance of individual trends within each action set; (3) the various subaims; and (4) the timing of particular events that might affect the trends. Also the individual participant's self-rated expertise on each assessment.	Relevance trees, various matrices, and simple questionnaires.

niques: The overriding purpose of CEA is to obtain certain kinds of information needed for "making decisions in [an] authorized area of . . . policy"; the activity would be a failure or near failure if all that came out of it was intellectual stimulation ("cognitive value") for the participants.[128]

This review of forecasting methodologies can be brought into sharper focus by observing that, substantively, the most important concern of Soviet forecasters in the last few years has been to help clarify and to project the future evolution of what has come to be known in the Communist countries as the scientific-technical revolution. This phrase refers especially to the fact that while science and technology have indeed been and continue to be "the main lever for building the material and technical basis of communism,"[129] the rate of development in these areas, coupled with the levels of economic well-being now achieved, place the Communist states on the edge of a major historical transition. Quantity can be turned into quality; science itself has become a fundamental productive force.[130]

> Today we are standing on the border of the historically formed industrial civilization; simultaneously, we are beginning to cross its horizon. The civilization of future decades will evidently be based on substantially different production forces than the industrial system of the past and present; and this transition will inevitably carry with it far-reaching revolutions and dramatic social and human problems. [Now] economic growth is beginning to depend much more on the overall state of science and the progress of technology than on a numerical increment of men and machines in the direct production process.[131]

For these reasons, among others, the 24th Party Congress in the Soviet Union declared that "speeding up scientific and technological progress is the decisive condition for raising the effectiveness of social production," and it held that "questions of speeding up [this] progress should be constantly in the focus of the attention of the Central Committees of the Communist Parties of Union republics, the territorial, regional, city and district Party committees, Party, government, economic, trade union and YCL organizations, and the collectives in enterprises and on collective and state farms."[132] In sum, every agency, every administrator, and every subordinate should give highest priority to the successful achievement of this revolution.[133]

Translating these conclusions into propositions (and marching orders) relevant to forecasting in the Soviet Union, V. M. Gluchkov and G. M. Dobrov anticipated the 24th Party Congress by writing in 1968:

> The high level of development of modern science, the unprecedented rate of scientific and technical progress, the unparalleled increase in its mutual relationships with other aspects of social existence—all of this requires that scientific and technical development be planned in a rational manner. [These] requirements of society for intelligent control . . . have created a new branch of knowledge—science management—and its newest sector—scientific and technical forecasting . . . . The most important goal of [this kind of] forecasting is the establishment of an environment within which the most promising directions for scientific and technical progress in our country can be selected and advanced.[134]

Attention to the necessity of exploring the course of the scientific-technical revolution has led, of course, to many books and papers, some of which shed a different light on questions of the choice of forecasting methods. A rather narrow but nonetheless interesting article (on how best to forecast indicators of scientific-technological progress in mechanical engineering) restricts the choice of methods as shown in Figure 6. A much more impressive analysis is given by U. A. Zykov;[135] its essence is summarized in Figure 7. According to Zykov, economic forecasts can have a variety of attributes, as indicated in the figure.[136] In this scheme, attributes can be combined only in the directions indicated by the arrows. (Thus, a forecast may be quantitative, normative, strategic, many-factored, and so on, but it cannot be descriptive and quantitative, or analytic and normative, at the same time.) The decisions among attributes culminates at the bottom of the diagram in one of four general classes of methods. A novelty of Zykov's analysis—one he himself misses—is that its applicability is not limited to either economic or scientific-technical forecasts. Clearly, it applies or can be made to apply to all of the *substantive* and *procedural* approaches discussed earlier.

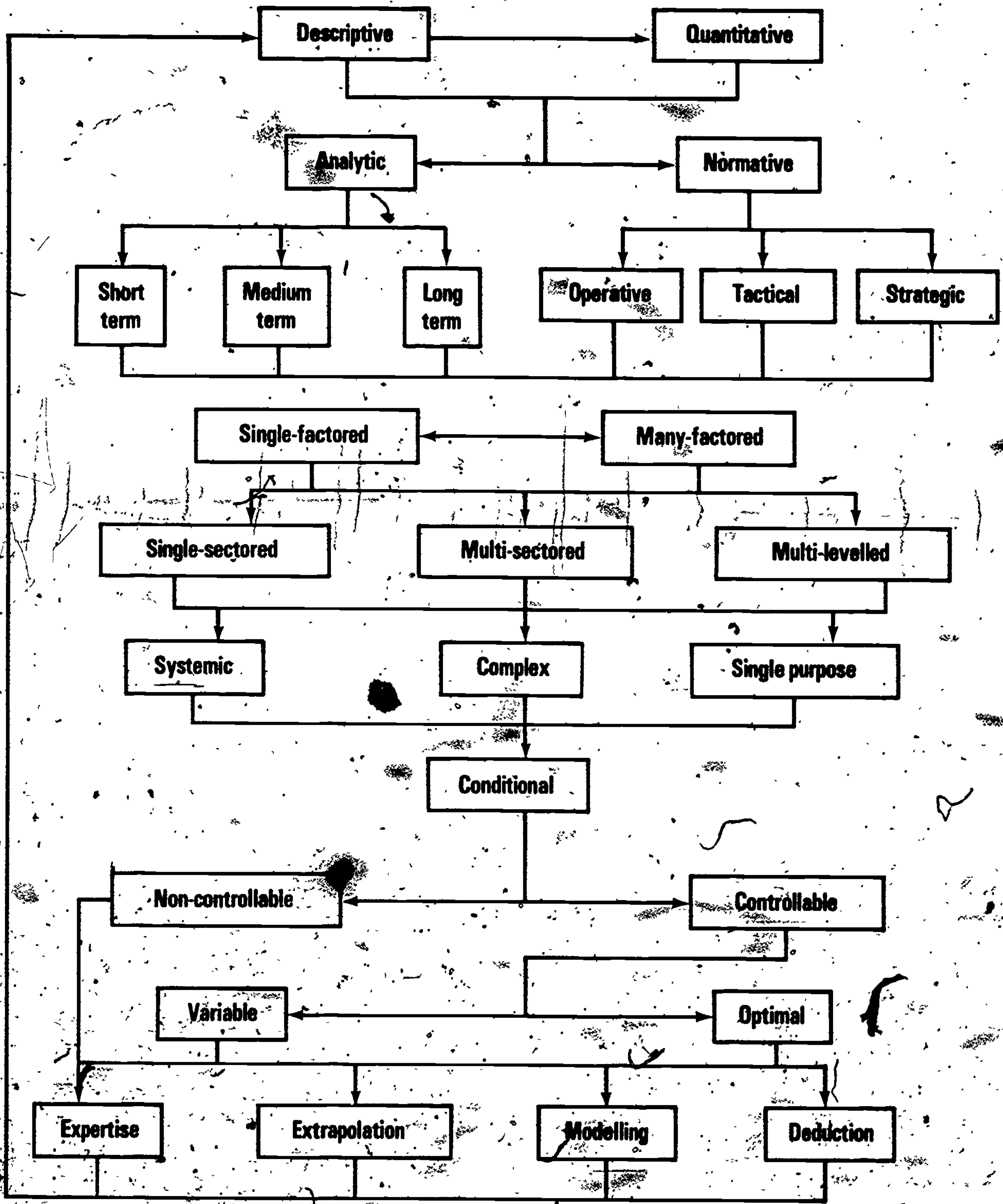
In this connection, it should be remarked that the notion of "universal" methods, while it might be congenial to Soviet forecasters since there is, after all, only one future in the end, has apparently not gotten much farther in the USSR than in the West. This is not to say, however, that attempts to develop generalized procedures, applicable within the context of any of the acceptable approaches, have not been made. We find, for example, that one of the pioneers in Soviet forecasting, Vladimir Lisichkin, has worked out a "complete" process which he believes provides a place

BASIC INDICATORS OF TECHNOLOGICAL PROGRESS	METHODS OF FORECASTING WHICH MAY BE USED: Dynamics of patenting	Analysis of patent information	Analogy	Expert opinion	Statistical interpolation and extrapolation	Methods of parametric forecasting	Multifactor regression analysis
1. Effect of new types of production equipment in chemical engineering.	X		X	X			
2. Effect of high-capacity equipment on production (coefficient of large-scale capacity).	X		X	X			
3. Contribution of automation to the total value of working machinery and equipment.	X		X	X	X		
4. Relative material capacity (in units of production).					X	X	
5. Index of growth of productivity of a single aggregate.		X	X	X			
6. Effect of new progressive materials.		X					
7. Durability (period of service before first overhaul).				X			
8. Level of standardization, unification, and normalization.			X	X	X		
9. Index of changing indicators of economicality of equipment.					X		
10. Index of price change.					X		
11. Index of quality of newly created or modernized equipment.			X	X			X
12. Technical equipping of labor.			X		X		
13. Availability of energy.					X		X
14. Level of mechanization of production; level of automation of production.					X		
15. Degree of mass production.			X		X		
16. Effect of new technological methods and progressive types of equipment.	X	X		X	X		
17. Level of specialization.					X		
18. Coefficient of the economical organization of labor.					X	X	
19. Technical level of production.					X	X	
20. Index of change in the cost of production; index of the change of productivity.					X		

Derived from E. G. Chistyakov and N. V. Dolganyeva, "Methods of Predicting the Progress of Science and Technology in Mechanical Engineering" ("Voprosy metodiki prognozirovania nauchno-teknicheskogo progressa v mashinostroenii"), *Isvestia AN SSSR Seria Ekonomicheskaya*, No. 2 (1972), p. 22.

Figure 6. Applicability of some methods in forecasting science-technology indicators.

for every "operational forecasting scheme" extant. Moreover, he is willing to assert that "the primary task for future developments in forecasting methodology will be the formulation and further elaboration of the principles and theories" embodied in his procedure.[137] This method of methods involves three stages (retrospection; diagnosis, and prognosis), in each of which specific tasks and steps are performed. No summary would do justice to the entire process, which is distinctive in concept and expression, and so we simply quote its essence here, omitting only the examples:



From L. A. Zykov, "On Certain Principles of the Economic Forecasting of Scientific and Technological Progress" ("O nekotorykh printsipakh ekonomicheskogo prognozirovania nauchno-teknicheskogo progressa"), *Izvestia AN SSSR Seria Ekonomicheskaya*, No. 3 (1973), p. 47.

Figure 7. Zykov's classification of types of economic forecasts of scientific-technological progress.

Stage I. RETROSPECTION

Task A. Refinement of the forecast object (i.e., delimitation of relationships between the forecast object and other objects, as well as the specification of feasible and permissible changes in the performance of the forecast object.)

Task B. Determination of the group to which the forecast object will belong [i.e., can it be forecasted perfectly or at least satisfactorily by itself, or must additional outside information about the object group be gathered and taken into account?].

Task C. Determination and precise interpretation of the parameters of the forecast object as a whole.

Step 1. Determination of the nature of the forecast (whether it is to be descriptive or quantitative.)

Step 2. Determination of the scale of the forecast object (local or global, etc.).

Step 3. Determination of the time period to be covered by retrospective analysis.

Task D. Analysis of the forecast object and definition of structural subunits, their parameters and properties.

Step 1. Isolation of the structural subunits of the forecast object.

Step 2. Exact definition of the object parameters (their characteristics and structural subparameters).

Step 3. Determination of the types of relationships between the structural subunits.

Step 4. Determination of the basic physical effects that govern the action of the subunits and, hence, the forecast object as a system.

Task E. Determination of the significance of each parameter of the structural subunits of the forecast object (as was done for the object as a whole in Step C).

Task F. Construction of an approximative and hypothetical model of the forecast object.

Stage II. DIAGNOSIS

Task A. Determination of the objective of the forecast (e.g., confirmation, estimation, tentative assessment, planning, management).

Task B. Determination of the relationship between forecaster and forecast object (e.g., passive, active).

Task C. Determination of the current characteristics of the forecast object.

Task D. Determination of the noise level for objects in this class and of coefficients representing these noise levels (where "noise" consists of factors which cause a loss or lessening of the information available about the object).

Task E. Determination of the forecasting methods to be used, singly or in combination.

Task F. Determination of methods to be used for evaluating the probability and accuracy of the forecast and for verifying it.

Stage III. PROGNOSIS (FORECAST)

Task A. Preparation of subunit forecasts.

Step 1. Forecasting of all subunit elements (following appropriate rules).

Step 2. Checking for the determination (i.e., to ensure that the same scientific language is used on the subunit level as is required for the objects as a whole).

Step 3. Checking for justifiability (i.e., to ensure that all requirements and problems identified for each subunit in Stages I and II have been dealt with).

Task B. Synthesis of the subunit forecasts of the same class.

Step 1. Synthesis of the forecasts (following appropriate rules).

Step 2. Checking for the determination.

Step 3. Checking for justifiability.

Task C. Integration of all classes of subunit forecasts into the forecast object (following appropriate rules).

Lisichkin's all-purpose method was stillborn, if the utter silence since its publication is the test. In spirit, however, it certainly anticipated an imposing order issued in late 1972 by Gosplan, the State Committee for Science and Technology, and the USSR Academy of Sciences. Entitled "Main Methodological Principles and Mandatory Requirements for the Completion of Scientific and Technical Forecasts," this directive sought to solve the set of problems, increasingly voiced from the late 1960's on, of inconsistencies among approaches and methods, unnecessary duplication of forecasts, incompleteness in the final results, and inadequate communication of findings to the planning and decisionmaking apparatus. As highlighted by Schroeder, who characterizes it as the "very epitome of a bureaucratic document," its central provisions are that henceforth every forecast, whether technological or social:

> . . . (1) must contain both technical indicators and indicators of the economic effectiveness of various ways "to implement domestic and world achievements" in the field involved; (2) should include an evaluation of the "social consequences" of each forecast development; (3) should include an analysis of relevant past and present developments in the USSR and the world and a prediction of developments in the period concerned; and (4) must be submitted to five separate agencies, with mandatory coverage of a large number of specified items.[138]

Curiously, the more recent Soviet literature on forecasting contains no mention whatsoever of this decree, though by implication the authors consistently support its intention. By the same token, however, *none* of the literature refers even to the existence of such official documents as Gosplan's book of standard methodological instructions for planning, which has been around for some years and, importantly, was revised in 1972–1973. Even if—to take the extreme case—the specialists whose work we have considered in these pages are not bound by these directives and guidelines, why have they not, in their published writings, sought explicitly to influence them?

By way of concluding this discussion of methodology in the USSR we might note another curious omission from the forecasting literature: the total absence of references to the extensive Soviet research that has been and is being performed in parapsychology, including precognition—the ultimate expert opinion technique. In contrast to the United States and other Western countries, the USSR has evinced a strong serious interest in parapsychology, and precisely in the same 10 years which have witnessed the appearance of the Soviet futurists.[139] By 1970 some 30 conferences on the subject had been held in the USSR—all but one or two of them, incidentally, closed to outsiders. Indeed, after the February 1968 conference (on "Scientific Problems of Telepathy"), the Novosti Press Agency declared, "It is clear that in the USSR, official science is actively and passionately inclined henceforth toward the enigmas of parapsychology."[140] Not only has "official science" made repeated careful tests of such nationally famous Soviet psychics as Wolf Messing, Nelya Mikhailova, and Karl Nikolaiev,[141] but, more important, it has supported (to the level of some 20 million rubles a year) research directed toward highly practical applications in such areas as space flight, military operations, and crime prevention.

It would seem straightforward for the Soviets to begin to ask whether what has been learned in this work can be transferred into scientific forecasting, but this has not happened yet, at least not publicly. Quite possibly the explanation is simply that too much is at stake. In the technical language of the forecaster and planner, however, the issue is not the role that might be played by a forecast in the planning process, or even its source; rather, the issue is one of criteria for evaluating its scientific merits. Among the Soviets and the Communists generally, as we have seen, these criteria are of various kinds—doctrinal, political, economic, logical, scientific, social, and, finally, personal—and they are applied very conservatively in scientific forecasting, as elsewhere.[142]

Some Conclusions

Soviet forecasting, we could conclude, begins and ends in the vision of the future given by the doctrine. This vision, this image, provides much and demands nothing—except recognition, belief, and elaboration. As with all good secular utopias, a principal strength of the Communist vision lies in the rather pleasurable tension existing between the fact that its ultimate goals seem almost ineffably desirable (being compounded, as they are, of roughly three parts golden rule and one part pure greed); and the fact that, given the peculiar nature of the world, the vision requires almost endless elaboration, both in thought and action. Thanks to the imp of the perverse, invincible ignorance, sloth, and other maladies of man, life has a way with noble and self-evident ideas in a finite world. From this simple fact arises the need for party, program, plan, and prognostication—none of which, everyone would agree, is itself ineffably desirable. But, of course, the doctrine provides the solution: eliminate the first by pursuing and gradually improving the final three. And since those individuals who stand to be eliminated along with "the party" have the most to lose, the doctrine also consecrates them to govern the process. Thus, in concrete experience, the vision is transformed into a truly cybernetic system, within which every act, process, and role, including that of forecaster, has a place and a purpose.

Accordingly, the important changes which have occurred in the practice of scientific forecasting in the Soviet Union over the last 50 years have taken place only in response to altered conditions identified by the party. For the first 40 years, and to the extent that forecasters had a separate identity at all, they were mere handmaidens to the economic planners and administrators. In recent years, largely because of the scientific-technical revolution and the program to implement a national planning system, they have become mere handmaidens to *all* types of planners and administrators. What might cause a Western observer to believe that this shift amounts to a fundamental redefinition in the role of the Soviet forecaster is the shock of discovering that the Soviets have in some respects now become like the mere handmaidens in the West who practice futures research. Whereas the Westerners started in technology, the Soviets began in economics. Each, it might seem, is now simply discovering the other's field, and both are discovering social forecasting. Superficially, there is something compelling about this view, if only because it suggests that real opportunities for East-West cooperation in forecasting may be found. While these opportunities do exist and should be developed, the view itself is wrong. The doctrine, the party tradition, and the actual experience of forecasters in the USSR make it so, as the Soviet forecaster is first to declare.

It is extremely important that Western forecasters understand what his declaration amounts to, explicitly and implicitly, because it tells us not just about forecasting in the Soviet Union, or about forecasting under conditions of dictatorship, or even about forecasting "when the future is known"—revealing through these undoubtedly are. In addition, it tells us a great deal about the very aims of forecasting, in this world, in this era. And this message goes well beyond the day-to-day problems of getting the last report published, meeting the next deadline, preparing for the next conference, or whatever. If we take the Soviet case seriously—and we must—we have a chance to ask ourselves all of the fundamental questions about the significance of forecasting in the West.

To spur such questioning, we offer the following six points as a kind of summary of the preceding pages and of the "declaration" from the USSR that runs through them:

1. The Soviet forecaster invariably sees forecasting as a way of helping to achieve the Communist victory, a victory that is never questioned. Bestuzhev-Lada is the most direct of all of his colleagues in making this point. In his paper for the Third World Future Research Conference in Bucharest in September 1973, he put it plainly: "Marxist social forecasting is a form of concretization of scientific Communist theory and an instrument of socialist planning."[143] Many similar statements can be found, as, for example, this one:

> Marxist-Leninist social prognostication . . . rests on the solid foundation of dialectical and historical materialism, on the theory of scientific communism, and it is rooted materially in the socialist mode of production, thus opening up the broadest possibilities for planned development of the economy and society as a whole. Therein lies the basic advantage of Marxist-Leninist prognostication over bourgeois futurology. What is needed is [only] to make better use of this advantage.[144]

With this understanding and framework, the Soviet forecaster obviously does not have to wonder about the ultimate purpose of the contribution he is asked or ordered to make. Specialists may and do disagree, of course, about the value of this method or that assignment, but the final arbiter of such arguments is always how time and skill can be used most effectively in the work of building Communism.

2. The Soviet forecaster sees forecasting by non-Communists as a rearguard, and perhaps blackguard, activity, destined to prove pointless. Until Communism has triumphed, however, it is also an article of faith that "social forecasts [by Communist and non-Communist specialists] have been and remain an ideological battle field."[145] Marxist and "bourgeois" forecasting are in a permanent, irreconcilable conflict; its only resolution will come with the death of capitalism.[146] When, for example, in 1972 the Economics Institute of the USSR Academy of Sciences was blistered by the party's Central Committee for failing to carry out its directives well, a portion of the complaint was concerned not only with the fact that the Institute had given "extremely little attention . . . to the methodology of long-range planning and social and eco-

nomic forecasting," but also that it had done "insufficient work . . . to explore the scientific groundlessness of bourgeois and revisionist 'theories' in the field of economics." And not enough had been done by the resident party organization to cultivate in the staff "the spirit of uncompromising struggle against bourgeois ideology, anticommunism, and revisionism."[147] While the Romanian Apostol can needle those "experts on 'social prognosis' from the socialist countries [who] denounce the 'bourgeois' character of futurology"[148]—a clear reference to Bestuzhev-Lada in particular—he is apparently alone in suggesting the possibility that Western forecasting is not necessarily antagonistic to Communism, and may even have a positive value. All Soviet writers who consider the question disagree. Against this background, Western forecasters can hardly take comfort in the fact that Western analysts have proclaimed the Cold War to be over or that the prospects of a higher synthesis between capitalism and Communism are good. A recent Polish paper states the Communist viewpoint plainly: "the theory of convergence, according to which because of technological progress countries with different social systems converge in the course of their development, is simply illusory."[149]

3. Not only does the Soviet forecaster know where he is going and who his intellectual opponents are, but he also has a better idea than his Western counterpart of what steps he must take in the short term to improve his profession. Western forecasters, lacking any sense of ultimate goals, tend to be problem-oriented, and problems tend to come to them one by one, without any underlying structure. As we have seen, it is much different in the USSR. Keenly appreciative of the dangers of "running too far ahead,"[150] Soviet specialists are working steadily away at an increasingly well-defined set of problems that includes developing a general theory of management, a system of "continuous" forecasting, SOEF, and OGAS. They are training forecasters; they are laying in the hardware and software they will require; and they are working on basic theoretical issues. Western writers, including many who have been cited in these pages, seem to relish pointing out the bureaucratic and conceptual blunders of Soviet planners and analysts as they move toward these objectives. These writers have presumably not yet noticed what happens in, say, the United States. In any event, they assuredly miss the point that the Soviets *have* enunciated their goals and *are* proceeding toward them, even if two steps forward and one step backward are required. We should keep constantly in mind, as the Soviets do, the success of the GOELRO plan for electrification.

4. While holding deeply to his belief that bourgeois forecasting involves blind service to a lost cause, the Soviet forecaster attends carefully to Western studies and, particularly, Western developments in methodology, even though the regime may not have yet sanctioned their use. Most negative criticism of the West in Soviet technical papers attacks not the methods, but the motives of the forecasters, often accusing them of selecting or deliberately distorting primary data in order to project a bright future for capitalism.[151] In any event, as indicated earlier by plotting the citations from Bestuzhev-Lada's *Window* and Richta's *Civilization at the Crossroads,* the Soviets and East Europeans pick up the Western literature within about two years. In contrast, forecasters in the West—or at least in the United States—seem to be totally oblivious to the Communist literature,[152] despite the fact that much of this material is translated. In effect, therefore, the Soviet specialist has an especially free good: Western developments are put at his disposal at no cost; he can (and does) use them to advance Communism; and Western futures researchers neither know what is happening nor bother to take advantage of research developments in the Communist countries.

5. The Soviet forecaster and the Western futures researcher are alike in being serious about their work and highly optimistic about the future.[153] Soviet optimism is expressed in various ways (though always, it should be repeated, in the context of the doctrine). Thus, Siforov writes, "It must be borne in mind that the potentialities of science and technology are practically inexhaustible. Only those predictions [of new developments in these areas] cannot be realized which are in conflict with the laws of the evolution of nature, society, and thinking.'" [154] And insofar as forecasting itself is concerned, there is this opinion:

> The forecasts made by nineteenth century scientists for the beginning of the twentieth century were 80 percent correct. Many scientists of the latter half of the twentieth century estimate that the accuracy of forecasts made for the beginning of the twenty-first century has increased to 90–95 percent. This belief is based on the fact that the basic laws of scientific-technical progress have been discovered, practical methods of forecasting have been developed, and problems which can be solved successfully by scientists of various countries have been defined.[155]

Quite possibly, however, there is a difference between East and West in their optimism: the Soviet is optimistic *because of* what he forecasts; the Westerner, *despite* what he forecasts. The Westerner needs to have something better; the Soviet already has it—in the doctrine.

6. Finally, the Soviet forecaster is not at all adverse to participating in international symposia and surveys, and perhaps even in international forecasting studies.[156] It does not matter, of course, whether the non-Communist participants understand why. But they should know some of the groundrules. Bestuzhev-Lada indicates what they are:

> . . . it is both possible and necessary to carry out a principally scientific debate [between Communists and non-Communists], compare one's position, and persuade by well-founded argumentation . . . . Soviet scientists categorically refuse attempts to use the problems of social prognostics for pro-capitalist propaganda purposes, that is, attempts to present as scientific prognoses the simple utopias of "eternal capitalism." But they are prepared to continue the dialogue with Western non-Marxist scientists about the whole series of questions which worries modern man, from the fight against air and water pollution and erosion, to the struggle against poverty, hunger, and diseases, for disarmament and peace all over the world and for a brighter future for mankind.[157]

When all is said, it is probably in this area that the greatest opportunity for establishing *useful* communication between forecasters in the two social systems can be found.

Implicit in these final paragraphs—and, indeed, throughout this chapter—are a number of candidates for research. The success of any of them depends, however, on an issue that transcends forecasting: the understanding we have of the Communist vision, its structure, and its dynamics. Fred Polak has concerned himself more than anyone else, perhaps, with the role of images of the future in determining basic values, priorities, and action. We need not concur with his entire analysis of this subject to agree that one of his conclusions is especially germane here, and it is offered as the conclusion to this chapter.

> Although much of our thinking about the future today is inevitably in terms of choosing between the two competing images of the future which the East and the West have set before us, we must in the long run pass beyond these dichotomies which paint the future in black and white. Neither Russia nor America alone can spawn the future. The image of the future, at its best, has always been universal in character, a vision to serve and foster the growth of all mankind . . . . A vision of the future which falls short of this universality will in the end leave the earth a smoking ruin. The same tool cannot serve simultaneously as sword and ploughshare, and the scope of the vision will determine the final use to which the tool is put . . . . Our first challenge is to examine the basic foundations of our existing visions, to analyze their contents, scope, and direction. Analyzed in these terms, how did the Russian go so far astray? Why are the traditionally full-speed-ahead Americans bogging down? Are they indeed bogging down? These two questions lead straight into two major projects which social science must undertake, the theory and dynamics of image formation and propagation, and field studies of current images of the future in action.[158]

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[1] I am indebted to Koit Ojamaa, formerly with the Department of Linguistics of the University of Connecticut, for these translations, as well as for his assistance in tracking down and helping to organize some of the literature.

FOOTNOTES

[1] The second point is not meant to be facetious. Where can one find, for example, even a single case in which a Communist has acknowledged the validity of any evidence or any argument offered by a non-Communist that would overturn a "law" of dialectical or historical materialism? Surely not in formal written confrontations, as witness the rejoinder by I. V. Kuznetsov to A. J. Ayer in *Problems of Philosophy* (Voprosy Filosofii), No. 1 (1962), on the issue of whether philosophy—including dialectical materialism—can be a science. The exchange is reprinted in *Soviet Studies in Philosophy,* Vol. 1, No. 1 (Summer 1962), pp. 14–36.

[2] Frederick Engels, *Socialism: Utopian and Scientific* (New York: International Publishers, 1935), pp. 70–71.

[3] "Excerpts from New Communist Program," St. Louis *Post-Dispatch* (July 30, 1961), p. 6D.

[4] V. I. Lenin, *Sotsial-Demokrat* (January 31, 1917); quoted in Nathan Leites, *A Study of Bolshevism* (Glencoe, Ill.: The Press Press, 1953), p. 81.

[5] Lenin, *Pravda* (May 9, 1918); quoted in Leites, *op. cit.,* p. 81. Leites provides many similar quotations on this point (pp. 79–83). It is thus noteworthy, perhaps, that the draft Program presented at the Twenty-Second Party Congress (fn. 3) asserted that "the U.S.A., the strongest capitalist power, is past its zenith and has entered the stage of decline."

[6] In the Soviet Union, Malenkov was the first major leader to declare that another world war might mean "the destruction of world civilization" (*Pravda,* March 13, 1954), but he was apparently forced by others in the Presidium to modify his position shortly thereafter. By April 1954 his position was that a nuclear war would mean only "the end of the capitalistic system." In any event, it was Khrushchev who first argued that full-scale war between the capitalist and socialist systems may not be inevitable; "peaceful competition," he maintained in his public statements, could be sufficient to ensure the worldwide victory of Communism. Within independent states, moreover, the establishment of Communism could be achieved without civil war; that is, the "revolution" need not be violent. This left the colonial states, and here, at least, it has been asserted that "as long as imperialism and colonialism exist, *wars of national liberation and revolutionary wars are unavoidable.*" [See V. D. Sokolovskii, *Soviet Military Strategy,* translated with an analysis by H. Dinerstein, L. Goure, and T. Wolfe, Report R-416-PR (Santa Monica, Calif.: The Rand Corporation, April 1963), p. 281; italics in the original.] In contrast, the Chinese have not publicly abandoned the doctrine of an inevitable conflict between Communist and non-Communist states. Indeed, Chinese Vice Premier Teng Hsiao-ping made the astonishing statement in Peking on May 25, 1974, that "the contention between the superpowers [i.e., the United States and the USSR] is growing more and more intense and is bound to lead eventually to war. Either they will fight each other, or the people will rise in revolution." Both the Russians and the Chinese have proclaimed that if war comes, they at least will not be the first to attack. This is good Leninism, but it is impossible to tell how seriously it should be taken. There is, for example, evidence that at the very time when Marshall Sokolovskii and his associates were preparing the book cited above, Khrushchev was not only working to develop a first-strike capability, but was fully prepared to use it. Uncertainty regarding Communist beliefs and intentions on this question remains high, a fact indicated in the United States by the discussion that began anew (after SALT I) on targeting doctrine for strategic ...ons.

[7] I. V. Lenin, *The Teachings of Karl Marx* (New York: International Publishers, 1930), p. 33.

[8] *History of the Communist Party of the Soviet Union (Bolsheviks): Short Course* (New York: International Publishers, 1939), p. 355. This extraordinary claim should not be dismissed as still another abberation of Stalinism. It remains today as a vital part of the Communist philosophical and political outlook. We find, for example, that Brezhnev, in his "Report of the Central Committee" to the Twenty-Fourth Congress, stated, "Comrades, our Party is a party of scientific communism. It is steadfastly guided by Marxist-Leninist science, which is the most advanced, revolutionary science of modern times, and does everything for its further development. Theoretical understanding of the phenomena of social life and of its main trends enables the Party to foresee the course of social processes, work out a correct political line and avoid errors and subjectivistic decisions." [*24th Congress of the Community Party of the Soviet Union, March 30–April 9, 1971: Documents* (Moscow: Novoste Press Agency Publishing House, 1971), p. 121. This volume will be cited hereafter as *24th Party Congress Documents.*]

[9] Lenin, Speech of December 23, 1921; quoted in Leites, *op. cit.,* p. 83.

[10] Liu Shao-chi, *How To Be a Good Communist* (Peking: Foreign Languages Press, n.d.), p. 38.

[11] Nikita Khrushchev (1958), as quoted in Moshe Decter (ed.); *The Profile of Communism,* rev. ed. (New York: Collier Books, 1961), p. 33.

[12] G. V. Kozlov, *Questions of Economics* (April 1952), as quoted in Ralph Hamil, "The Cloudy Future of Communism," *The Futurist,* Vol. IV (December 1970), p. 214.

[13] St. Louis *Post Dispatch* (July 30, 1961), p. 6D. Incidentally, the "complete" triumph of socialism in the Soviet Union had first been proclaimed in 1936; see *History of the CPSU (Short Course),* pp. 319, 342.

[14] Engels, *op. cit.,* pp. 72–73. These assertions may be usefully compared with the answers given in a survey of a handful of Soviet citizens, all of them born after the death of Lenin, to the question, "How do you visualize communism?" While mention was made of technological advances, especially those that would reduce physical labor or increase longevity, the dominant theme was of a time when "human relations" will have changed, when man will be free of "material worries" and thus "able to devote himself fully to his spiritual interests," when the "social situation" will have developed to the point that "will allow everyone full rein to develop his creative potential." ["The Future We See," *Soviet Life,* No. 3 (March 1972), pp. 30–31]. This theme can be traced as far back as 1845 in the writings of Engels. Somewhat different is this opinion: "The fifty-year experience of the existence of the socialist state with its successes and failures, its victories and difficulties, will become, I believe, the foundation on which one can build the system of the future life of mankind . . . . The union of people, free from the operation of chance, will, of course, not be free from fresh conflicts and contradictions, the nature of which cannot be foreseen, and it is pointless to guess at. If we nevertheless try to imagine the people of the [distant] future [we should do so by trying to] bring out the best in the twentieth century man. One of the decisive factors in this process will be liberation of the creative element in man . . . . Creative work ennobles a man, raises him above selfish interests, makes him better and more free. I confess that when I think of the future I think above all

of a life in which every one will be able to determine his vocation and follow it." [Daniil Granin, "A Journey into the Future," in Robert Magidoff (ed.), *Russian Science Fiction, 1969* (New York. New York University Press, 1969), pp. 16-17.] A sociologist interviewed in the *Soviet Life* survey reported that in a study of his, some 2,000 respondents were asked what they "personally need to be happy." By far the largest percentage (77) gave an answer that would perhaps have both delighted and dismayed the classic Marxists. "To have an absorbing job."

15 "Through [the] early years Russia endured because she lived in the future. The glorious life-to-be would compensate her for the drab life-that-was. A new political system, a new freedom, a new emancipation for the individual, a new and speeded industrialism, and a new distribution of the products of industry on a more equitable basis—all these were fruits to be reaped in the future." So wrote the "Red Dean of Canterbury," Hewlett Johnson, *The Soviet Power: The Socialist Sixth of the World* (New York: International Publishers, 1940), pp. 86–87. Many similar statements can, of course, be found; like the future outcome to which they point, they also have a peculiar timelessness about them, so that they are equally accurate in 1920, 1940, 1960, or 1980.

16 Reference will occasionally be made in the following pages to the work of forecasters in the socialist countries of Eastern Europe, who have at times adopted attitudes toward problems of futures studies which differ from those of their Soviet counterparts, though usually with the insistence that these attitudes are nevertheless consistent with Marxist doctrine. Notably missing from this discussion are references to ideas from the Peoples Republic of China and its ideological allies. One cannot help regretting that the Chinese have not as yet seen fit to participate, if only as critics of the "bourgeois West," in the open discussion and development of futures research, especially because even the scant evidence provided by Chinese English-language publications suggests that the Peoples Republic of China (PRC) views forecasting in a unique way. For example, unlike other Communist countries, the PRC almost never refers in its public statements to "the future"; while there is an unmistakable futures orientation, the phrase is conspicuously avoided. Similarly, the PRC almost never refers to what "will" occur; [illegible] statements talk about what "is" the case (and, thus, what nee[illegible] be corrected or built upon), and sometimes what "must" be the case (and, thus, how things can some day be expected to materialize). Forecasts are so rare as to be nonexistent, even when banal. (*China Pictorial*, No. 8 [1968], carries an article on pp. 38–39 entitled, "A New Peak in Computer Technology," in the course of which reference is made to "Chairman Mao's brilliant prediction, 'The Chinese people have high aspirations, they have ability, and they will certainly catch up with and surpass advanced world levels in the not too distant future.' " This is the only mention of a prediction or of forecasting in several years' worth of issues of this magazine.) Since the Chinese are committed to planning, however, they are undoubtedly engaged in forecasting. But how is this work organized? What methods are being used? What subjects are being investigated? How are the results integrated into the planning process? Answers to these and other questions are difficult to find; moreover, there would seem to be no awareness within the PRC of the worldwide futures movement. The interaction that might flow from such an awareness has thus, unfortunately, been precluded. Yet as a sign that this aloofness may change, we might observe that Mao's "brilliant prediction," quoted above, tends merely to echo an argument that led in 1867 to the establishment of the first college in China for the study of foreign knowledge. In its recommendation to the Imperial Government for the creation of such an institution, the "Foreign Board" in Peking made these key points. "It has occurred to your servants that the appliances of foreigners, their machinery and fire-arms, their vessels and carriages, are one and all derived from a knowledge of astronomy and mathematics . . . . The Chinese are not inferior in cleverness and intelligence to the men of the West, and if in astronomy and mathematics, in the examination of causes and effects (in natural history, manufactures, etc.), in the fabrication of articles and successful imitation of models, and in prediction of the future, students will so earnestly apply themselves as to possess themselves of all secrets, China will then be strong of her own strength." The government accepted this argument, despite violent opposition by those who believed that a proficiency in the philosophy of Confucius was sufficient for educated men. The parallels seem clear.

17 Here and in what follows, we shall use the phrase "the doctrine" as a shorthand for terms like "Marxism," "dialectical materialism," and "historical materialism" in order to avoid bogging down the discussion in theoretical niceties. As mentioned before, however, these terms are not interchangeable; readers should thus be sensitive to possibly significant shifts in reference.

18 Needless to say, there is an ambiguity here. If Communism were to be achieved next year, then "short term" might mean "next month," and the alternative steps to be forecasted and evaluated would concern those over the coming 30 days. As the attainment of Communism recedes into a more distant future ("sooner or later"), so the period that is "short term" grows. It may be that this ambiguity has helped permit some Soviet forecasters to begin speaking of long-range forecasting and to evince an interest in futures research. If so, however, the obvious danger arises that the forecasters risk being accused of implicitly asserting that the achievement of Communism must be put farther off.

19 Leites's first book was *The Operational Code of the Politburo* (New York: McGraw-Hill Book Co., 1951). It was followed in 1952 by Leites, *A Study of Bolshevism* (cited in fn. 4), which focused primarily on ennunciating the portions of the code that concern the relationships between the Party and the world at large. A later book—by Leites and Elsa Bernaut, *Ritual of Liquidation: The Case of the Moscow Trials* (Glencoe, Ill.: The Free Press, 1954)—focused on the code as it governs internal party relations at the highest level. The entire series is based on an analysis of the papers and speeches of Lenin and Stalin, though literally hundreds of other sources are considered.

20 Leites emphasizes that "you cannot predict the score in a game from its rules; [but neither] can you predict it without knowing the rules"—including the rules for making rules. He points to four limitations of the code: (1) it intermixes serious statements with an unknown amount of "mere propaganda" (a drawback that can, however, be solved by testing forecasts derived from the code against actual practice), (2) the code is "highly ambiguous", (3) it contains many contradictory statements (as does Communist doctrine itself), and (4) it is incomplete in the sense that "it is far from affirming rules of conduct for all important occasions and on all important matters." (*Study of Bolshevism*, pp. 17–18.) It might be noted that these imperfections can be eliminated or reduced, in Leites's opinion, through formal content analyses. But the very task of trying to establish an operational code might best be seen as a kind of speculative inquiry that must precede any such analysis, where the desired information is slight or inaccessible (as is the case in the inner councils of any regime), creation of a code may well be all that one can do. And it may have considerable value, if only in forcing the analyst or policy-maker to organize his impressions. Cf. Alexander L. George, *The "Operational Code": A Neglected Approach to the Study of Political Leaders and Decision-Making*, Research Memorandum

RM-5427-PR (Santa Monica, Calif.: The Rand Corporation, September 1967).

[21] Except as noted otherwise, the items considered here are abstracted (or inferred) from the discursive presentation in *A Study of Bolshevism*, Chapters I and IV.

[22] The importance of the belief in "no accidents"—while it admits of several interpretations and a genuine indeterminancy—cannot be overemphasized. According to Meyer, the phrase is "constantly heard in the Parties"; he finds its classic expression in Engels's *Ludwig Feuerbach*, which Meyer quotes as concluding: "Historical events . . . appear on the whole to be . . . governed by chance. But where on the surface accident holds sway, there actually it is always governed by inner, hidden laws, and it is only a matter of discovering these laws." [See Frank S. Meyer, *The Moulding of Communists* (New York: Harcourt, Brace and Company, 1961), pp. 58–61 and 193, fn. 13. Meyer, incidentally, outlines his own "code"—a fascinating description of how Party cadres are created, motivated, and exploited.]

[23] One has to look outside the USSR to find a socialist forecaster who explicitly uses the doctrine to argue the case for "multiple futures" and the tenet that the determinism even of ultimate outcomes should be understood probabilistically. The best case of this sort is made by Romanian Pavel Apostal, "Marxism and the Structure of the Future," *Futures*, Vol. 4, No. 3 (September 1972), pp. 200–210. This is not to say, however, that probabilistic approaches in forecasting are not used or advocated in the USSR; such approaches are considered later in this chapter.

[24] Engels wrote in *Anti-Duhring*, "Freedom of the will . . . means nothing but the capacity to make decisions with real knowledge of the subject. Therefore the *freer* a man's judgment is in relation to a definite question, with so much greater *necessity* is the content of this judgment determined; while the uncertainty, founded on ignorance, which seems to make an arbitrary choice among many different and conflicting possible decisions, shows by this precisely that it is not free; that it is controlled by the very object it should itself control" (quoted in Meyer, *op. cit.*, p. 57). This is the accepted doctrine of the party; for example, Lenin refers to it with approval in his *Teachings of Karl Marx*, p. 11.

[25] In contrast, some Soviet writers of fiction and historians have made forays—each personally dangerous to the author because unacceptable to the bureaucracy—into distinctly different alternative futures. One especially interesting venture, though more a call for reform that a forecast, is to be found in Andrei Amalrik's *Will the Soviet Union Survive until 1984?* (New York: Harper & Row, 1970), which portrays a spiritually moribund Soviet regime, isolated from the people and from the world, seemingly incapable of infusing itself with new life and, thus, destined to destroy itself or be destroyed. Worse yet, Amalrik's tract, which obviously could not be published in the USSR, portends global catastrophe unless certain changes are made. (It might be mentioned in passing that while Amalrik sees little hope for the world, he sees none at all in the work of *Western* forecasters, who appear, in his judgment, to be as unlikely to tackle the truly pressing problems of our time as anyone else. Here are the concluding lines from his book: "Evidently, if 'futurology' had existed in Imperial Rome, where, as we are told, people were already erecting six-story buildings and children's merry-go-rounds were driven by steam, the fifth century 'futurologists' would have predicted for the following century the construction of twenty-story buildings and the industrial utilization of steam power. As we now know, however, in the sixth century goats were grazing in the Forum—just as they are now, beneath my window, in this village" [p. 67].) Another vision, this one embodied in the manuscript version of Nikolai Amosoff's fictional *Notes from the Future* (published in a sanitized abridgement by Simon and Schuster in 1970), involved the world as it would appear to a person who had been placed in anabiosis in the late 1960's and awakened 50 years or so later. The manuscript did not survive the state's censor, but the reason was not that the story was filled with fantastic technology. Rather, it was that when the hero was revived he did not find a "happy all-Communist world." The history of the manuscript is told in George St. George, "1991," *Look*, Vol. 34, No. 14 (July 14, 1970), especially pp. 55–58.

[26] *History of the CPSU (Short Course)*, p. 355. In this context it is worth recalling Engels's comment on religion and philosophy, all of which, he said, have a stock of "bunk": "The history of science is the history of the gradual clearing away of this nonsense, or rather of its replacement by fresh but always less absurd nonsense." Frederick Engles, Letter to Conrad Schmidt (August 5, 1880); in Lewis S. Feuer (ed.), *Marx and Engels: Basic Writings on Politics and Philosophy* (Garden City, N.Y.: Anchor Books, 1959), p. 405.

[27] Western writers have frequently failed to see or to appreciate this highly optimistic motif in the party tradition and the doctrine; they have preferred to amuse themselves with the notion that, in the end, the complexity of affairs makes it as necessary for the Soviets as for those in the West to "muddle through"—now using computers, of course. Hence, for example, the fanciful idea that the two systems are converging. Hence, too, the idea that when it comes down to basics in forecasting, the differences between the systems must tend to disappear. For instance, we find Daniel Bell, in his *The Coming of Post-Industrial Society* (pp. 99ff.) trying to distinguish "three levels of discourse" in Soviet discussions of social development. The first is represented in "the tattered realm of official ideology"; Bell uses passages from Grigori Glezerman's *The Laws of Social Development* to illustrate that formal Soviet theory on future social change is hopelessly inconsistent. On the "scholarly" level, he cites the views of A. M. Rumiantsev, as expressed in his paper, "Social Prognostication and Planning in the Soviet Union," presented in 1970 at the Sixth World Congress of Sociology; Bell suggests that this paper ("remarkable for its freedom from cant") represents the thinking of the managerial communists," whose interest is simply in getting on with the difficult job of forecasting, especially the difficulties of taking into account all of the hard and soft factors that must be considered when trying to forecast the "scientific technological revolution"—about which more later. On the third level—the level of "empirical social researches"—Bell briefly reviews work over the last decade on the occupational divisions in the USSR, pointing out that some Soviet sociologists have begun to question the contemporary usefulness of Lenin's concepts of the class and of the party. Bell appears to believe that on the third level, and probably also on the second, the Soviets are increasingly coming to share Western notions about industrial and postindustrial society, though the continued tyranny of official doctrine makes it impossible for practitioners on these levels to say so, or to trace out the consequences of such beliefs "for Party doctrine and ideological dogma." He dismisses as "fatuous," "popular," and not "serious" the arguments suggesting otherwise by writers like V. Afanasyev and I. V. Bestuzhev-Lada. More important, he shows no inclination to consider the possibility that the three "levels" may actually be of a piece, though he himself provides some good evidence that they are. Thus, Glezerman's theoretical ideas may in fact strike *us* as "confused," but his book does consist of "lectures for postgraduate students at Moscow University and of the Philosophy Department of the Academy of Social Sciences of the Central Committee of the Communist Party." While works later attacked by the party as containing serious theoretical mistakes have

indeed been published throughout the history of the USSR, where might we look to confirm that the party organization of this academy has been called down for tolerating book publication of lectures shot through with recognizable and fatal inconsistencies? (Surely there has been enough time: Glezerman's book was published in 1962—a fact Bell neglects to mention.) Similarly, Rumiantsev may indeed speak of the complexity of social forecasting, but he "also argues that the basic texts of Marxism-Leninism are still correct." Would such an author, or his reviewers, fail to discern an incompatibility of views if one existed? Empirical sociologists have no doubt criticized some of Lenin's conceptual formulations and may even have adopted some notions from the West, but always in the context of the development of the theory of Communism. Is this not one way in which the "science" can grow? And a Bestuzhev-Lada may certainly deliver himself of a popular article in the Party argot. But is this cant not designed to awaken us from our dogmatic slumbers?

[28] But not necessarily all of the probabilities. The pertinent ideas are summed up in this remark by Lenin: "In order to learn to swim one must go into the water. There is no battle in this world where all probabilities are known beforehand" (*Zvezda*, April 1, 1912; quoted in Leites, *op. cit.*, p. 88).

[29] Roy A. Medvedev, *Let History Judge: The Origins and Consequences of Stalinism* (New York: Alfred A. Knopf, 1972), pp. 74–75. The quotation from Wells is from his *Russia in the Shadows* (1921).

[30] Julian Huxley, *A Scientist among the Soviets* (New York: Harper & Brothers Publishers, 1932), pp. 58–59. Like many other works from the same period, this book succeeds in capturing the positive side of theory, intention, and accomplishment in the USSR, but it is extremely naive or ignorant about the negative side. (For example, Huxley remarks on a pleasant rest home at Gorky for workers, but he fails to mention the slave-labor camp nearby in Sormova; he is pleased to note that the maximum penalty for murder is 10 years' imprisonment, but he seem unaware that the then standard sentence for "counter-revolution activity" was 25 years; and so on.)

[31] Hewlett Johnson (*op. cit.*, p. 86) traces the origins of the Five Year Plans to a 1920 letter from Lenin to G. M. Krzhizhanovsky, then chairman of the State Planning Committee (Gosplan), in which he wrote: "Couldn't you produce a plan (not a technical but a political scheme) which would be understood by the proletariat? For instance, in 10 years (or 5?) we shall build 20 (or 30 or 50?) power stations covering the country with a network of such stations, each with a radius of operation of say 400 versts (or 200 if we are unable to achieve more) . . . . We need such a plan at once to give the masses a shining unimpeded prospect to work for." At the 8th Congress of Soviets (December 1920), Lenin made his famous statement: "Without a plan of electrification we cannot tackle the work of actual construction. We need this programme as the first rough draft, to be placed before the whole of Russia, of an economic plan, calculated ahead for at least ten years and showing the way now to give Russia in actual fact the economic basis that is required by Communism . . . . Communism is Soviet government plus the electrification of the whole country." Johnson, along with Soviet propagandists today, quotes H. G. Wells's reaction to this idea: "Lenin, who like other good orthodox Marxist denounces all Utopians, has succumbed at last to a Utopia, the Utopia of the electricians . . . . Can one imagine a more courageous project in a vast flat land of forests and illiterate peasants, with no water power, with no technical skill available, and with trade and industry at the last gasp? . . . I cannot see anything of the sort happening in this dark crystal of Russia, but this little man at the Kremlin can." As Johnson delights in pointing out, Wells was far from being the only Westerner who misunderstood the nature and potential of the regime.

[32] "Prognoses as such are of course interesting, as prognostication in a way uncovers the history of the future. But prognoses without plans are simply pure contemplation, and plans without prior scientific prognoses will inevitably acquire subjective and voluntaristic traits which cannot but affect their efficiency. This is why prognostication is an organic part of the system of socialist planning." [I. V. Bestuzhev-Lada, "Social Prognostics Research in the Soviet Union," in Robert Jungk and Johan Galtung (eds.), *Mankind 2000* (London: Allen and Unwin, 1969), p. 300: hereafter cited as "Social Prognostics Research"] Bestuzhev-Lada is here describing the current belief in the USSR, but it accurately reflects the thinking of the 1920's as well.

[33] I. V. Bestuzhev-Lada, "Bourgeois 'Futurology' and the Future of Mankind," *Political Affairs* (September 1970), p. 37. (Hereafter cited as "Bourgeois Futurology.")

[34] Bestuzhev-Lada, "Bourgeois Futurology," p. 37.

[35] C. G. Strumlin, *Economic Life* (April 2, 1927), as quoted in Y. Belik, "Scientific Forecasting in Long-Term Planning" ("Nauchnoe prognozirovanie v perspektivnom planirovanii"), *Planovoe Khozyaistvo*, No. 5 (May 1973), pp. 28–29. Belik also quotes Stalin as having said, "Our plans are not plan-forecasts; they are plan-directives."

[36] I. V. Bestuzhev-Lada, "How to Forecast the World's Future: Asimov Disputed," *The Current Digest of the Soviet Press*, Vol. 19, No. 20 (June 1967), p. 9. (Hereafter cited as "How To Forecast the World's Future.")

[37] V. V. Nalimov, "From Diagnosis to Forecast," *Znanie-sila*, No. 11 (1972), as quoted in Belik, *op. cit.*, p. 25.

[38] Medvedev, *Let History Judge*, p. 331. This remarkable book, written by a Soviet citizen who considers himself a Marxist-Leninist, details the horrors of Stalinism, and should be read in its entirety for the many insights it provides into the character of institutions and people that believe they have the truth or know what sacrifices are required to achieve it. It is no small matter to observe that the book is intended as an elaboration of the few words quoted here.

[39] Medvedev offers on pp. 102–109 an account of how and with what success the first Five-Year Plan was carried out. He points to Stalin's order, when informed that even the "base-line" goals could not be met, that they be *increased*, and he shows how the data revealed to the world about the accomplishments of the plan were distorted to indicate complete success. Medvedev concludes that because of Stalin's "incompetence," "adventurism," "poor leadership," and "gigantomania," the costs of industrial development were much higher than they would have been "with more rational planning and leadership." He also cites the "just appraisal" offered in the *Soviet Historical Encyclopedia*: "Stalin and his entourage often did not give due consideration to real possibilities; to an excessive degree they intensified tempos of industrial construction that were too high to begin with." While Medvedev and the *Encyclopedia* are certainly right, it should be noted that, on grounds of the operational code, they can be right only in retrospect; as mentioned earlier, the tempo of development toward large goals is taken to be one of the unpredictables.

[40] Report of the 16th Party Congress (1937), as quoted in Belik, *op. cit.*, p. 31.

[41] See Leites and Bernaut, *op. cit.*, especially Chap. 2 ("The Battle of the Forecasts") and Chap. 12 ("Predicting Is Preferring").

[42] V. I. Mezhlauk, the long-time Chairman of Gosplan, was arrested and shot in 1937; his successor, G. I. Smirnov, was executed

later the same year; and Smirnov's successor, N. A. Voznesensky, was removed from office in 1948 and shot in 1950. As an indication that few such victims really knew what was happening, but believed that the problem, whatever it was, would be settled properly, Medvedev (p. 402) tells us that just before Mezhlauk was shot, he was working in his prison cell to complete a manuscript entitled, "On Planning and Ways To Improve It."

[43] One of today's most prominent Soviet authorities on forecasting, V. M. Glushkov, commented a few years ago: "The forecaster is not a science-fiction writer who bears no responsibilities for his prophecies. The forecaster is a participant in the control of science and technology and the entire economy. He answers for the assumptions he mades. [And he] must be ready to assume the responsibility for his recommendation[s]." This means, Glushkov added, that the forecaster is "duty-bound" to report "immediately" any pertinent changes in estimates which may result from his current research. Times are more benign in the Soviet Union, no doubt, but it is difficult not to sense that a message is being conveyed here which goes beyond these words. See V. M. Glushkov, "Modeling the Future," *Nature* (Priroda), No. 1 (January 1969); translated as *Forecasting and Modeling of the Future*, JPRS-47782 (Washington, D.C.: Joint Publications Research Service, April 4, 1969), p. 4. Hereafter cited as "Modeling the Future."

[44] Erich Jantsch, "The Organization of Technological Forecasting in the Soviet Union Notes from a Brief Visit," *Technological Forecasting*, Vol. 1 (1969), p. 83. (Hereafter cited as "TF in the USSR.")

[45] The reasons for this historical coincidence were entirely dissimilar between the two cases, of course, except occasionally on the level of grand exhortation. We shall touch upon some of these reasons in a moment.

[46] I. V. Bestuzhev-Lada, "Social Prognostics Research," pp. 304-305. The subject of the first conference was the kind of future that "awaits mankind"; the second, which was sponsored by Gosplan and the USSR Academy of Sciences, was concerned with methods of economic forecasting. The 1967 conferences he cites dealt with topics much closer to present concerns in Soviet forecasting: problems of social prognostication (April), the city of the future (May), and scientific-technological progress (May).

[47] W. Rolbiecki, "Prognostication and Prognoseology," in Robert Jungk and Johan Galtung (eds.), *op. cit.*, p. 279. Participants at this meeting included representatives from Bulgaria, Czechoslovakia, the German Democratic Republic, Hungary, Poland, Romania, and the USSR, plus "guests from Yugoslavia."

[48] G. M. Dobrov, "Criteria of Selection," *Nature* (Priroda), No. 1 (January 1969), as translated in *Forecasting and Modeling of the Future*, JPRS-47782 (Washington, D.C.: Joint Publications Research Service, April 4, 1969), p. 8. (Hereafter cited as "Criteria of Selection.") This meeting was followed by one in Moscow in January 1968 on the same subject.

[49] Bestuzhev-Lada, "Social Prognostics Research," p. 304. Among the substantive topics considered in these seminars were urban processes, social impacts of public education, leisure and tourism, and disarmament and international security affairs.

[50] Gertrude E. Schroeder, "Recent Developments in Soviet Planning and Incentives," *Soviet Economic Prospects for the Seventies*, Committee Print of the Joint Economic Committee, U.S. Congress (Washington, D.C.: U.S. Government Printing Office, June 27, 1973), p. 13. (Hereafter cited as "Recent Developments.")

[51] Bestuzhev-Lada in "Soviet Prognostics Research" (p. 305) ...ions three such nontechnical works: M. Vasiliev and S. Guschev, *Report from the XXI Century: 39 Soviet Scientists on Science and Technology in the Future*, 2nd ed. (Moscow: Sovetskaya Rossiya, 1963); G. M. Dobrov and A. Golyan-Nikolsky, *The Century of Great Expectations: The Fate of Scientific-Technological Progress in the XX Century* (Kiev: Naukova dumka, 1964); and I. Lada and O. Pisarzhevsky, *Contours of the Future* (Moscow: Znanie, 1965).

[52] Notably *The Future of Science* (Moscow: Znanie, 1966), featuring contributions from authors around the world. Annual editions have since been issued.

[53] For example, G. M. Dobrov, *Science about Science*. dumka, 1966), the final chapter of which provided a survey of methods of forecasting. Later volumes included the anthology, *Forecasting Scientific-Technical Progress* (Moscow: Znanie, 1968), and G M Dobrov, *Prognosticating Science and Technology* (Moscow: Nauka, 1969).

[54] For example, J. Gvishiani and V. A. Lisichkin, *Prognostika* (Moscow, 1968), which is described by Jantsch ("TF in the USSR," p. 85) as "the first Russian book on forecasting techniques." Of greater importance, perhaps, is I. V. Bestuzhev-Lada, *Window into the Future* (Okno v budushchee) (Moscow: Mysl, 1970). Because of its reputation in the West, portions of this book (hereafter cited as *Window*) were translated into English for use in preparing this chapter. Independently, an annotated translation has been made under the direction of Richard Woods at the University of Minnesota

[55] Bestuzhev-Lada, "Social Prognostics Research," p. 305.

[56] Bestuzhev-Lada, "How To Forecast the World's Future," p. 10.

[57] S. S. Strelkov, "Studies of the Problems of Economic Forecasting" ("Nauchnye issledovania po problemam ekonomicheskogo prognozirovania"), *Izvestia AN SSSR Seria Ekonomicheskaya*, No. 2 (1972), pp. 135-136.

[58] Readers familiar with Western publications may also conclude from these figures that Soviet specialists are much better informed about Western work on forecasting than Westerners are about work in the USSR. This should not be surprising, given the importance of the role in Soviet forecasting of studying history (mentioned earlier) and the blithesome tendency of Western futures researchers to ignore the need for scholarship.

[59] R. Richta, et al., *Civilization at the Crossroads* (Prague: International Arts and Science Press, 1969). This study was completed in 1966, during the period of liberalization that came to an end with the Russian invasion of 1968; the bibliography was extended in the course of preparing the English translation.

[60] Schroeder, "Recent Developments," p. 15.

[61] As quoted in Bestuzhev-Lada, "Bourgeois Futurology," p. 50.

[62] I. V. Bestuzhev-Lada, "Social Forecasting," *Soviet Cybernetics Review*, Vol. 3, No. 5 (May 1969), pp. 55-59.

[63] Bestuzhev-Lada, "Social Forecasting," p. 57, and "Social Prognostics Research," p. 305. In his 1967 article, "How To Forecast the World's Future," Bestuzhev-Lada writes, "According to my strictly personal computations, more than 30 sectors and research groups in Moscow, each with an average staff of five to ten, are engaged directly or indirectly in problems of social prediction. There are also futurology specialists in other cities of our country . . . But all these research groups have not yet managed to gladden us with an abundance of scientific output" (p. 10). Names of some of the organizations involved are given in these sources, as well as in Jantsch, "TF in the USSR," and in the articles by Agnew, Gogol, Hassel, Leber,

Schroeder, and others included in the bibliography at the end of this chapter. Most of them are institutes or departments within the USSR Academy of Sciences or the SSR Academy of Science of individual Republics; additionally, as many as 60 "temporary commissions" have been established to make forecasts in special areas of interest.

[64] As quoted by the editors of the magazine *Priroda (Nature)* in introducing a special series of articles on forecasting, published in January 1969.

[65] I. I. Artobolevskij and S. V. Shukhardin, "Industry and Production in the Year 2000," *Soviet Cybernetics Review* (January 1970), p. 69. See also Bestuzhev-Lada, "Bourgeois Futurology," p. 50.

[66] V. G. Felzenbaum, "Measurement of the Economic Effectiveness of Alternatives in Forecasting Scientific and Technological Progress" ("Izmerenie ekonomicheskoi effektivnosti alternativ nauchno-tekhnicheskogo progressa pri prognozirovanii"), *Izvestia AN SSSR Seria Ekonomicheskaya*, No. 3 (1973), p. 40. (Hereafter cited as "Measurement of Economic Effectiveness.")

[67] G. G. Ivanov and A. I. Marek, "Certain Methodological Questions of the Interrelations of Economic Development Forecasting and the Planning of Scientific Studies" ("Nekotorye metodologicheskie voprosy vzaimosvyazei prognozirovania razvitia ekanomiki i planirovania nauchnykh issledovanii"), *Izvestia AN SSSR Seria Ekonomicheskaya*, No. 2, (1973), p. 85. (Hereafter cited as "Methodological Questions.")

[68] Bestuzhev-Lada, "Bourgeois Futurology," pp. 38 and 50. In "Social Forecasting" (p. 63), he asserts that the basic reason for newer approaches to forecasting is to "solve the problems set before us by the Communist Party and the government." Something of the same answer has also been expressed in this way: "To make scientific and technical forecasting work for the cause of Communist development is to utilize more effectively the basic advantages of our social system, as determined by the planned system of management and a unified scientific and technical policy." See V. M. Glushkov and G. M. Dobrov, "Scientific Prognosis," *Izvestiya* (July 10, 1968); translated as *Scientific and Technical Forecasting Seen Vital to Soviet Progress*, JPRS-46113 (Washington, D.C.: Joint Publications Research Service, August 6, 1968). (Hereafter cited as "Scientific Prognosis.")

[69] The fears of the political and military leadership about Khrushchev's incompetence in international affairs provide the theme of Oleg Penkovskiy's *The Penkovskiy Papers* (Garden City, N.Y.: Doubleday and Company, Inc., 1965), which was presumably written by him in 1961–62. According to this book, Penkovskiy's own fears, as an army colonel assigned to the Chief Intelligence Directorate (GRU) of the Soviet General Staff, led him to become an espionage agent for the West. These activities were uncovered by the Committee for State Security (KGB) in 1962, and Penkovskiy is reported to have been shot in 1963. (It should be noted that charges have been made recently that both the book and the man are CIA fabrications.) In any event, it appears that the KGB did play a key role in helping to turn Khrushchev out of office.

[70] Alex Nove, "Prospects for Econ. nic Growth in the USSR," in Morris Bornstein and Daniel R. Fus eld (eds.), *The Soviet Economy* (Homewood, Ill.: Richard D. Irwin, 1966), p. 318; as quoted in Gary North, "The Crisis in Soviet Economic Planning," *Modern Age*, Vol. 14, No. 1 (Winter 1969–70), pp. 54–55. (North's article, which [illegible]gs together many scholarly sources, will hereafter be cited as [illegible]sis in Planning.") Cf. also A. S. Becker, *The Future of Input-Output in Soviet Planning*, Paper P-3206 (Santa Monica, Calif.: The Rand Corporation, August 1965), pp. 5, 10–11, and 17.

[71] North, "Crisis in Planning," p. 51; these estimates were paraphrased by North from Leon Smolinski, "What Next in Soviet Planning?" *Foreign Affairs*, Vol. XLII, No. 4 (July 1964), pp. 602–613.

[72] Penkovskiy, *op. cit.*, p. 208. Penkovskiy continues in high scorn: "How long have we been overtaking America? We were overtaking America under Stalin; and now under Khrushchev we are still overtaking America. The fool. At least he could try to say something sensible. Of course, he personally has overtaken quite a bit: he has three country houses near Moscow, several in the Caucasus. . . ." (pp. 208–209).

[73] Schroeder, "Recent Developments," pp. 14–15.

[74] See pp. 159–160. Incidentally, other Soviet specialists have maintained that this category of forecasts "properly should be interpreted as [a] subsystem" of socio-economic forecasts; see G. M. Dobrov and L. P. Smirnov, "Forecasting as a Means for Scientific and Technological Policy Control," *Technological Forecasting and Social Change*, Vol. 4, No. 1 (1972), p. 7. (Hereafter cited as "Forecasting for Policy Control.") In fact, this subordination probably gives a more accurate insight into the emphasis placed upon science and technology as key forces of production in the Soviet economy.

[75] Note that the entry, "moral and ethical developments," at the bottom of the left-hand branch for "socio-economic prognostication" is followed by a question mark. Bestuzhev-Lada says that this subject should "probably" be included here, though very little work has actually been done—a point that he repeats in *Window* (pp. 146 and 197–198). [Cf. F. C. Ikle, "Social Forecasting and the Problem of Changing Values," *Futures*, Vol. 3, No. 2 (June 1971), pp. 142–150.] It might be mentioned that neglect of values has not been as important an issue in Communist forecasting as it has in the West, since the doctrine holds that moral and ethical considerations are emphatically not central in personal or social development. Because all values are class values, and values change as a result of changes in material conditions, the inevitable victory of Communism entails that the only values which can and will matter are those of the proletariat. The accepted view was expressed in 1951 by the Soviet philosopher, P. A. Sharia, in these words: "The founders of Marxism had no need to separate out a special science of ethics, since the scientific theory of social development they created already provided a scientific foundation for morality, too, as one of the forms of social consciousness . . . . One must not confuse two things: the *basing* of socialism on ethics, which the classics of Marxism-Leninism attacked bitterly, and the ethical nature of Marxism itself, as the most progressive, scientific world-view, striving toward incessant progress, towards the liberation of exploited and suffering humanity." Quoted in Eugene Kamenka, "Marx, Marxism, and Ethics," *Soviet Survey*, No. 35 (January–March 1961), p. 109. See also Howard Selsam, *Socialism and Ethics* (New York: International Publishers, 1943), especially Chap. III.

[76] Bestuzhev-Lada, *Window*, pp. 15ff.

[77] Cf. Y. Ozhegov, "The Struggle for the Future of Mankind" ("Borba za budushchee chelovechestva"), *Mirovaya ekonomika i mezhdunarodnye otnoshenia*, No. 9 (September 1972), pp. 26–36, wherein bourgeois social forecasters are divided into three groups: (1) those who admit the possibility of social forecasting, (2) those who deny it, and (3) those who "subjectively limit" but nevertheless admit the possibility in certain areas. Ozhegov's discussion makes all three groups the servants of a corrupt and dying social system, struggling vainly against the inevitable. Lacking "an objective historical

perspective," however, they cannot correctly appraise the underlying "ideological crisis of Western futurology," and consequently cannot produce adequately scientific forecasts. Among the authors whose works are reviewed in this context are Aron, Bell, Bright, Helmer, Kahn and Wiener, and Polak.

78 This is not to minimize, however, the value or influence among the Soviets of the conventional disciplines as approaches to a study of the future which can successfully navigate between the Scylla of Party tradition and the Charybdis of received doctrine. A book like George Fischer (ed.), *Science and Ideology in Soviet Society* (New York: Atherton Press, 1967), while not directly concerned with forecasting, makes plain the large role played by each of the disciplines in shaping specialized awareness of the issues at stake and of the problems yet to be resolved. In the opening chapter, Fischer (pp. 1–46) discusses recent developments in sociology, highlighting the differences between trends toward research involving a "future" orientation and research requiring a "concrete" orientation. (Interestingly, he finds the principal contrasts to be that the sociologists who have adopted the "future" strategy are much less rigorous in methodology, much more casual in their choice and use of souce material, much more concerned about "the central institutions of the society," much more likely to base conclusions on deductions from first-principles, and infinitely more prone to "talk of today's society as no more than a passing way station en route to an ideal though distant future.") In the same volume, Richard T. De George considers Soviet philosophy (pp. 46–81), arguing, among other things, that "practical necessity has forced Soviet ideologists [and philosophers] to begin to do what all the anti-Marxist critiques of the Marxist interpretation of history could not force them to do. It has forced them to analyze and revise some of their prophetic claims and to start on an empirical study of [the laws of] their own developing society. [But it must be emphasized that this] work and clarification is taking place within . . . Marxist-Leninist bounds" (pp. 78–79). Folowing a review by Loren R. Graham of cybernetics (a subject that will be considered briefly in a moment), there is also a paper by Herbert S. Levine on developments in economics (pp. 107–138), which shows how Soviet expectations about the use of mathematical and computer techniques in economics have been tempered by theoretical, technical, administrative, and political realities—all of which can be traced, to some extent, to Soviet conceptions about the future and how it can best be planned.

79 See footnote 84 below for an elaboration of this point. Strikingly different definitions have been offered by some forecasters in Eastern Europe. For example, Polish author Waldemar Rolbiecki characterizes prognostication as "the activity of man consisting in foresight and forecasting"; he recommends the term "prognoselogy" to denote the science concerned with the methodology, psychology, sociology, and philosophy of the prognostic process (*op. cit.*, pp. 278, 282–285). He explicitly rules out the possibility that forecasting itself can be a science, at least in any conventional sense (p. 279).

80 E. Manncharova and V. Yankulin, as paraphrased in Irene Agnew, "Ukraine Coordinates Economic Cybernetics Work," *Soviet Cybernetics Review* (July 1970), p. 35.

81 G. S. Pospelov, "Control System Design Theory," *Soviet Cybernetics Review* (November 1970), p. 24.

82 Agnew, *op. cit.*, p. 35 (italics added). As such, of course, OR must be relevant to all types of plans, programs, or projects—a rather fanciful notion, if only because the analyst will certainly have to defer at some point to the beliefs of the Party leadership. Nonethe[illegible]here seems to be a tendency to try to enlarge the scope of the OR approach in the Soviet Union. A harbinger of this tendency may be found in the writings of Romanian Pavel Apostol, who has formally called for "an Extended Operations Research (EOR)," which he sees as OR applied to complete systems of "social action," such as economic, military, or cultural affairs. (See Apostol, *op. cit.*, p. 209.)

83 Agnew, *op. cit.*, p. 35.

84 In Bestuzhev-Lada's opinion (*Windows*, pp. 111ff.) prognostics, operations research, and decision theory are "links in a chain of approaches to problems of the future." Diagramatically, he represents this chain as shown in the diagram, where the top level indicates the activity to be performed, the second level, the output of the activity; and the third level, the approach that governs and is defined by the activities and outputs.

85 Loren R. Graham, "Cybernetics," in George Fischer (ed.), *op. cit.*, p 90. Graham's paper traces out the rise of cybernetics in the Soviet Union, summarizes the internal debate about the relationship between cybernetics and the doctrine, explains in detail why the appearance of this new science has been greeted with renewed hope about the possibility of making Communism "work," and attempts to evaluate its lasting contributions to Soviet planning.

86 Graham accurately forecasted this decline in enthusiasm, pointing especially to major theoretical and philosophical obstacles to complete success.

87 Moiseev, for one, has said that "until now, we have been unable to understand fully what is meant by 'systems analysis,' although we use this term, because to us it means something directly concerned with physics"—apparently, in that it involves the construction of models. [See N. Moissev, "The Present State of Futures Research in the Soviet Union," in Nigel Hawkes (ed.), *International Seminar on Trends in Mathematical Modelling*, Venice, December 13–18, 1971 (New York: Springer-Verlag, 1973), p. 14. Hereafter cited as "Futures Research in the USSR."] In contrast, systems analysis has been equated with "the scientific spirit" by D. M. Gvishiani [see G. M Dobrov, "Technology Assessment in the USSR or the Science of Science: A Basis for the Effective Management of Scientific Activity," *Technology Assessment*, Vol. 1, No. 3 (1973), p. 192]. A simpler and perhaps even more useful definition, for Soviet purposes, has been given by Pospelov, who asserts that "in practice" systems analysis is merely "the procedure of making decisions. It is concerned with constructing a picture of the future and a formulation of goals and tasks. The procedure makes clear what action must be taken in each of the given situations and how." [G. S. Pospelov, "Control System Design Theory," *Soviet Cybernetics Review* (November 1970), p. 24.]

88 Cf. the discussion of these points in Chap. 1.

89 These advantages have not as yet been sensed in other Communist countries. For example, "the systems approach, with its profound concern for the underlying political processes, can hardly be discussed in Romania. The alleged 'objectivity' of the 'science' of forecasting is used as a pretext to avoid any consideration of political processes. [At the same time, however,] the word 'system' is considered a 'good' word in Romania—as is 'cybernetics' in all Communist countries—and is used in all possible and also impossible contexts." Erich Jantsch, "The Organization of Forecasting in Romania: Notes from a Brief Visit," *Technological Forecasting and Social Change*, Vol. 4, No. 1 (1972), pp. 21–22.

90 Schroeder, "Recent D[illegible]ments," p. 30.

91 N. Fedorenko, "On the [illegible]boration of a System of Optimal Functioning of the Socialist Economy," *Problems of Economics*,

Vol. 15, No. 9 (January 1973), pp. 9–12. Hereafter cited as "Elaboration of SOEF.") In these pages, incidentally, Fedorenko also refers explicitly to the need of assuming that "basic characteristics in the dynamics of the national economy—volume of branch output, limits on capital investment, etc.—were already established" (i.e., given by the party).

92 V. A. Lisichkin, "The Process of Making Forecasts," *Technological Forecasting* 1 (1969), p. 103. (Hereafter cited as "Process of Making Forecasts.")

93 Bestuzhev-Lada, "Social Prognostics Research," p. 303.

94 Glushkov and Dobrov, "Scientific Prognosis," p. 3. By 1972, this number had somehow increased to 130. See Dobrov and Smirnov, "Forecasting for Policy Control," p. 10.

95 Bestuzhev-Lada, *Window*, pp. 66–67.

96 Observe, however, that additions are allowed for only in the realms of "General Scientific Methods" and "Specific Scientific Methods."

97 PATTERN and Delphi are rarely mentioned by name in the Soviet literature, except in passing, but "hierarchical trees" or "goal trees" and "expert opinion" techniques are often cited. No doubt most references to "trees" of one sort or another are actually references to relevance trees, PATTERN being an archetypical instance. Similarly, "expert opinion" may mean Delphi—but this is more problematical, since the Soviets seem determined to invent their own version of the technique, one that is more in keeping with the nature and aims of forecasting in the Soviet Union—a point discussed further below.

98 A. Becker, whose *The Future of Input-Output in Soviet Planning* we paraphrase here, cites the basic Soviet papers.

99 A good summary of these matters will be found in H. S. Levine, "Economics," in Fischer (ed.), *op. cit.*, pp. 123–128, and in Schroeder, "Recent Developments," pp. 22–29.

100 Schroeder, "Recent Developments," p. 24.

101 K. A. Bagrinovsky, "A System of Models for Central Planning," in Nigel Hawkes (ed.), *International Seminar on Trends in Mathematical Modelling*, Venice, December 13–18, 1971 (New York: Springer-Verlag, 1973), pp. 21–30.

102 A. A. Arakelyan and V. S. Dadayan, "Experience of Model Usage in Long-Term Economic Forecast" ("Opyt [illegible]nomiko-matematicheskogo modelirovania v dolgosrochnom p[illegible]ozirovanii"), *Vestnik AN SSSR*, No. 2 (1973), pp. 58–64.

103 S. Shatalin, "New Methods, New Means," *Current Digest of the Soviet Press*, Vol. 24, No. 29 (August 16, 1972), p. 8.

104 Federenko, "Elaboration of SOEF," p. 16. In this connection it might be mentioned that a considerable bureaucracy exists for promoting mathematical-economic contributions to the national program, for example, in 1968 Gosplan established a Department for the Introduction of Economic-Mathematical Methods in Planning. Constant pressure is also being applied by the top leadership. At the 24th Party Congress, for example, Brezhnev declared that "wider use of these methods [i.e., "economico-mathematical modeling, systems analysis, and so on"] must be made, and sectoral automated management systems must be created more rapidly"; Kosygin insisted that "it is the duty of the economic executives to learn the new management techniques based on a thorough knowledge of Marxist-Leninist theory" (*24th Party Congress Documents*, pp. 81,

105 A. Klinski, as quoted in H. S. Levine, in Fischer (ed.), *op. cit.*, p. 123. Many of the details in this review of I/O are based on Levine's essay, as well as Schroeder's paper.

106 A. Klinski, in Levine, in Fischer (ed.), *op. cit.*, p. 123.

107 Probably a most distressing outcome, especially if, as Becker infers (*op. cit.*, p. 2), "several hundred research and project-making institutions" participated in the effort.

108 Quoted from *Problems of Economics*, No. 2 (1968), p. 20, by Schroeder, "Recent Developments," p. 27, who adds, "Probably, the same statement could be made by someone writing in 1973."

109 As quoted in Schroeder, "Recent Developments," p. 28.

110 Soviet awareness of the Gordon-Helmer study and of the Delphi technique generally has been very current, as indicated in part by the fact that all references to the Gordon-Helmer work cite the original English version published by The Rand Corporation. Importantly, these references also suggest that the Soviets, like most others around the world who commented on the study, failed to see that it was not so much a forecast as an experiment in forecasting, and thus have tended to consider only its apparently substantive findings.

111 Cf. Bestuzhev-Lada's complaint in 1967 ("How To Forecast the World's Future," p. 10) that "theoretically, every science must engage in prediction in its own field. But try, for example, to force the Academy of Pedagogy to examine the problems of education of the next century or even the next few decades! Or the Academy of Agricultural Sciences the problems of agriculture in the 1970's or 1980's. They will be lucky if they cope with the present five-year plan."

112 I. Ehin, "Some Methodological Problems of Forecasting," *Transactions AS Estonian SSR*, Vol. 20, No. 4 (1971), p. 369, explicitly recommends Delphi and PATTERN for this purpose. (Ehin's paper will hereafter be cited as "Some Methodological Problems.")

113 To judge by the forecasting literature, Soviet forecasters make regular use of probabilistic inputs and computations, particularly in mathematical-economic model-building. But they are usually reticent and always vague when it comes to discussing the role of probability in their analyses—and perhaps this is understandable, in view of the ideological constraints reviewed earlier. As an indication of how even an expert can be perplexing on this score, consider carefully the possible meaning of this statement: "The stream of information on new discoveries, ideas, and inventions . . . is very copious. We know in advance that not everything contained there will be realized. In other words, we have information not about a single version of the future, but about its many versions. And the forecaster will accomplish the mission before him if he is able to determine, or foretell, just which version will be realized; and such a forecast is always made with some degree of probability." (Dobrov, "Criteria of Selection," p. 6.)

114 U. A. Zykov, "On Certain Principles of the Economic Forecasting of Scientific and Technological Progress" ("O [illegible]orykh printsipakh ekonomicheskogo prognozirovani[illegible]uchno-teknicheskogo progressa"), *Izvestia AN SSSR Seria Ek[illegible]omicheskaya*, No. 3 (1973), p. 50.

115 Zykov, *op. cit.*, p. 50.

116 Dobrov and Smirnov, "Forecasting for Policy Control," p. 10.

117 Ehin, "Some Methodological Problems," pp. 368–369. Following the suggestions of the American W. Gilman, Ehin declares further that "to find and develop a larger number of non-conforming

scientists . . is an important task Such non-conforming personalities may counterbalance the determining influence (or rigidity) created by [conventional] predictions and plans" (p. 369).

118 Moiseev, "Futures Research in the USSR," p. 15.

119 Dobrov, "Criteria of Selection," p. 9.

120 This point is especially emphasized by Moiseev in "Futures Research in the USSR," pp. 14–15 and 17. On the other hand, Bestuzhev-Lada (in "How To Forecast the World's Future," p. 9) has said that in U.S. Delphi studies "a large degree of objectivity has ensured a large degree of reliability in the predictions"—at least in gathering forecasts based on "patterns of present-day social thought among scientists"!

121 Dobrov, "Criteria of Selection," p. 9. (Soviet forecasters, like their Western counterparts, have not worried overmuch about the infinite regress implied by this counsel, the Soviets, at least, can take comfort in knowing that it would lead eventually to the certainties in the doctrine.) Another suggestion that has been made, but apparently not yet acted upon, for increasing the reliability of expert opinion techniques, would be to avoid one-shot investigations and develop a "continuous system" for tapping the minds of specialists. This would provide the wherewithal for making comparisons, uncovering basic problems, and so on. This idea has been urged by V. M. Glushkov (*op. cit.*, p. 3), Dobrov and Smirnov ("Forecasting for Policy Control," p. 18), and others.

122 See, for example, U. Ennuste, "On Prediction Means and Variations of an Optimal Planning Problem" ("O prognozirovanii srednikh znachenii i dispersii parametrov zadach optimalnogo planirovania"), *Transactions AS Estonian SSR*, Vol. 21, No. 3 (1972), pp. 245–252.

123 Strictly computer simulations are included among the mathematical-economic methods. But "simulation" as used here is intended to refer to the approach taken when the system being investigated either is still in the process of development or is so complex that it cannot be reduced to a set of equations. In these cases, simulation models are used to "unite the method of expertise with the method of mathematical modeling One might say, for instance, that the method of mathematical models allows us to create a chess board, and the method of expertise allows the players to show capabilities which they possess, but which the computer does not." In "simulation" the two are joined (Moiseev, "Futures Research in the USSR," pp. 15–16.)

124 In his extensive survey of reports on Delphi, Murray Turoff has found only one published Soviet paper. G. M. Dobrov and L. Smirnov, "Forecast as a Means for Scientific and Technological Policy Control," which was presented at the Seminar on Technological Forecasting, United Nations Economic Commission for Europe, Warsaw, Poland, December 1970. While it would be easy to argue that the lack of Delphi studies in the Soviet Union (particularly in view of the many references) is attributable to the fact that Delphi requires anonymity and open-endedness, the explanation may well be much simpler: it is an import from the United States.

125 Western forecasters may be surprised to learn that polls about future states of affairs have been conducted not only among the experts, but also among the public (An example was given earlier in fn 14) The number is not significant nor is it in the United States, for that matter but it does represent a break from the attitude common among the leadership in the 1960's This attitude was epitomized, perhaps, in a 1958 speech by Khrushchev when he asserted that if the USSR were to take issues like "immediate discontinuation of nuclear tests [and] complete rejection of the use of nuclear weapons . . . and hold a poll on them among the population, they are sure to be backed by an absolute majority. This is beyond doubt . . ."—and, hence, there is no need to hold the poll. [N. S. Khrushchev, *For Victory in Peaceful Competition with Capitalism* (New York: E. P. Dutton and Co., 1960), p. 590.] Gradually (and probably reluctantly) it has come to be recognized that on some issues the "experts" are the people. The point is nowhere better exemplified than in the area of forecasts of consumer demand, where mathematical-economic approaches have proven less than satisfactory and are now being augmented by the results of public polls on desires, trends, and preferences. (Details are given in the papers by Budnik, Gogol, Stanev, and others cited in the bibliography.)

126 The first term appears in Glushkov and Dobrov, "Scientific Prognosis," p 4; the second, in Dobrov and Smirnov, "Forecasting for Policy Control," *passim* The description that follows is based on Dobrov and Smirnov's discussion on pp. 11 18, omitting the computational steps used for analyzing the data.

127 SPRITE is Bell Canada's version of the Delphi technique, distinctive in that it involves interaction between different panels of respondents (e.g., "experts" vs. "laymen") during the evaluation of the same set of issues.

128 Dobrov and Smirnov, "Forecasting for Policy Control," p. 18.

129 L. Brezhnev, *24th Party Congress Documents*, p. 69.

130 Cf. the items by N. V. Markov and I. A. Andreev in the bibliography. N. N. Semonov, in his "The World of the Future," [*Bulletin of the Atomic Scientists*, Vol. 20, No. 2 (February 1964), p. 10], points out that it was Khrushchev who, at the 22nd Party Congress (1961), observed that "science is becoming more and more a direct force of production, and production itself is becoming the technological application of modern science."

131 R. Richta et al., "The Perspective of the Scientific and Technological Revolution," in Robert Jungk and Johan Galtung (eds.), *op. cit.*, p. 200. Richta and his colleagues in Czechoslovakia were among the first students of prognostics to look closely at the nature and ramifications of the scientific-technical revolution; they were also among the first to declare that this revolution would intrinsically operate to the advantage of the Communist countries, rather than the imperialists. [This view has since found widespread expression. See, for example, *24th Party Congress Documents*, pp. 147 148, and Bestuzhev-Lada, "Bourgeois Forecasting," wherein he asserts that "the socioeconomic effects of the scientific and technological revolution as they appear in the light of modern forecasting data are in irreconcilable contradiction with the further existence of the capitalist mode of production" (p 49).] Finally, they were the first to make clear that the revolution could not be equated to what others have called the "post-industrial" transformation in part because the service sector would eventually give way to the influence of automation and better management, in part because the scientific-technical revolution would have the building of the new man at its heart: man would not be the final and almost incidental beneficiary of developments taking place somewhere out there, in "society."

132 *24th Party Congress Documents*, p. 224.

133 Here, as elsewhere, it is well to recall that global perceptions about the future as enunciated by the party leadership are to be taken on all levels of society as truths, and their implications, as orders. From the analyst and planner's point of view especially "it is wise not to lose sight of national economic good in the name of . tability of individual enterprises, for it is what is good for the national economy that is ultimately good for the individual enterprises." [V G. Felzenbaum, "Measurement of the Economic Effectiveness of Alternatives

in Forecasting Scientific and Technological Progress" ("Izmerenie ekonomicheskoi effektivnosti alternativ nauchno-teknicheskogo progressa pri prognozirovanii", *Izvestia AN SSSR Seria Ekonomicheskaya*, No. 3 (1973), p. 63.] One should compare the converse of this: "What is good for General Motors is good for the United States."

[134] Glushkov and Dobrov, "Scientific Prognoses," pp 2, 3, and 5

[135] Zykov, *op. cit.*

[136] These attributes will speak for themselves, except perhaps for "conditional" (which simply means that the forecast takes an "if then" form) and "noncontrollable-controllable" (which is intended to indicate whether the condition the "if" – is within man's control).

[137] Lisichkin, "Process of Making Forecasts," pp. 103 and 104. The list of stages and steps that follows is from this paper.

[138] Schroeder, "Recent Developments," p. 16.

[139] It has been reported that Stalin used an astrologer named Yuri Yamakin for a time, that Stalin eventually sent him to a labor camp, and that after Stalin's death, Khrushchev brought Yamakin back and gave him "an official suite at the Kremlin." [Herbert B. Greenhouse, *Premonitions. A Leap into the Future* (New York. Warner Paperback Library, 1973), p. 115.] This is interesting and perhaps even true, but it is also irrelevant. Until Khrushchev was deposed, the official view of parapsychology and any of its kin was not only negative but hostile. For example, the *Soviet Encyclopedia* in 1956 characterized telepathy as "an antisocial, idealistic fiction about man's supernatural power to perceive phenomena which, considering the time and the place, cannot be perceived." Sustained *and open* scientific research did not begin until about 1965. The record of Soviet papers and reports on parapsychological investigations sums up the matter: according to one Soviet source, in 1957 three papers appeared, all of which were negative; in 1967, there were 152 papers, of which only 15 were negative. See Sheila Ostrander and Lynn Schroeder, *Psychic Discoveries behind the Iron Curtain* (New York: Bantam Books, 1970), pp. 20 and 141. This fascinating book is the source of the details in the text.

[140] Quoted in Ostrander and Schroeder, *op. cit.*, p. 64.

[141] In a statement that would have to be considered roughly equivalent to an endorsement of Jeane Dixon by the Deputy Director of the U.S. National Science Foundation, Nikolai Semyonov of the USSR Academy of Sciences said in 1966 that "it is very important to scientifically study the psychic phenomena of sensitives like Wolf Messing." (Quoted in Ostrander and Schroeder, *op. cit*, p 44) The remarkable Messing, who has the unique distinction of having been tested by Stalin, Gandhi, Einstein, and Freud, believes that there is nothing supernatural about his abilities and that, however they may ultimately be explained, nothing will be found that is inconsistent with dialectical materialism

[142] A case in point is provided by the experience of Anatoly Vitalyevich Dyakov, a trained meterologist stationed at the Kuznetsk Metallurgical Combine in Western Siberia In his spare time, Dyakov makes long-range weather forecasts, ten days to three or more months ahead, strictly on the basis of his observations of solar activity, which he has been studying for 35 years. His forecasts are widely requested because their accuracy ranges somewhere between 80 and 90%. In contrast, the "scientific" forecasts made by the official Hydrometeorological Center in Western Siberia average about 60% accurate. Nevertheless, the weather service officials at the center have denounced Dyakov's work as "charlatanism, not science," and have "certified [him] as a sorcerer." As a result, he is unable to get his scientific papers published. *Pravda*, at least, has now come to his defense, see Y. Chernichenko, "Weather for the Whole Summer," *Current Digest of the Soviet Press*, Vol. 24, No. 39 (October 25, 1972), pp. 20-21.

[143] I. V. Bestuzhev-Lada, "Systems Analysis and the Prognosis of Social Needs of the Youth," as quoted in H. A. Linstone, "The Third World Future Research Conference," *Technological Forecasting and Social Change*, Vol. 4, No. 3 (1973), p. 241. In a paper given at the Seventh World Congress of Sociology in 1970, Hungarian Gyorgy Fukasz expressed the idea in fewer words. "Scientific futurology is in fact the theory of Communism." As quoted in K. Kuman, "Futurology The View from Eastern Europe (Review of R. Richta's *Civilization at the Crossroads*)," *Futures*, Vol. 4, No. 1 (March 1972), p. 91.

[144] Bestuzhev-Lada, "Bourgeois Futurology," p. 39.

[145] Bestuzhev-Lada, "Social Forecasting," p. 59.

[146] "Of course, there can be no compromise with views and pronouncements hostile to Marxism . . . . It must be remembered firmly that we are for peaceful co-existence of states with diverse social systems, but against peaceful co-existence of ideologies, since that would mean ideological disarmament." *Kommunist*, No. 10 (August 1956), as quoted in Harry and Bonaro Overstreet, *What We Must Know about Communism* (New York. Pocket Books, Inc., 1958), p. 109. These words appeared in a rather different context (as advice to Soviet intellectuals generally), but there is no indication in the Soviet forecasting literature that thinking on this matter diverges in any way. And time has not had the least effect on the 1956 position, thus, *Pravda* declared baldly on April 2, 1974, that "there can be no talk of any ideological coexistence or even ideological armistice."

[147] "Economic Institute Is Taken to Task," p. 1.

[148] Apostol, "Marxism and the Structure of the Future," p. 202.

[149] Eugeniusz Olszewski and Lech Zacher, "Political Implications and the Role of Government in the Planning of Technological Future in the Socialist Countries," *Anticipation*, No. 15 (December 1973), p. 34.

[150] Bestuzhev-Lada, "How To Forecast the World's Future," p. 10.

[151] An example is provided in Y. A. Chizhov, "Why Economic Forecasts are Being Reviewed" ("Pochemu peresmatrivayutsya ekonomicheskie prognozy?"), *S.Sh.A.*, No. 6 (June 1973), pp. 20-29. This paper reviews recent U.S. forecasts of the American economy, and concludes that the errors were not due so much to the failings of the models used, but rather to the inaccuracy of government statistics and misleading budgetary information.

[152] Exceptions may be found in authors like Bell, op. cit., and Ikle, *op. cit.*

[153] The optimism among Westerners has been mentioned often. See, for example, Irene Traviss, "Futurology and the Problem of Values," *International Social Science Journal*, Vol. XXI, No. 4 (1969), pp. 580-582.

[154] V. N. Siforov, "The Philosophy of Long-Term Forecasts," *Priroda* (Nature), No. 1 (January 1969), translated in *Forecasting and Modeling of the Future*, JPRS 47782 (Washington, D.C.. Joint Publications Reference Service, April 4, 1969), p. 12.

[155] I. I. Artobolevskij and S. V Shukhardin, *op cit*, p. 68

[156] We would note that Soviet specialists were among the respondents to the survey reported in the appendix to this volume

[157] Bestuzhev-Lada, "Social Prognostics Research," pp. 305–306. Compare the opinion of Daniil Granin: "Modern natural sciences, modern technology require the convening of international symposiums, the creation of permanent international committees, institutes and organizations, the participation of different countries and of whole continents. Such a 'collectivization' of resources, minds, and communications will undoubtedly extend and attempts to oppose it are historically doomed" (in Cranin, *op. cit.*, pp. 28–29).

[158] Fred Polak, *The Image of the Future* (New York: Elsevier Scientific Publishing Company, 1973), pp. 303–304.

Part III

Professional Issues

Chapter 13

The Futures Field: Functions, Forms, and Critical Issues

Roy Amara

Form Follows Function

Form should follow function in structuring the futures field[1] as surely as it should in designing physical structures. Thus an analysis of institutional form is meaningless unless it is preceded or accompanied by a clear image of what the futures field is and what purposes it is to serve.

It is not the purpose here to dwell on the variety of definitions that have been used to characterize the futures field.[2] The two that seem to come closest to capturing the essence of what is admittedly still an activity being shaped by its practitioners are

1. Any systematic attempt to improve our understanding of the future consequences of present developments and choices.
2. Any efforts to systematize our assumptions and perceptions about the future. These fall into three categories: the exploration of *possible* futures (the art of futurism), the exploration of *probable* futures (the science of futurism), and the exploration of *preferable* futures (the politics and psychology of futurism).[3]

Although such definitions are useful in improving communication and in providing frames of common reference, they can shed only limited light on the functions of the futures field. To explore such functions more directly, it is instructive to probe briefly into the origins of the futures field.

An interesting digression might be made at this point tracing the roots of the futures field to a variety of eighteenth or nineteenth century utopians, social reformers, essayists, technological forecasters, or perhaps even to ancient Grecian oracles or soothsayers. Interesting as this might be, it would not shed nearly as much light on the functions of the futures field as it would if we limited our purview to more recent history—say, of the last two or three decades.

Within that period three important and quite distinct components bearing on the origins and functions of the futures field have emerged. The first is an image-forming and goal-oriented component, with visionary and conceptual roots, aimed at extending the range and richness of images of the future. A second is an analytically and methodologically based component, with scientific and technological roots, aimed basically at enhancing intentionality in human affairs. And the third is a grass-roots and action-oriented component, with political and sociological roots, directed at advancing participatory social planning. These three components correspond approximately to the possible, probable, and preferable aspects of the futures field contained in the second definition presented earlier.

The image-forming or goal-oriented roots of the futures field are the most difficult to trace and describe. In part, this is because such roots are not uniquely tied to the futures field and in part because they often lead to strong charismatic individuals or seminal thinkers with imaginative and, at times, unorthodox images of the future. Such individuals almost defy cataloguing and, in fact, many might not identify with the futures field at all. They may be described as modern-day social evolutionaries, ecological futurists, social activists, science fiction writers, and so forth. The common threads binding them are visionary perceptions of possible futures. Sometimes these perceptions are accompanied with useful clues about how such futures may be realized. Sometimes these images are utopian, sometimes antiutopian. The crucially important function fulfilled is that of expanding man's horizon in the art of the possible.

The modern analytical roots of the futures field are somewhat simpler to trace. They originate in the early 1940's and can be traced to the application of operations research to military tactical and more general operational planning problems during World War II. Dominated heavily by those trained in the physical and mathematical sciences, the successful application of multidisciplinary, explicit, and systematic approaches to real world problem-solving led progressively to the development of several other related activities. The age of "big systems," particularly in space and defense, in the 1950's witnessed the application of systems analysis to the design and implementation of a variety of complex hardware and software systems. Attention and emphasis shifted somewhat in the early 1960's to more economic- and business-oriented planning with the advent of cost-benefit and cost-effectiveness analyses and the emergence of program-planning-budgeting and decision analysis as two other problem-oriented multidisciplines. The groundwork was thus being laid for the emergence of strategic planning and forecasting as important components of a developing systems science. And it was in the early to mid-1960's that the first real attempts were made at institutionalizing futures research—with methodological emphases on forecasting, premodeling, and scenario generation activities.[4]

By way of contrast, the participatory roots of the futures field are not yet clearly etched. No real signs of participatory futures field movements can yet be identified in the industrial or corporate sectors. In the public sector, the varied citizen-based groups that have emerged since the early 1960's have not always identified themselves with the futures movement. And yet such groups indeed do represent a vital component in the futures field, particularly those that have sprung up in the last several years under the several "tomorrow/Year 2000/goals" movements. Virtually all of these groups are actively involved in State or local futures planning or goals generation with varying degrees of success. Most of them do not yet have adequate resources—financial, methodological, staff, or otherwise—to achieve the objectives they have set for themselves. And fewer still have very clear perceptions of how to orchestrate successfully the resources required to meet their stated objectives. And yet these nascent groups can represent a component of considerable vitality and importance to the futures field. This is both because they focus on such tangible planning issues as population control, public transportation, national resource utilization, continuing education, and land use, and because they seek broad citizen involvement in thinking creatively about long-term alternative futures.

In summary, then, the general features of the futures field may presently be broadly outlined as follows:

- Visionary — *Integrative* and *strategic* with a temporal focus seldom less than 5 years, and usually 20 or more years, into the future.
- Analytical — *Multidisciplinary* and *methodological* with special emphasis on explicitness in forecasting, modeling, scenario building, and related activities.
- Participatory — *Problem-* and *implementation*-oriented with particular attention to the social and political dimensions of planning.

Functions (or Roles) of the Futures Field

We are now in a somewhat better position to view the basic *functions* or *roles* in the futures field in terms of the confluence of its visionary, analytical, and participatory components. Inevitably some degree of idiosyncratic abstraction is necessary and no claim to uniqueness or essentiality is made. The five basic functions are defined as follows:

1. *Goals Formulation.* This role might be characterized in several ways: issue or problem definition, future-image creation, and alternative futures generation. It is directly related to the visionary component of the futures field and therefore involves the least explicit, most intuitive, and highly personalized components of the field.

2. *Methods Development.* This role includes the development of a body of explicit, organized knowledge on exploratory and tested approaches for event and trend forecasting, process modeling, and data integration. Together with the application-oriented function (see item 3 below), the methodological orientation stems from the analytical or research component of the futures field.

3. *Applications.* One of the practical objectives of the futures field is to provide inputs to planning and decisionmaking processes by helping in expanding the range of useful alternatives, in evaluating future consequences of such alternatives, and in structuring programs of intervention or action. This function may or

may not involve the application of formal methodologies.

4. *Coupling*. It is difficult to describe adequately the process by which the results of futures research are assimilated (or not) by intended users—individuals, groups, or organizations. The function involves social psychological and organizational aspects as much as it does information transfer, since the objective is to influence individual perceptions, behavior, and attitudes. Taken together with item 5, the coupling function is tied closely to the participatory roots of the futures field.

5. *Implementation*. Images, methods, and plans must eventually make themselves felt by "reduction to practice." Implementation actually embodies the culmination of one role and the start of another. First, it includes the interventions and actions actually taken to realize the objectives of a plan; at the same time it provides the information feedback (e.g., indicators) that may lead to the generation of different sets of goals, the development of new methodologies, or the initiation of modified programs of action.

It is useful at this point to visualize these five functions or roles diagrammatically as vertices of a pentagon, as shown in Figure 1. Also represented in the diagram is their relationship to the "roots" of the futures field described earlier. The diagram will prove useful in the subsequent development of existing and possible institutional forms.

Existing Forms: Goal Setters,[5] Researchers, Planners, Citizen Groups

Constructing any typology of existing institutional forms in the futures field—for a field as poorly defined as this one—is not easy. Nevertheless, an initial attempt will be made to relate institutional forms to the functions shown in Figure 1. It is recognized that some inaccuracies in representation will inevitably result. The purpose of the exercise is to assess the degree of match or mismatch between basic futures functions and existing futures institutions.

An "institution" may be defined as "a significant practice, relationship or organization in a society or culture."[6] The number and variety of futures institutions encompassed by this definition is quite large. In recent years some attempts have been made to identify, enumerate, and classify such entities.[7] Although comparability of results is made difficult by the lack of crisp typologies, the following general picture emerges.

During the past 5 years, the total number of futures institutions (to be categorized more precisely subsequently) has remained remarkably constant—approximately 150. To be sure, a current listing would include a number of individuals and organizations not appearing 5 years ago, and conversely. This may suggest that a certain amount of maturing and "shaking out" is taking place. Or it may simply reflect the normal

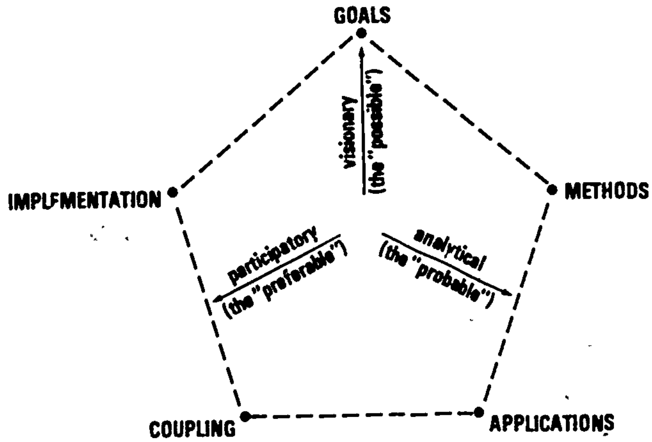


Figure 1. Diagrammatic representation of basic functions or roles in the futures field.

activity of "new starts" and "failures" associated with any new exploratory activity. In any event, the absolute numbers themselves are probably somewhat misleading since the "80-20" rule appears to be applicable here; namely, that 20 percent of the members (i.e., 30) accounts for 80 percent of the futures-related activity.

The existing institutions are categorized into four major groupings: goal setters, research organizations, corporate or government planning groups, and citizen groups. No attempt will be made to classify particular individuals, organizations, or groups in accordance with this scheme for such an attempt is very likely to suffer from inaccuracies and misrepresentations due to faulty data and misperceived emphases.

Goal Setters (50 to 60).[8] This grouping is intended to include particularly creative individuals contributing to the futures field largely on their own. They may be found in universities, research organizations, or elsewhere. Many of them would be identified as visionaries; some might not even recognize that they are members of the futures community; others might pursue futures only as a part-time activity. For each of these reasons, this is the fuzziest—and yet the most important—category, because it is from this sector that most of the highly imaginative image-oriented thinking emanates.

Research organizations (25 to 30). Herein are included most independent—profit or nonprofit, completely or partially (but not peripherally) dedicated, large or small—research organizations engaged in futures research. They may be found within universities or elsewhere in the public or private sector. Although the variety of subforms is still large, it is by no means as heterogeneous a grouping as the others. In fact, this grouping represents the most highly developed of futures institutional forms. Functionally, the emphasis of the group is on the applications-oriented activities and usually (but not always) on methodology.

Corporate or government planning groups (50). This grouping includes most futures-oriented units (sometimes consisting of single individuals) that have evolved from related tactical planning, programing-planning-budgeting, corporate development, or technological forecasting activities. Normally such units are not directly involved in research themselves. Instead they serve as interfacers between the world outside and internal organization operating units. The focus is thus clearly on coupling activities.

Citizen groups (10-25). This grouping includes the most dynamic—and perhaps from a futures standpoint—the most unorthodox members. As noted earlier, the varied "tomorrow/2000/goals" movements might not, at first, be included as part of the futures field. And yet, it is very likely that the success of the futures field depends critically on the effectiveness with which research results can ultimately be coupled to, and be used by, large segments of the interested public.

Conceptually, it is useful to overlay the *dominant* roles of each of the four institutional forms on the diagram of basic functions shown in Figure 1. This is done in Figure 2.

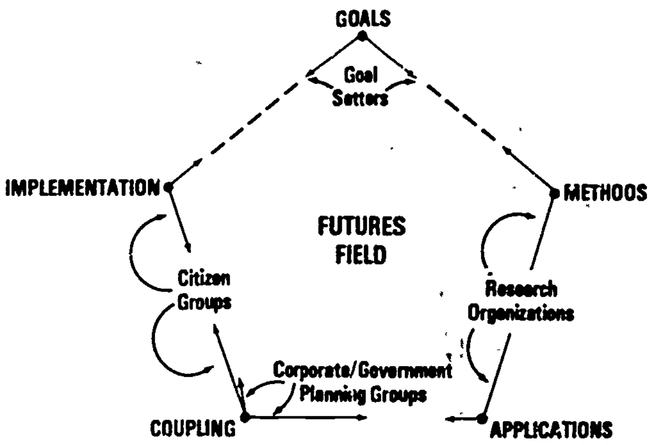


Figure 2. Overlay of institutional forms on basic function diagram.

Idealized Forms: Matching Form to Function

Even a casual glance at the diagram in Figure 2 is sufficient to highlight the principal structural problems in the young and inchoate futures field. As will be recalled, the original diagram in Figure 1 was intended to map the basic functions or roles of the futures field with a defined function or role at each node. Equally as important as the designation of each node is the degree of connectivity existing among these nodes. Degree of connectivity may be interpreted here as a measure of the coherence or unity of the field. Alternatively, it may be interpreted as a measure of the transactional activity between basic functions. For example, one of the simplest connectivity patterns is one in which only adjacent nodes are interconnected. In principle, however, every node can be connected to every other node.

In the present context, institutional forms may be viewed primarily as vehicles for facilitating interconnection among the functions as shown in Figure 2. Thus the "relational" aspect of futures institutions is stressed here, without ignoring the "practice" and "organization" aspects contained in the original definition. This is because the development of the futures field is currently being hampered most by just those shortcomings that are reflections of "poor connectivity": fragmentation of efforts, poor liaison and collaboration, lack of coherence and communication, proliferation of efforts, and so forth.

A simple circuit of the five links shown in Figure 2 can provide some useful insights both about the existing futures institutions and the requirements for new or modified institutional forms. Further insights may be obtained by including other links (e.g., goals-coupling or goals-application), but for simplicity this is not done here.

1. *Goals-Methods Link.* The connectivity along this link is poor or nonexistent. This is because few, if any, *systematic* methods are known for identifying critical issues, for formulating goals, for constructing alternative futures, or for providing early warning signals. At present it appears unlikely that those involved in goal-setting processes will be disposed, or able, to contribute directly to advances in futures methodology. It is somewhat more likely that methodologically oriented researchers may be able to elicit useful insights from goal formulators on the intellectual and conceptual processes used, if serious and systematic attempts to do so were made. In other words, the link emanating from the "goals" node is likely to remain short indeed, whereas efforts to extend methods development toward goals formulation activities are likely to be more promising. At present no particular institutional forms are known to exist that are designed to focus primarily on this functional linking aspect of the futures field.

2. *Methods-Applications Link.* This is, without doubt, the best connected functional link. It represents the relatively well-developed analytical or research arm of the futures field. To be sure, a number of operational problems do exist. Although it is believed that methods development and application functions develop best when they coexist, an almost universal tendency exists to slight methodological efforts in the press of applications-oriented activities. Nevertheless, a variety of institutional forms exists that are presently forging this link—with a set of organizational characteristics (size, composition, structure, support mix, and so forth) that are increasingly well understood.

3. *Applications-Coupling Link.* The connectivity along this link is generally very poor. Herein exists the currently most critical institutional problem in the futures field: the absence of effective mechanisms for coupling research results to ultimate users in both private and public sectors. It is believed that the absence of linking institutions here is seriously hampering the development of the futures field. And yet this problem is only now being given the attention it deserves.

What makes this particularly difficult is that research organizations are at present limited in their ability to develop "coupling arms." Aside from the limited ability of an analytically oriented research staff to function effectively in a social psychological (not just data transfer) context, the basically idiosyncratic nature of coupling activities requires a time-consuming "hand crafting" effort as well. The function is thus more properly performed by planning units located within the organizations or groups they are to serve. Operationally, such units should appear very much like research organizations at one "end" and very much like analogues of their parent organizations or groups at the other. No such institutional forms in either the public or private sector are known to exist at present.

4. *Coupling-Implementation Link.* This link is weak or nonexistent in the futures field. In the public sector this is where most citizen groups are beginning to operate with the dual objectives of increasing citizen awareness by participatory planning efforts (coupling) and of laying the groundwork for implementation of

such plans (implementation). Although some groups have successfully performed the coupling function, they now appear to be faltering somewhat in the implementation phases. It is too early to judge whether emerging institutional forms can bridge this gap.

5. *Implementation-Goals Link.* This link is largely undefined and uncharted in the futures field. It is possible, but rare indeed, for goal-setters to be implementation-oriented as well. (This does not mean that useful social indicator or social audit data is not of interest to such individuals.) And it is even less likely that implementing groups will be concerned with general images of the future—at the implementation stage the focus is far more specific, concrete, and action-oriented. Whether effective institutional forms can be created to bridge this gap in the *near*[9] term is not clear.

What are the implications, then, for future institutional forms in the futures field? For the more immediate future, no *new* institutional forms seem to be required. Rather, a need does exist to reshape existing forms along three dimensions in which "linkages" are either weak or virtually nonexistent. The three modified institutional forms will be designated in terms of their dominant roles: "M" (Methods), "A" (Applications), and "C" (Coupling).[10]

1. *M-Forms.* As already noted, research on futures methods is almost invariably slighted when it becomes integral with applications-oriented activity. And yet an almost certain sterility in methods research is the result when the two are separated. Ideally, what is needed is an institutional form that focuses on futures methods development, while at the same time providing a link to goal-formulation and applications-oriented activities. The functional spanning required is large but not impossible.

Operationally, the number of professionals need not be large (10 to 20) and such a unit might function equally well within a university or a nonprofit institute setting. An important feature of the operation would be the "flow-through" quality of some staff—notably the goal setters or image generators who are not likely to want to maintain strong organizational affiliations. At the same time, the core professional staff must be sufficiently stable to pursue particular applications problems to completion and to provide stimulus and feedback to methods development activities.

2. *A-Forms.* No shortage of applications-oriented analyses exists in the futures field. Much of this work is not particularly responsive to client needs, and severe difficulties normally exist in coupling the results of that which is responsive to ultimate users.

Ideally, what is needed are institutional forms with the following features: high competence in the problem-solving applications areas of the futures field, while at the same time maintaining close contact with methods research at one end and consumer needs at the other.

Operationally, the size of the group need not exceed 20 to 30 professionals operating either in the public or private sectors. The "coupling" arm would be the most diffcult to develop and maintain. For this, staff members with user-oriented sensitivities would be required who were willing to devote considerable time in interfacing activities.

3. *C-Forms.* Effective futures coupling institutions are presently nonexistent in both the public or private sectors. It is true that particular individuals (i.e., futurists in residence) often attempt to perform this function for private corporations or government agencies. But the results are generally disappointing because the resources and level of understanding are not matched to the tasks at hand.

Ideally, coupling institutions are designed to provide bridges between applications or strategic planning outputs and the ultimate users of such outputs. Again, the required functional span is such that some staff must be engaged in applications activities while others must work closely with "line operating" staff who are immersed in tactical planning and day-to-day decisionmaking.

Operationally, the size of such units depends directly on the size of the parent organization or group within which the C-form may be embedded. In effect it must appear as an amalgam of a staff and line activity. It is even conceivable that it could function with a very small core of professionals (i.e., 5 to 10) supported by a larger number of "flow-through" staff from other line and staff organizations.

In summary, diagrammatically, these idealized institutional forms are mapped as shown in Figure 3.

Some Researchable Issues Related to Basic Roles

Structuring institutional forms in order to maintain a suitable balance among required roles or functions is difficult. Equally important, however, are a host of institutionally, methodologically, substantively, and operationally related issues stemming from the basic functions of the futures field; these issues are critical to the further development of the field. Many are endemic

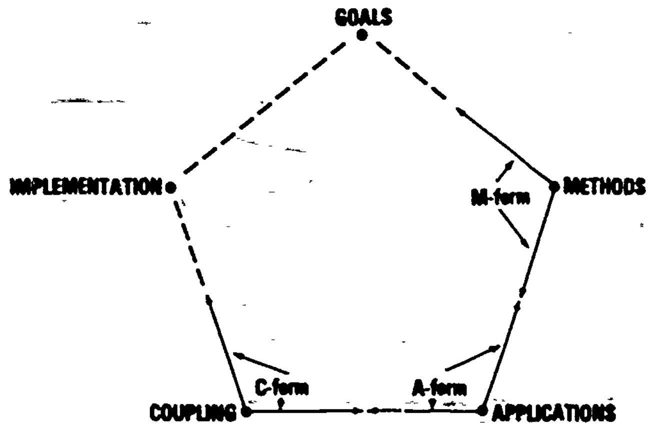


Figure 3. Idealized institutional forms in the futures field.

to research or planning activities generally—futures or otherwise; others are brought into sharp focus by the futures field in particular.

Institutionally Related Issues

Institutionalized activities of any kind bring their own special brand of problems with them. In the futures field these are aggravated by the poorly defined boundaries of the field, the wide range of interests and disciplines to be accommodated, and the highly intellectualized and creative nature of its activities. Thus questions of working group structure, orientations, and effectiveness loom large in discharging required roles or functions in the futures field.

Among the most important researchable issues[11] are the following:

- Retrospective studies of factors (e.g., composition, size, and age) affecting group creativity and productivity in the futures field.
- Exploration of the role of advocacy and its impact on organizational effectiveness in the futures field.
- Study of the influence of "public interest" status—that is, profit or nonprofit structure—on the objectives and performance of organizations in the futures field.
- Analysis of the sources of organizational support in the futures field and the relationship to problems addressed and conclusions reached ("gun for hire" syndrome).
- Study and improvement of communication patterns (or "networking") among institutions in the futures field in terms of nature, frequency, and extent of information exchange and transfer.

Methodologically Related Issues

The major need related to the *methodological* role is the equivalent of a "unified field theory." Methodological proliferation runs high in futures research, compounded by similar proliferation of technique from other sister analytical activities (e.g., operations research, systems analysis, decision analysis, planning-programing-budgeting, and technology assessment). For example, about 50 technological forecasting methods are purported to exist.[12]

What is clearly not needed is another fashionable name for a minor variant to an already known principle or technique. Instead, a serious start should be made in structuring a "science of complexity" that aims to integrate the quantitative and qualitative, analytical and judgmental, and short-term and long-term approaches to systems planning and analysis.[13] A prime objective is to understand and put the methodologies used in the futures field in such a context.

Among the most important researchable issues are these:

- Retrospective studies of the utility of futures methods and the factors that led to their success or failure.
- Development of methods for making value orientations of futures researchers as explicit as possible so that the possible influence on research output may be assessed more easily.
- Exploration of the role of multidisciplinary inputs of several experts to produce joint forecasts, models, and scenarios or alternative futures; particular attention should be directed to the role of group judg-

ments as communicating and integrating vehicles to help bridge organizational boundaries.

- Study of the principal data integrating or cross-relating techniques—cross-impact analysis, scenario building, gaming/simulation, and others—to detect underlying similarities and differences.
- Development of an array of quantitative and qualitative validating measures that can be applied to futures outputs.

Substantively Related Issues

The major need related to the *applications* role is the characterization and understanding of social change processes at the macro (or societal) level. Any futures application inherently deals with change—either change as it has occurred in the past for possible guidance in the present or, more importantly, as it may occur in the future. The dynamics of such processes are now very poorly understood, almost in any social context. The ultimate objective is thus to assist in the development of a "science of change."

Among the most important researchable issues are the following:

- Development of a language and framework for characterizing social change processes including determinative, formative, and random aspects, as well as such basic elements as values, goals, and indices of social performance.[14]
- Study of the role of societal images in social change processes and of how such images should be incorporated into futures activities.[15]
- Exploration of the dynamics of social change from the standpoint of the occurrence of continuities and discontinuities, nature of critical time constants, positive and negative feedback effects, and so forth.
- Analysis of the role of institutions in social change processes vis-a-vis lead/lag relationships, adaptation capabilities, and renewal mechanisms.
- Study of the relationship of change processes at the macro (societal) level to change processes at the micro (individual or group) level.

Operationally Related Issues

The major need related to the *coupling* role is in characterizing and understanding change processes at the micro (or individual) level. This, after all, is the level at which most change processes begin—where individual perceptions, behavior, and attitudes are formed. Also, in view of the strong participatory roots of the futures field, considerable attention should be paid to the nature of the processes by which such participation is to be fostered and realized.

Among the most important researchable issues are these:

- Retrospective studies of the role of futures research outputs in the realization of perceptual shifts on an individual or group basis.
- Exploration of the factors influencing credibility of research outputs as a function of personal and local "cultural" characteristics.
- Analysis of behavioral and attitudinal change processes triggered by alternative images of the future.
- Study of barriers to micro change processes.
- Evaluation of relative effectiveness and role of individualized "hand crafting" micro change processes versus group "culture-modification" micro change processes.

Some Speculations on the Future of the Futures Field

The futures field is still clearly in an embryonic stage of development and it is very difficult to perceive the forms it may take at maturity. More difficult still is the drawing of any clear picture of the evolution of the entire body of processes aimed at advancing the role of intentionality in human affairs, i.e., formal planning, analysis, and decisionmaking.

It is a virtual certainty that the futures field will eventually be subsumed in some larger context—be this a science of change or a science of complexity or under some other rubric—and very likely in accordance with society's proclivity to function on the basis of selective attention and inattention, a new social planning focus will gradually develop. But the futures field will leave, as have other similar activities in the past, its distinctive marks. These are likely to include:

- A deeper understanding of *social change processes* at macro and micro levels.
- A more effective integration of *qualitative* (or judgmental) and *quantitative* (or analytical) components in planning.
- A more *balanced multidisciplinary* approach to planning, with the social sciences as full partners with the physical and analytical sciences.

In the long run, however, the futures field may contain the seeds of its own transformation (or even demise) through a demystification of social planning. Perhaps not unexpectedly, information technology—the centerpiece of the postindustrial society—may play a pivotal role in this transformation.

In a very real sense, the complexity of the problems we face has led us to specialization in planning as it has elsewhere in society. As a result, the basic activities of thinking (or "imaging") and doing (or "acting") have been pulled farther apart. The diagram in Figure 2 may properly be viewed as an artificiality in which a complex "tinkers-to-evers-to-chance" (methods-to-applications-to-coupling) has been substituted for the more direct "goals-implementation" linkage. The two basic players are in actuality goal setters or image generators and implementors. But these are also the most difficult to join because of the lack of a common language and the different philosophical orientations.

Intermediation of some kind will still be required to sort out and evaluate options and consequences, even in the long run. But the form of that intermediation will likely change. This may be viewed in two stages. In the first, the goal setters may be provided vehicles for easier access and interaction with each other, as well as to a new class of intermediaries. In the second, participatory planning and decision-making (involving implementors) at grass-roots levels can increasingly become a reality, with the important feature that effective linkages can now also be established with goal formulators. The pentagon of Figure 1 thus may now become a triangle, or perhaps even a line where the shortened response times permit rapid iteration, interleaving of thinking and doing, and the amalgamation of incrementalism and rationalization in planning.

Perhaps these are no more than fanciful images. But perhaps they suggest real possibilities for coping with increasing complexity by turning that complexity back on itself in providing information monitoring, gathering, analyzing, and disseminating capabilities for social planning and action—a particularized embodiment of the Noosphere of Pierre Teilhard de Chardin.

FOOTNOTES

Note. A number of stimulating conversations with Robert Johansen of the staff of the Institute for the Future served to clarify and extend some of the concepts presented in this paper. This paper is copyrighted © 1974 by the Institute for the Future, Inc. All rights are reserved.

[1] "Futures field" (or "futures") is used synonymously with "futurism", "futurology," or "futuristics." "Futures research" is used in a narrower sense to denote the research or analytical arm of the futures field.

[2] John McHale, *Typological Survey of Futures Research in the U.S.*, Contract H SM-42-69-47. National Institute of Mental Health, 1970.

[3] *Ibid.*

[4] R. Amara, "The Institute for the Future: Its Evolving Role." *The Futurist* (June 1973), pp. 123-126.

[5] An alternate term may be "image generators."

[6] Webster's Seventh New Collegiate Dictionary (Springfield, Ma.. G. & C. Merriam and Co., 1966).

[7] J McHale, op cit., and W W. Simmons. *Exploratory Planning Briefs*, (Greenwich, Conn. October 1973).

[8] Best estimates of the number of institutions included in each grouping.

[9] Some speculations for the more distant future appear in the last section of this chapter.

[10] These terms are used here only as convenient designators.

[11] These issues deal with the whole array of futures field functions or roles defined earlier.

[12] E. Jantsch, *Technological Forecasting in Perspective* (Paris: OECD, 1967).

[13] This corresponds, in part, to the largely unrealized objectives of general systems theory.

[14] Albert and Donna Wilson, *Toward the Institutionalization of Change*, Working Paper WP-11, Institute for the Future (August 1970). R. Amara, "Toward a Framework for National Goals and Policy Research," *Policy Sciences* (March 1972), pp. 59 70.

[15] Fred Polak, *The Image of the Future* (San Francisco Jossey-Bass Inc., 1973).

Chapter 14

Communications in Futures Research

Harold A. Linstone

Typology

The title "futurist" serves as an unbrella for a wide spectrum of individuals just as does the label "systems analyst." For this discussion it is helpful to categorize the futurists in terms of two parameters: (a) the futures paradigm and (b) the futurist's background and disciplinary interest:

Paradigm Grouping

There appear to be four basic types as shown in Table 1:

A. *The Discounters*
This paradigm involves the application of a discounting factor to the future (and the past). The individual concentrates on the present and the discounting factor limits his concern to the immediate future (and immediate past). This subject is treated in detail in a later section.

B. *The Extrapolators*
The philosophy is that the future is an extension of the past; hence trend extrapolation is the favored technique for studying the future. The trend may be linear, exponential, or cyclical, but it is determined entirely by data (i.e., information about the past). The fact that over 90 percent of all forecasts are trend extrapolations suggests the popularity of this paradigm. Herman Kahn's "surprise-free" future with its stress on extrapolation of population and GNP falls in this class. He assumes that the parameters significant to the past generation will be equally descriptive of the twenty-first century. The Forrester and Meadows "standard model" of the world, based on data from 1900 to 1970, runs to the year 2100 with the assumption that the elements and interactions in the model, i.e., its structure, remain unchanged in the next 130 years. The businessman who asks for the "most likely" forecast of market or needs or of population or government budget expects a trend

Table 1. Paradigms of Futurists

TYPE	PHILOSOPHY	TOOLS	CHARACTERISTICS
DISCOUNTERS	Interest only in near term problems		Disinterest in forecasts Improvisation
EXTRAPOLATORS	Future is extension of past	● Trend extrapolation, ● Models based on past	Emphasis on data Empiricism Lockean I.S.*
GOAL SETTERS	Idealism Future can be created	● Normative (needs) analyses ● Imagination, vision	Emphasis on values Creation of new models Leibnizian I.S.
CYBERNETICISTS	Combining of past and future creative approaches (feedback)	● Interaction of exploratory and normative forecasting ● Use of multiple tools	Adaptive Alternative futures Singerian I.S.

*I.S. = Inquiring Systems.

extrapolation. The Leontief input-output model is entirely dependent upon information about the past to create a forecast.

C. *The Goal Setters*

We include here both utopians and dystopians. This group does not believe that the future is an extension of the past. It dreams of a future (usually only one) which is new and different. Imagination and creativity play a vital role as a new future is designed. In Ozbekhan's terms, the cus is on what we "ought to do" or not do rather than on what has been done or is most likely to be done. New values are readily accepted: Charles Reich's "Consciousness III," B.F. Skinner's programmed man, and the doomsday prophets are all examples. Most of the world's religions and cultures reflect utopian views of the future. The Book of Isaiah pictures an ideal society. Plato's "Politeia" envisions an ideal state, and Marx's socialist society is a modern normative vision. As Polak notes, the story of salvation always operates as a projection backward, from the future into the present.[1] Goal setters focus on a destination. If they also concern themselves concretely with ways to carve out a trail to reach the destination, they may well move into the next category.

D. *The Cyberneticists*

This paradigm calls for an integration of past and future orientations or exploratory and normative forecasting. The past is recognized as important in establishing the initial directions for the future, but new futures are considered as a basis for changing the trend. Feedback is thus an essential means in developing an appreciation of the future. Unlike the goal setter, the cyberneticist is not committed to one future but considers alternatives. He fits John Platt's "wagon train" concept. The immediate movement is determined by the past and present. Beyond is a region which can be reconnoitered and the direction of the wagon train changed in consequence thereof. As the train moves on, further changes are likely as the terrain is traversed and new reconnaissance about previously unknown areas becomes accessible. A distant objective is assumed but may be adjusted if the path indicates that it is no longer reasonable.

The paradigms are idealized in the sense that they rarely occur in pure form. Thus the extrapolator may apply considerable discounting in basing the data for his extrapolation only on the recent past, or the goal setter may be influenced by ideas and movements which have proven successful in the past.

Background/Interest Grouping

In systems studies two strands of development proceeded in parallel with very little communication for almost a whole generation (1940's to 1960's). (1) Von Bertalanffy came to systems from a background in biology. He developed general systems theory and a following in the life sciences. (2) Independently the electronics-aerospace community developed systems analysis along engineering lines. With the addition of military-oriented operations researchers and economists, the stage was set for cost-effectiveness. Within each strand internal communications were adequate, but the two enjoyed little contact with each other until social systems became a common concern.

The situation in futures research is more complex. The separate strands include economists who produce economics forecasts, technologists who generate technological forecasts, military analysts who develop threat estimates, business planners who do research on future markets, sociologists who study social trends, and artists/writers who mold futurist ideas into various forms. Then there are popularizers who produce bestsellers or television series about the future and, finally, entrepreneurs who organize future-oriented activities. And even within one discipline there are wide variations in paradigms. Our time frame is also much more extended than that of the systems area. Sociologist C. S. Gilfillan produced his first work in 1907 and "Recent Social Trends" was published in 1933. Futuristics constitutes a very old—if not always honored occupation. Our emphasis in this paper will be on the recent years (1960's and 1970's).

Table 2 presents an overview of the combinations of both groupings which are readily identifiable. Representative names are shown; at times one name may appear in two categories.

Communication Among Futurists

Within a Single Paradigm—Interest Block

This represents the simplest communication problem. There are invisible colleges and individuals tend to be familiar with one another's writings in areas (or subsets of areas) such as technology-cybernetics, sociology-extrapolation, and business-goal setting. For example, consider the intersection technology-extrapolation. There are professional organizations which provide trend extrapolations to their members (e.g., Electronic Industries Association's "Electronics 1985") and professional discipline-oriented journals which present articles on trend extrapolations.

In the blocks which include the goal setter paradigm we find strong personalities surrounded by disciples who form a closely knit communication system. Examples are Robert Theobald, John Galtung, Paolo Soleri, and Buckminster Fuller. Teilhard de Chardin has evoked a society bearing his name.

Books constitute a favorite communication method in areas such as cybernetics-technology and goal setter-sociology. Calder, Polak, Reich, Skinner, Martino, Ayres, and Jantsch are all authors of significant books.

Between Paradigm—Interest Blocks

Here we find books, reports, journals, and conferences as the principal means of communication. Books such as the Kahn-Wiener's *The Year 2000* and Meadows et al.'s *The Limits to Growth* are widely read for the substantive forecasts; Jantsch's *Technological Forecasting in Perspective* is methodology-oriented. Reports are produced by organizations such as the Institute for the Future, The Futures Group, Stanford Research Institute, and the Hudson Institute for business or government planners. Most frequently the subjects are specifically commissioned by the client. Journals such as *Futures* and *Technological Forecasting and Social Change* were created in 1968 and 1969, respectively, to serve as major forums fo communication for both methodology and substantive forecasts.

A recent survey indicated the following organizational breakdown of authors for these journals.

Author Affiliation	*Author Affiliation** TF&SC	Futures
Government	33%†	13%
Universities	25%	44%
Nonprofit Institutes	22%	29%
Industry	19%	10%
Unaffiliated	1%	4%

*First four years of publications
†14% military

In Europe, we also have the French "Futuribles" and the German "Analysen und Prognosen."

Table 2.

TYPE ↓		FUNCTIONAL ORGANIZATIONS Business Military	ARCHITECTS TECHNOLOGISTS ↓	SOCIOLOGISTS THEOLOGIANS ↓	ECONOMISTS ↓	WRITERS ARTISTS ↓
DISCOUNTERS	MOST OF THE PUBLIC	Unsophisticated businesses				
EXTRAPOLATORS	POPULARIZERS: Toffler, Jungk ENTREPRENEURS: Cetron, Gordon	Conventional business planners Utility industry Military agencies	Most technologists Lenz	Bell Gilfillan ← Kahn McHale	Most economists	"Year 2000" (Kahn-Wiener)
GOAL SETTERS		Creative marketing organizations Drucker Bennis Harman De Gaulle	Dubos Gabor Fuller Soleri Spilhaus	de Chardin Galtung Reich Skinner Saint Simon	Galbraith Theobald	Burgess Bradbury Capek Clarke Kafka Orwell most science fiction
CYBERNETICISTS		Defense: RAND systems analysis groups	Ayres Forrester Jantsch → Martino Turoff Gordon	← Dalkey Michael Ozbekhan	Boulding Leontief	"Limits to Growth" De Jouvenel

Conferences constitute another popular mode of communication. Examples:

Sponsor	Participation	
Chamber of Commerce/ Business School sponsored annual Forecast Conference	Invitation and open	Business-economics
U.S. Air Force (J. Martino)	Invitation	Technology-Military
National Academy of Engineering	Invitation	Technology
IEEE–Airlie House, Va.	Invitation	Technology-Sociology
Alpbach Symposium	Invitation	Sociology-Technology
Bellagio Symposium	Invitation	Sociology-Technology
World Futures Research Conferences	1970, 72 Open 1968, 73 Invitation	Sociology-Humanities
Tech Assessment 1973	Open	Business-Technology
Industrial Management (J. Bright)	Open–Payment	Business-Technology
SINCRO–France	Open–Payment	Business-Technology
MBP–Germany	Open–Payment	Business-Technology

These conferences are dominated by presentations of papers (which are sometimes collected in book form later). The invitational conferences also facilitate the operation of invisible colleges. Participants often establish contacts at the conference and maintain them between meetings.

It is somewhat surprising that futurists confine themselves to highly conventional means of communication. The conferences themselves have not been as innovative in their *modus operandi* as certain professional society meetings which have no explicit futures orientation (e.g., some Institute of Management Sciences meetings). Techniques such as Delphi conferencing and advanced media techniques are not yet in use.

Communications with Nonfuturists

Nonfuturist Academicians and Professionals

The traditional discipline orientation tends to limit the futures interest of academicians and other professionals to their own narrow fields. Besides occasional forecasts pertinent to those fields and found in their professional journals, their principal source of futures material is the popular press. Excerpts from books such as *The Limits to Growth* and *The Year 2000* are published in newspapers and magazines. In addition, a professional society meeting might feature a guest speaker who presents a talk about the future—often more in the vein of enterainment than serious insight. One exception is the American Association for the Advancement of Science and its journal *Science;* another is the *Bulletin of Atomic Scientists.* Both have a serious concern with the interactions of politics and science, most frequently focusing on short range concerns, however.

The discipline orientation of most academicians and professionals predisposes them against inter- or trans-disciplinary modes of thought. We are facing here a long tradition which is extremely difficult to change. The Ph.D. dissertation is a case in point. It forces the candidate to "know more and more about less and less," culminating in a dissertation which only a handful of his peers can understand. As Robert Straus has noted:

> departments have become the principal bases of power within universities . . . Because almost all departments are identified with specific disciplines, their political functions have often served as forces to preserve anachronistic disciplinary identities, to enforce obsolete compartmentalization of teaching, and to discourage formal research and teaching activities that cross disciplinary lines. This . . . has been vigorously supported by many faculty who wish to work within comfortably delineated areas of specialization and subspecialization, unperturbed by the need to recognize broader implications in their own spheres of interest and blissfully oblivious to the implications that developments outside of their discipline may have to their own special interests.[2]

Even if futures research were an established discipline the compartmentalization would inhibit communications with the bulk of academicians. Since most professional people are the product of a highly discipline-oriented training, their perspective also tends to be constrained, although perhaps not as severely as that of the academician. This is particularly true of lower and middle levels in government and industry.

Consider the planner in the armored vehicle field in the Army. He may be concerned with the planning of future tanks, possibly covering a period of 10 to 15 years. He may even be exposed to alternative world scenarios involving diverse types of warfare. If he is presented with a scenario which considers that the only type of war after 1980 is nuclear war, he will try to assess how tanks can be used in nuclear war. If he is given a scenario indicating that the only kind of war will be guerrilla war, he will determine how to employ tanks in guerrilla war. It will probably not occur to him that in either case tanks may no longer be needed. And if that thought does cross his mind it will be quickly extinguished by the recognition that this might imply the end of the agency for which he is planning. In the typical bureaucratic organization the original *raison d'etre* slowly, but inexorably, loses its primacy once the organization is well established to the goal of self-perpetuation. Consideration of the future is limited to those aspects which will support this objective.

Another reflection of the bias in the military is the manipulation of the future "threat." Inflated or distorted intelligence data are made public to support requests for higher defense budgets. The fictitious "missile gap" in 1960 and Pentagon press releases about new Soviet submarine bases timed to coincide with Congressional budget hearings are cases in point. Similarly, business planning activities tend to be strongly biased in their view of the future. Petroleum companies and automobile manufacturers have consistently ignored forecasts which warn of possibilities, or suggest alternatives, detrimental to their established lines of business. They may even go to considerable pains to undermine or discredit such futures studies by public relations and other techniques. Politicians subsist on a base of popular support and therefore use forecasts to strengthen this support. As discussed in Chapter 5, Hitler's "thousand-year Reich" is illustrative of the use of forecasts to manipulate public opinion.

Thus we conclude that many potential users of forecasts misuse such information to advance the cause of their organizations or their own fortunes. The picture is not entirely one-sided, however. Consider the creation of the new Office of Technology Assessment. The origin of the concept appears to have been communications between Congressman Emilio Q. Daddario, MIT's Jerome Wiesner, and Charles Lindbergh. Daddario undertook the drive to implement technology assessment with his Congressional subcommittee in 1965, and the effort reached fruition in 1973. Of course, it might be argued that technology assessment is only marginally future-oriented. The basis for its need resides not in future technology nearly as much as in current technology. As Harvey Brooks has noted, the social and environmental difficulties we encounter today are consequences of new or excessively massive applications of *old* and familiar technologies. This explains the relatively limited role of technological forecasting in technology assessment.[3]

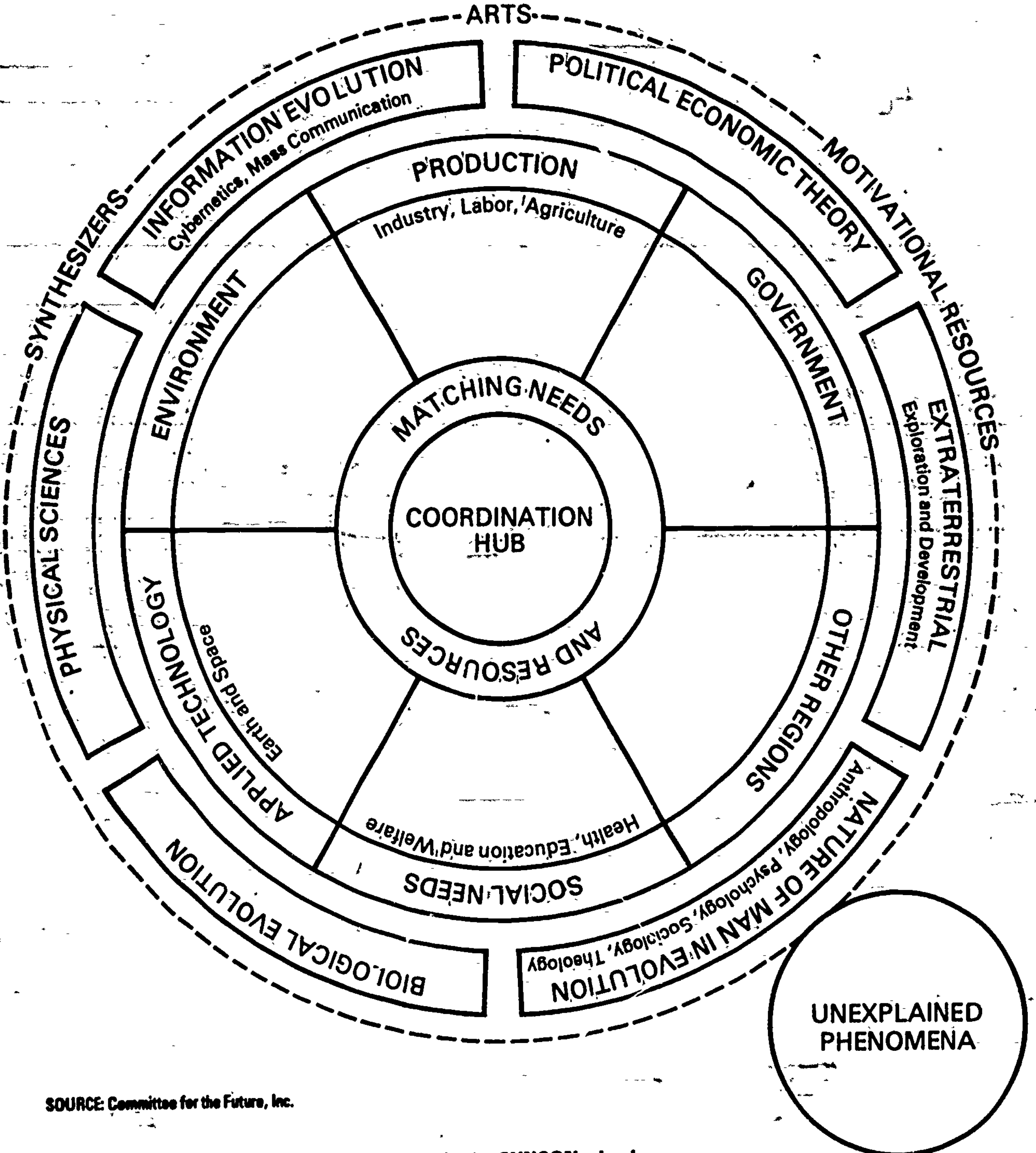
The General Public

The individual's concern with the future is strongly reflected in his children. Traditionally they have provided him with a form of immortality, as well as a means of security in his old age. Life insurance and savings for children's college educations are concrete means of implementing this concern today. The individual is also conscious of his own future in terms of health and economic security. The latter is a fundamental consideration in the support of organizational stability and includes seniority and tenure. The identification of the security of the individual with the perpetuation of the organization which employs him is, in turn, responsible for the rigidity and permanence which characterize most organizations. Change is often interpreted as danger, i.e., a threat to security, and is therefore resisted.

The general public constitutes the largest body with which the futurist interacts. Elected officials necessarily take their cue from the attitudes of the public. If the people discount the future, their representatives will generally follow (exceptions are those who lead rather than follow). The subject of discounting is thus at the heart of the problem of communication between futurist and the public.

The higher strata of the socio-economic hierarchy tend to take some interest in futures activities, although this may well be placed in the category of entertainment. They read books like Toffler's "Future Shock" and magazines like *The Futurist;* they see movies like "A Clockwork Orange" and "Fahrenheit 451." They enjoy plays like "R.U.R." and performances like those of Buckminster Fuller. They may join a local chapter of the World Future Society, attend a SYNCON gathering, or become involved in a communications network such as that developed by Robert Theobald. The latter two represent interesting new communications concepts. SYNCON is an acronym for "synergistic convergence" and is the brainchild of Barbara Marx Hubbard and her Committee for the Future. In its current format a large group of people meet for 3 or 4 days to discuss societal goals and aims. Initially they are partitioned into 8 to 15 groups depending upon background and interest.

The groups are physically arranged to form a wheel as shown in Figure 1. Each group is reasonably homogeneous, and the common interest facilitates its discussions. In the second stage, pairs of groups are combined for further discussions. The combining process continues until there is only one group. Individuals of entirely different backgrounds are forced to interact



SOURCE: Committee for the Future, Inc.

Figure 1. The SYNCON wheel.

(e.g., artists and engineers). Thus the traditional compartmentalization is systematically broken down. The group discussions are interspersed with informal talks by futurists, interviews, and other presentations. Closed circuit television pipes this input to each group and also monitors the progress of the group discussions. SYNCON sessions have been held at Southern Illinois University, in Los Angeles, and Washington, D.C.

Theobald's net involves newsletters and other written materials (e.g., the magazine *Futures Conditional*) circulated to interested individuals with feedback emphasized.

Some members of the lower strata of the socio-economic hierarchy find satisfaction in prophetic spellbinders such as Jean Dixon and Edgar Cayce. In most cases they are too much immersed in the day-to-day problems of living to spend their leisure time in the pursuit of the future.

A fundamental problem which inhibits communication is system complexity. The individual is familiar with the world which immediately surrounds him in time and space. He may be able to project one or a few changes and superimpose them on his present world. He is unable, however, to consider simultaneously a large number of simultaneous changes in elements of the system and their interactions. He can visualize increased crowding with a growing number of automobiles and see the likelihood of more freeways. But he has difficulty determining the impact of a shift from suburbia to a different communal life style on private transportation, or even of the impact of high taxes on automobile use. This is, of course, the same difficulty which impedes the act of forecasting itself. A consequence of the complexity of the systems of most interest is the difficulty of communicating insights about such systems to others. We will consider this problem further in the next section.

Communication Dilemma No. 1: System Complexity

Systems which involve nature, man, society, and technology are very different from those which are purely technological or purely economic.[4] The many variables, the high degree of interdependence between them, and the nonlinearity of the relationships leads to fundamentally dissimilar behavior. Complex systems are stable in the face of most changes in input, yet exhibit catastrophic changes with a few gradual alterations in input. They have long lag times in accommodating to desired changes and historical as well as spatial linkages. The overall impact is often felt in the occurrence of unexpected or counterintuitive consequences.

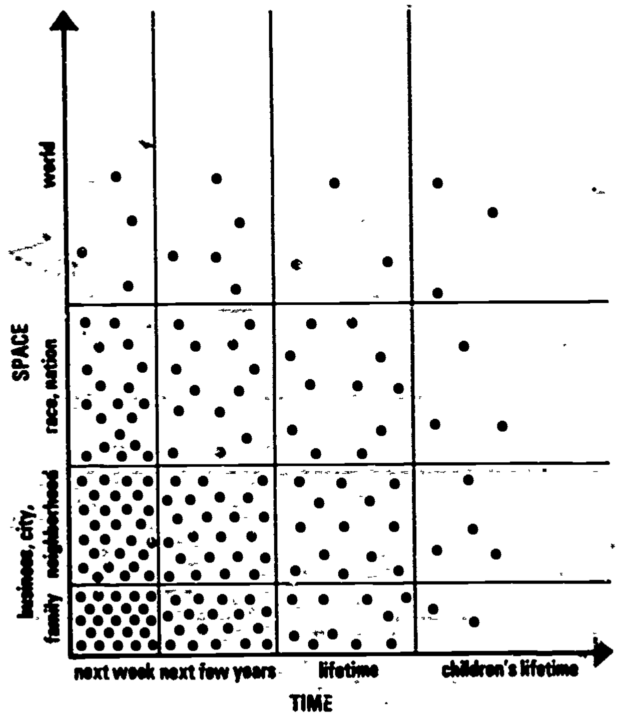
Clearly the traditional scientific approach of structuring, partitioning into subsystems, and analysis of each element is inadequate for such systen Rational microdecisions all too frequently lead to ational macrodecisions. Even the bold work of Forrester and Meadows in system dynamics and the scenarios of Kahn and Wiener manage to treat only the tip of the iceberg. Holistic insights must be obtained and communicated. A narrative is not only an unsuitable means to communicate a painting by Van Gogh or Picasso; it also fails to communicate a future urban environment. A picture cannot communicate either an orgasm or a societal breakdown. The weakness in holistic communications is seen most glaringly in scenario writing, which utterly fails to transmit the significance of a projected future. In traditional fashion it tends to project a few currently meaningful parameters and neglects to consider many potentially crucial ones (which also change) as well as their numerous interactions. Our normal failure is not the inability to derive a "correct" scenario, but rather to derive *any* holistic representation other than "more of the same."

A novelist may have the talent to communicate the meaning of a complex future more effectively: Orwell's *1984* and Kafka's *Trial* are examples of such capability. We must explore communication concepts outside the range familiar to the academic community. I want to make it clear that I am not referring to simplistic devices such as placing Fuller geodesic domes, abstract paintings, or space vehicles on exhibit—a procedure *de rigueur* at some future oriented meetings. The works of Holling,[5] Johnson,[6] and Schule[7] are more indicative of the need. Among the innovative paths are the following:

1. Use of the computer as a high level communicator The computer can give the individual a new ability to "play" with complex systems, to get the "feel" or experience which he relies on to comprehend such systems. It is possible to create an informal, intimate dialog which serves to concretize *Gedankenexperimente.* The key is the use use of multiple sensory modalities in interacting with the computer (manual-visual, audio-visual). A first step is the workshop series developed by Holling and Goldberg for participative urban and regional planning at the University of British Columbia.

2. Scores and scenarios developed by artists together with systems scientists and futurists. The joining of the arts and sciences may prove a vital step in communicating complex future alternatives. The "scoring" concept of Lawrence Halprin[8] is a example of a catalytic agent to effect the melding of artists and nonartists in an endeavor of this kind Audio-visual taping in turn may be the forerunner of three-dimensional multiple motor sensory environmental simulations.

3. Vivified models of complex systems. The value of models which are true prototypes of new systems is recognized in technology-based industry. The most recent change in procedure for military aircraft procurement in the United States reintroduces the prototype as a key step before commitment to production. It is surprising that insistence on "living" models has been lacking in so many large-scale social system proposals. An individual who is physically able to live in several new urban environments is far more likely to make a meaningful choice than one who must do so on the basis of narratives, pictures, or architect's models.



D. L. Meadows et al, *The Limits To Growth*, (New York: Universe Books, 1972).

Figure 2. Human perspectives.

Communication Dilemma No. 2: Discounting the Future

A bitter lesson which every forecaster and planner learns is that the vast majority of his clientele has a very short planning horizon as well as a short memory. Most people are really only concerned with their immediate neighborhood in space and time (see Figure 2). Occurrences which appear to be far removed from the present position are heavily discounted. Moreover, the degree of discounting may well vary with the individual's cultural and social status. A person at the bottom of Maslow's human values pyramid will discount environmental pollution much more heavily than someone near the top. The poor, for whom survival is a daily challenge, are hardly going to lose much sleep over a pollution or population crisis 20 years in the future. A similar difference applies to space: a slum-dweller worries about rats he can see in his room, while the jet set worries about depletion of wild game in distant Africa. Among nations, Americans seem to have a particular penchant for discounting. They are a young Nation (and the young discount more than the mature). They are raised on installment buying and "fly now, pay later" exhortations. The writer is only as good as his latest book, and the politician is attuned to the voter's persistent query, "But what have you done for me lately?"

The massive impact of this personal discounting process on the Forrester-Meadows World Dynamics model is readily demonstrated. Consider an individual who was unconcerned about global pollution in 1950 and is still untroubled by current world population density and food availability. Normalizing these variables to 1950 and 1970, respectively, the Meadows "standard" run generated the pollution, population, and population/food production curves denoted in Figure 3 by "0."[9] Crises peak in 60 years for pollution, in 80 years for population, and in 90 years for food production. *Application of a discount rate equal to, or greater than, 5 percent reduces the population and pollution crises to minor significance, i.e., no dramatic worsening of the current situation is perceived by today's observer.* Future food shortages may exert a somewhat stronger personal fear and possibly result in a lower discount rate for food production than for population or pollution data. But here we find that no large-scale deterioration of the situation occurs for 50 years even without discounting; in fact, there is some improvement over the next 20 years. It is not surprising, therefore, that cries of crises fall on deaf ears.

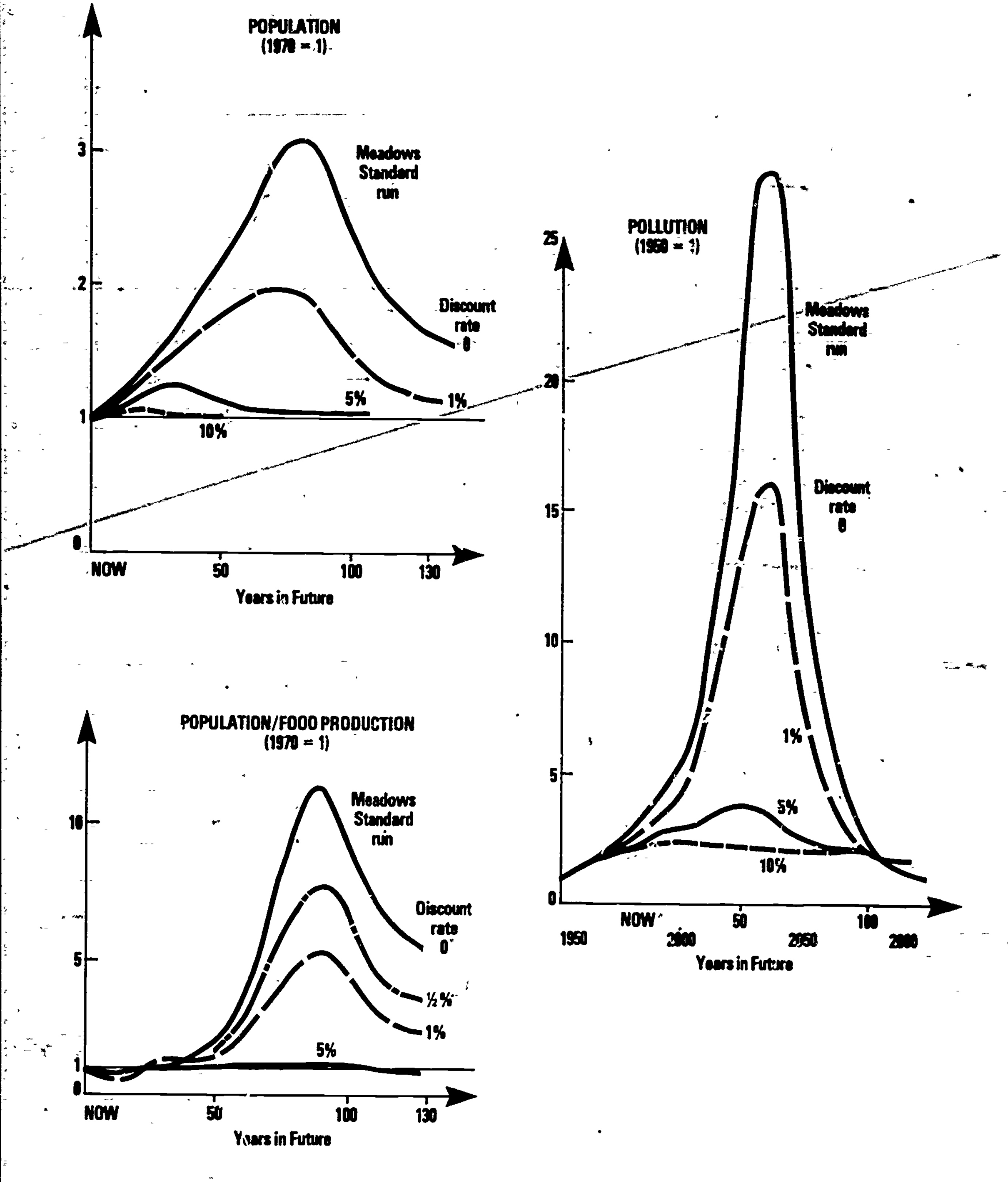


Figure 3. The discounting effect in crisis perception.

Apparently discounting acts in both directions—future and past. A crisis about to happen or just experienced is discounted little, while events a generation in the future or in the past are discounted severely. The historical pattern of national wars suggests that a war is discounted completely in the span of about one generation. The tendency to discount the past is observed even among extrapolation-oriented forecasters. While they do look at the past it is almost invariably the recent past. I would guess that of the 90 percent of forecasts which are trend extrapolations, 90 percent use data extending back no more than one generation. The reason usually given is that either data, e.g., GNP, is not available or is not relevant.

Forecasters lack a sense of history. One of the most interesting techniques—forecasting by analogy—is rarely used. An outstanding study was done by Mazlish on the analogy between the impact of railroad technology and space technology, with emphasis on spinoffs.[10] The current energy crisis has had its predecessors: animal food shortage in Paleolithic times, manpower shortage in the fourth century A.D. in Rome, and most specifically the wood shortage in the sixth century in Europe (which led to the introduction of coal). There may, in fact, be a genetic basis for man's concentration on the present. An ability to maintain such focus probably enhanced his chances of survival in frequently hostile surroundings.

Unfortunately, this space-time discounting phenomenon is usually poorly understood by the futures researcher and is a major reason for the ineffectiveness of long-range planning activities generally. The forecaster points to remotely located threats and opportunities only to find a frustrating and maddening unresponsiveness. Rarely does he try to come to grips with the basic communication difficulty. Yet we do have some clues. The most effective trigger to force installation of traffic lights at a dangerous intersection is the occurrence there of a series of fatal accidents in a short time span. Catastrophic fires spur the enactment of new fire safety rules, and passenger aircraft collisions lead to improvement in air traffic control. The actuality of the Soviet *Sputnik* galvanized a dramatic shift from the low-key Vanguard space effort to the gigantic Apollo program. The Arab oil embargo has stimulated both energy conservation measures and research on new kinds of energy sources and means of delivery.

The key to effective communication is a perceived crisis or opportunity of sufficiently serious proportions in the near space-time neighborhood of a sufficiently potent client or constituency. This can be brought about in two ways, schematically shown in Figure 4: (1) moving the distant crisis or opportunity well within the client's current field of perception or planning horizon or (2) extending the client's field of perception or planning horizon.

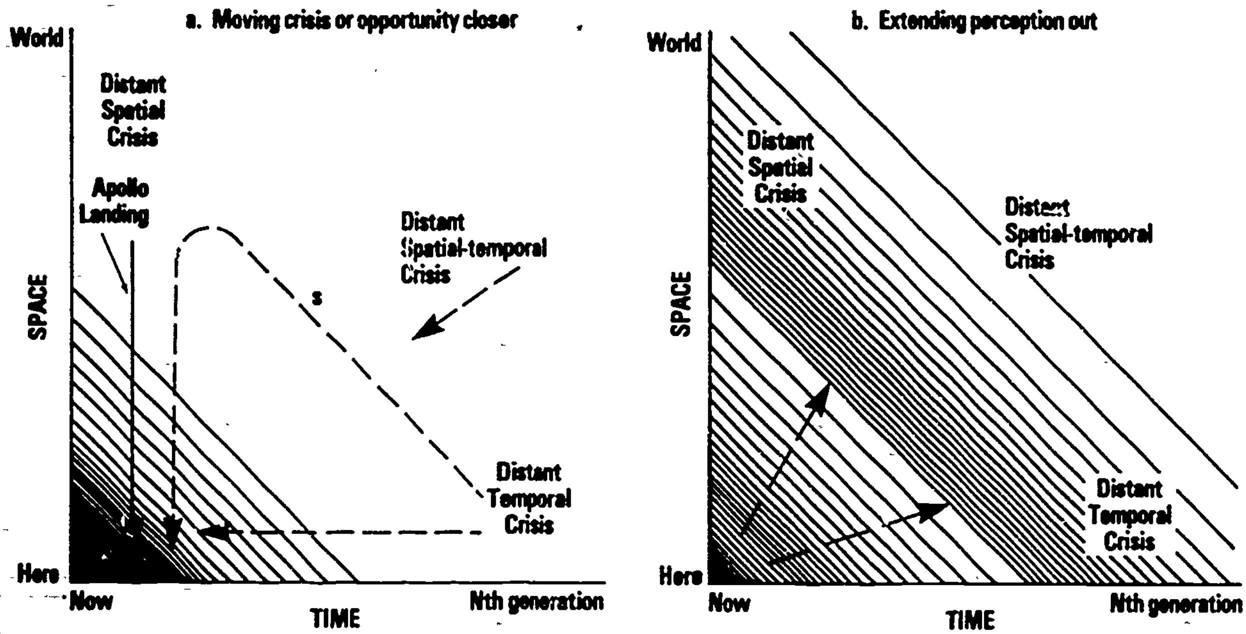


Figure 4. Space-time perception.

1. The most obvious means to compress the time dimension is to create a minicrisis in the near term. If a potential future catastrophe can be averted by such a strategy we are justified in considering it. Labor uses the concept in precipitating strikes; the military use it in creating "threats" (e.g., the nonexistent "missile gap" in 1960). The current Arab oil embargo had precisely the characteristics of a minicrisis for the United States. The awareness and actions which resulted may avoid a much more serious crisis in the 1980's.

There are less obvious but equally intriguing approaches. We note that technology, in the shape of communications, has been far more successful in compressing space than time. Television has dramatically foreshortened the space dimension and brought distant crises and triumphs into the living room with forceful impact. The Kennedy assassination events and the Apollo manned lunar landing are examples of its capability. Communication has been much less effective in similarly foreshortening the time dimension. Orson Welles's radio version of H. G. Wells's *War of the Worlds* is a rare example, again reflecting the importance of the artist.

We should not only study improved means of using communication to compress time (e.g., "All in the Family" set in 1990?) but ways to substitute space for time and then compress the space dimension (see arrow S in Figure 4). If we can recognize a future crisis for our society in a current or recent crisis of another society, we may be able to focus on that society and use communications to bring its perception vividly into our living rooms. Possible examples are the overpopulated cities of India and the undernourished poor of Africa and South America. Similarly, we may find future life styles already casting their shadows in other cultures today. As 300 hours in the life of "An American Family" were filmed in 1972 and subsequently presented in 12-hour long episodes on television, the family life in other potentially relevant cultures can be brought home to today's public.

There are, of course, also dangers inherent in a strategy of type 1: repeated creation of minicrises can dull their effectiveness (the "cry wolf" syndrome), and misuse by vested interests may cause harm.

2. The other alternative, extension of the individual's field of perception or planning horizon, poses even more intriguing questions. Are the current limits physiologically determined or illusory? There is growing interest in the development of new concepts to condition the individual to a longer time horizon. The work of Jean Houston and Robert Masters on time distortion in altered states of consciousness is pertinent. We all recognize that 5 minutes can at times seem like an hour; conversely, individuals facing death but unexpectedly surviving (e.g., airborne parachutists with unopened parachutes landing safely in a snow bank) consistently report that their entire lives unreeled before them in a very brief time interval. Masters and Houston invoke altered states of consciousness to decondition the ordinary way of thinking about time. In this way an enormous subjective experience can occur in a very brief period of clock measured time. They have successfully used this accelerated mental process of time distortion with novelists who are blocked in their work. The use of "mind games" is only one of the techniques of a vast frontier.[11] It is obvious that our vast ignorance in this area can no lon' be accepted.

This discussion has concentrated on the future. It has already been noted that discounting of the past may also reduce the effectiveness of the forecaster.

Extending the forecaster's historical consciousness would appear to pose a more readily solvable problem. Interaction between historians and futurists can be facilitated. Again the question resolves into one of interdisciplinary communications. One of the means should be joint "games" to attempt to do a forecast starting at a specific point in history and assuming only knowledge of information of events prior to that time point. Comparison with actual history subsequent to the forecast starting date can be most instructive. Even detailed analysis of the relatively few available historical forecasts (e.g., the anonymous 1763 work *The Reign of George VI, 1900-1925* and H. G. Wells's *Anticipations)* is a worthwhile exercise too seldom undertaken by futurists.

Some New Suggestions

The Working Groups at the 1973 Special World Conference for Futures Research brought forth a number of proposals bearing on improved communications involving futurists. These serve to indicate current thinking. The last two of the four following proposals generated at Rome address themselves to the basic dilemma of communicating holistic insights about complex systems:

1. Proposal for Worldwide Futures Communication Network. Presently remotely located groups are unable to confer with each other on vital common issues, e.g., world population growth forecasts. A task force should undertake the development of a multi-

media network which includes progressively cassettes, videotapes, and ultimately a two-way real time interactive, computer augmented conferencing network available to all groups in the world interested in the study of problems of the future on a continuous, i.e., when wanted, basis. The appropriate sponsor is the World Federation for Future Studies with United Nations and/or foundation support. Full-time professionals will be required. The project should begin as soon as possible so that international operation could be effected within 4 years. The concept could be demonstrated at a forthcoming World Federation for Future Studies Meeting (through simultaneous meetings held at a number of locations).

The Rome conference itself underscored the importance of such a concept: the absence of representatives from distant underdeveloped countries as well as the hiatus in communications between successive meetings. The technology is available today and analogous networks are already operating internationally (e.g., worldwide weather reporting system available to all airports and used in all countries since the 1960's or the ARPA net in the United States). The proposed system can, in fact, use existing networks in many areas at a cost of $10 per hour or less.

2. Participative Conference. Futures researchers are in danger of becoming a self-appointed elite which mirrors the existing elitist system in the society for which they presume to advocate change. A very high priority should be given to develop techniques and methods to enable the entire community to participate in futures studies. A conference is proposed which provides the opportunity for simultaneous activities to satisfy many needs and many abilities. It should be organized so that any person can design his own conferencing agenda and meaningfully participate in one or ten days. He could participate simply by collecting information, by dialoging with others concerning ideas, or by developing and initiating action programs. The World Federation is an appropriate sponsor. Such a conference could be held as soon as practically feasible and in almost any location in the world. The diagram (Figure 5) suggests one possible physical arrangement.

This concept enables highly informed as well as uninformed individuals to participate in both informal and highly structured situations. It provides for maximum opportunity of choice and freedom as well as structure and can be adapted to global, regional, or local situations. It is practical because it provides for maximum opportunities to participate in planning and implementation. The financial cost is realistic and does not fall on only one hosting organization.

3. Proposals for Futures Museums. The realization of quality of life models presents a problem in social mini-technology. Using futures research methodology, the establishment of small-scale quality of life "museums" is proposed. The word museum here is used in the original sense (*museion* means home of the arts and techniques).

This activity will facilitate the dialogue between futurists and others at a level of holism which is unattainable with more conventional means of communication. An individual can walk through and "feel" alternative future cities in the same way he experiences colonial Williamsburg, Virginia City, or Disney's Futureland.

4. Magic Mountain Centers. Magic Mountain Centers are proposed where "re-creation" of the individual can take place in a holistic way. Such centers will provide space, solitude, skilled teachers, and enablers. They will afford the opportunity to communicate with oneself, one's family, and others of interest. The leaders of Magic Mountain must be teachers and gurus who do not control the processes required but facilitate and bear witness by their own behavior. Those who come to Magic Mountain can come for short or long periods. Resources of art, religion, and science will be available to help people move between the known the the unknown in new and open ways. In this manner the future-making capacities will be released in the individual. The sponsor suggested is the Committee of Correspondence on Religion of the Future which is already being set up through The Rome Special World Conference. Initially, centers in Europe, Asia, Africa, and North and South America are advocated. The concept is considered vital because futurists and others need to learn to work in new ways in order to be able to create futures, not merely theorize about them. Ideas and starting points for this kind of center already exist in France, England, Japan, North America, and other locations.

Conclusion

The two dilemmas described in the sections on *communicating complexity* and *dealing with discounting* must be given top priority in the communications aspect of futures research. The other communications difficulties pale by comparison. If these critical communications difficulties cannot be overcome, much of

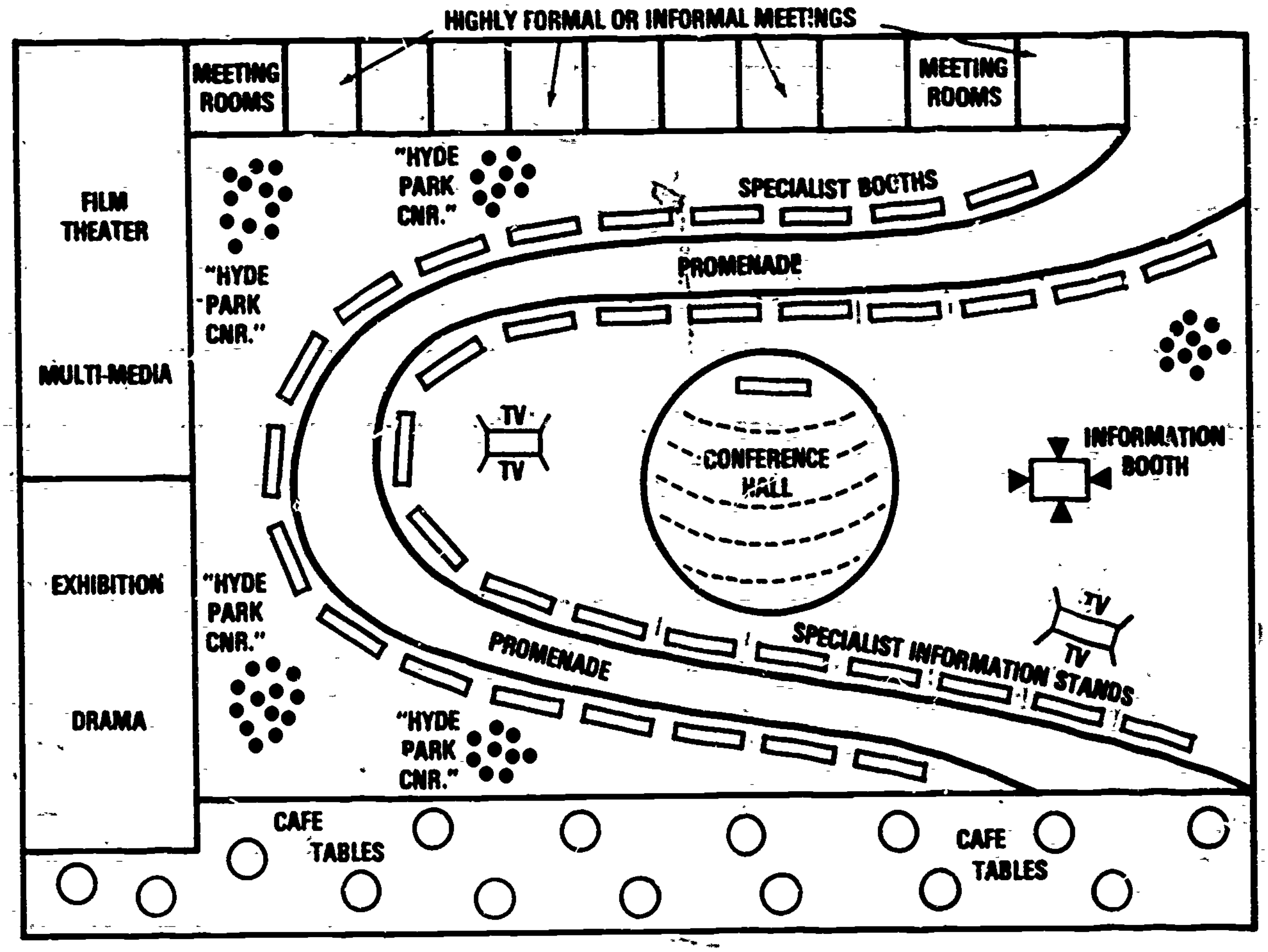


Figure 5. Suggested facilitative meeting environment (Group 2).

the substantive achievement in futures research will be wasted. The planners and futurists will find themselves in the role of the court jester: they will be tolerated, and only so long as they amuse.

FOOTNOTES

[1] F. Polak, *The Image of the Future* (San Francisco: Jossey Bass, 1972), p. 5.

[2] R. Straus, "Departments and Disciplines: Stasis and Change," *Science*, Vol. 182 (November 30, 1973), pp. 897–898.

[3] Cf. case studies of technology assessment by The MITRE Corporation: [*A Technology Assessment Methodology*, Project Summary and Six volumes (Washington, D.C.: 1971)] and by J. W. Dickey, D. M. Glancy, and E. M. Jennelle [*Technology Assessment* (Lexington, Mass.: Lexington Books, 1973)].

[4] Note Holling's description of ecosystems: "There could hardly be another area with as much ignorance, as much confusion, and as much unknown. We have had a taxonomy of ecosystem structure for years and only now are glimmers of an explanation of the dynamics emerging. We also have 40 years of experience in modelling ecological and economic processes in a simplistically irrelevant way and at the best we have only found what not to do. But if our knowledge of ecological and economic systems is in a bad state, our knowledge of man is considerably worse . . . Wherever we look there are gaps—gaps between methods, disciplines, institutions, and constituencies." (C. S. Holling and M. A. Goldberg, "Resource Science Workshops," Resource Science Centre, University of British Columbia, September 1971).

[5] Holling and Goldberg, *op. cit.*; C. S. Holling and M. A. Goldberg, "Ecology and Planning," *Journal of the American Institute of Planners* (July 1971), p. 221.

[6] A Johnson, "Information Tools that Decisionmakers Can Really Talk With," *Innovation*, No. 10 (1970); A. Johnson, "Organization, Perception, and Control in Living Systems," *Industrial Management Review* (Winter 1969).

[7] D. S. Scheele, unpublished material, Social Engineering Technology, Inc., Los Angeles, Calif.

[8] L. Halprin, *The RSVP Cycles: Creative Processes in the Human Environment* (New York: George Braziller, Inc., 1969).

[9] The "0" curves are based on Figure 35, p. 124, of Meadows, *op. cit.*

[10] B. Mazlish, *The Railroad and the Space Program: An Exploration in Historical Analogy* (Cambridge, Mass.: MIT Press, 1965).

[11] R. Masters and J. Houston, "The Varieties of Postpsychedelic Experience," *Intellectual Digest* (March 1973), p. 18; R. Masters and J. Houston, *Mind Games* (New York: Viking Press, 1972), p. 73.

Chapter 15

Monitoring the Future

Wayne I. Boucher
and
Katherine H. Willson

Executives, analysts, and citizens alike have a vital interest in monitoring the environment, that confluence of natural and man-made conditions relevant to current choices or the evaluation of current policies.[1] Analytically, this confluence can be viewed as a culmination of one or more pasts, or the rushing in of one or more futures. In practical affairs, especially where decisions are being made under pressure, it is typically both. Surprisingly, while millions of dollars and man-hours are spent annually in capturing the past in the expectation that it may prove relevant, and while every individual and organization has some sort of system, formal or not, for making portions of this information relevant, very little is known about how these systems actually work—what they include, how they include it, whom they serve, with what effect, and so on. This is particularly so in business and government.[2] We assert, however, that every successful system involves consideration of the future; indeed, the argument is increasingly being made that institutional intelligence or monitoring capabilities should be formally structured to include the approaches and results of futures research.[3]

As if in response to this need, the growth of the futures movement has been followed closely over the last decade or so by an increasing number of attempts by persons within the movement to develop information systems and sources that can help (from the outside) to alert decisionmakers to important prospective trends and events. These efforts have sometimes involved the establishment of wholly new organizations, but more often they have simply been appended as new activities within existing institutions. In the following paragraphs, we consider briefly several of the more significant "trend watching" systems now in existence or being developed, and then look more closely at four services involving novel and quite interesting products.[4] Throughout, our focus falls entirely on efforts that have, or promise to have, a futures orientation; that is to say, we omit from consideration the work of those hundreds, perhaps thousands, of public and private organizations that merely track developments over time without projecting them into the future.

Trend Analysis Program

The Trend Analysis Program (TAP), which is administered by the Institute for Life Insurance (ILI), is designed to identify, on the basis of a continual search of published literature, the social trends which may be significant to the insurance industry.[4] TAP is an effort by the insurance business to "study the environment in which our business operates and to transmit what we learn to our member companies so that they can be attuned to what is expected of them and be, therefore, more effective in serving the public."[5] This program is unique in several respects, perhaps the most interesting of them being that it is the only voluntary yet formalized system for monitoring the future which has been established within and by a major U.S. business sector. Four subject areas are monitored: (1) science and technology; (2) the social sciences; (3) values, attitudes, and opinions; (4) and political systems. Each of approximately 120 volunteers within ILI member firms is assigned to read a specific periodical that regularly publishes on one or more of these subjects. This literature consists primarily of popular publications, but a few specialized journals are included as well. When articles are found that contain suggestions of trends that could be important, abstracts are prepared by the volunteers and forwarded to an ILI coordinating committee consisting of 15 senior executives from ILI member companies, supported, as required, by outside specialists in futures research. The role of the committee is to scan periodically the abstracts that have been submitted by the readers and to decide which are significant enough to be reported upon in the next *Trend Report*. The abstracts selected are then summarized and published for use by ILI members.

Each report treats one topic in detail. The reports also include a section, "Straws in the Wind," which briefly presents interesting and relevant issues discussed in the recent literature. This section contains excerpts of items taken directly from the Abstract Analysis Committee meetings. Incidentally, while *Trend Reports* are based almost totally on material in published sources, like most of the other services described in this chapter, they also contain pertinent data from other sources. In addition to TAP, ILI has conducted since 1968 a program called MAP (Monitoring Attitudes of the Public). MAP is a monitoring system which attempts to survey public attitudes and opinions toward life insurance, the life insurance business, and related issues. MAP surveys are conducted annually. Attitudes on 50 topics were measured in 1974. These included acceptance of life insurance coverage for women, the family, government regulations, consumerism, and life insurance as a necessity. New questions are investigated each year but emphasis is kept on systematic monitoring of attitudes toward change by repeating questions from survey to survey, holding the methodology constant. This practice has generated a great deal of information about important trends in public attitudes toward current social issues.

Trend Report

The Urban Research Corporation of Chicago publishes the *Trend Report*, an early warning monitoring system comparable to the Trend Analysis Program of the Institute for Life Insurance. While the TAP program monitors and abstracts magazine periodical articles, the Urban Research Corporation scans and evaluates the coverage of issues in newspaper articles. More than 200 newspapers from 159 metropolitan areas in the United States are monitored. The procedure for compiling a *Trend Report* involves categorizing articles into ten major subjects and 124 subtopics. The ten categories are education, employment, environment, government and politics, health, housing and urban development, human and economic relations, law and order, transportation, and welfare and poverty. The technique of content analysis is applied to the articles, measuring the increase or decrease of coverage of each topic. The actual number of lines of text given to each topic is a basis of measurement. These data, along with commentary specifically geared to business interests on each topic, are provided in each quarterly *Trend Report*. A quarterly subscription is $500.

U.S. Industrial Outlook

Many government agencies collect data on various time series, but only a few attempt extrapolation. Of these, even fewer attempt to look more than two or three quarters or years ahead. Obvious exceptions are the Bureau of the Census and the Bureau of Labor Statistics. Still another exception, although perhaps less widely known, is the Bureau of Domestic Commerce of the U.S. Department of Commerce, which publishes annually a document entitled *U.S. Industrial Outlook*. Issues published up to 1975 contained projections to the year 1980; beginning with the 1976 issue, projections were made to 1985. In all cases, several hundred indicators in particular industry sectors (e.g., chemicals, paper, steel), all arranged by SIC code, are forecasted. Though the methods used to derive these forecasts are not explained, a mathematical model of some sort is no doubt involved. What distinguishes the Bureau's projections from similar efforts is that an attempt is made here and there to assess the impact of potential social, technological, demographic, and even political developments on the future course of the indicators. Again, however, the approach used to identify and trace out these interrelationships is not explained. Copies may be obtained from the Superintendent of Government Documents for $5.45.

MAPTEK

Quantum Science Corporation, a New York based multinational information services company, maintains the MAPTEK Data Bases. At present there are eight data bases in the MAPTEK system, of which five provide forecasts. These five are:

1. MAPTEK Computer Equipment Market Forecast, which provides a five-year market projection of more than 95 categories of computer equipment in terms of average selling price, quantity sold, and total dollar volume. This data base also forecasts the usage of these equipments in 40 end-user products.
2. MAPTEK Electronic Component Market Forecast, which provides a five-year market projection in terms of average price, quantity, and total dollar volume of each component category. Also given is the usage of over 300 categories of electronic components in each of more than 450 equipment categories.

3. MAPTEK Computer Services Market Forecast, which provides a five-year projection of user expenditures and vendor revenues for the computer services industry. It covers 13 end-user industry sectors and 79 individual computer services categories.
4. MAPTEK European Computer/Communications Data Base presents five-year forecasts for computer equipment (by product type) and DP services for 17 European countries classified by 9 market groups and 13 research (industry) sectors.
5. MAPTEK Office Technology Forecast, which provides a five-year market projection in terms of average selling price, quantity, and total dollar volume of each office equipment category, structured by industry sector and office size.

These data bases are made available on a fee basis through the MAPTEK Associate Program. The service's aim is to provide primary research information for strategic and tactical market, product, and business planning to executives of the electronics industries.

Stanford Research Institute

One of the oldest and most important services available from the independent research community is the Business Intelligence Program (formerly known as the Long-Range Planning Service) provided by the Stanford Research Institute. This service, which was established in 1958, is available by subscription for $6500 per year for an initial two-year primary contract. Participation provides (1) new reports as they are issued (approximately 30 reports are published each year); (2) copies of previously published reports still in print (approximately 200 reports); (3) an inquiry service, which permits clients to ask for additional information about subjects addressed in current reports; (4) consultation with authors and professional staff (priviliges similar in scope to those of the inquiry service); (5) microfilm backup to research reports; (6) a Report Subject Index, covering all reports in print, including subject headings cross referenced to all pertinent reports, plus alphabetical and numerical listings of the reports; (7) executive summaries of each new report; and (8) occasional seminars or conferences devoted to themes of interest to many clients.

The Business Intelligence Program monitors and projects change as it relates to business strategy, operations, and management, drawing out implications of economic, social, technological, and political change for various segments of the business community. Reports cover developments worldwide, but selected topics are regionally oriented (e.g., North America, Europe, Japan). They alert executives to new business opportunities, trends, or threats, providing information on new processes, products, markets, and advanced planning techniques.

More than 540 reports have been published since the program began. Representative topics include Trends in Manufactured Housing, Econometric Forecasting in Business Planning, Environmental Policy and Impact Statements, Trends in the Western European Chemical Industry, Weather Forecasting, Nuclear Power Prospects, and Business Opportunities in the USSR.

MAP

In 1966 work began at the General Electric Company to develop a sophisticated computer-based data bank and forecasting system for the use of GE's corporate planners. The success of this effort, which was performed at TEMPO, GE's center for advanced studies, led the corporation to make the system available to outside users on a subscription basis. In this mode, the service is known as the Management-Analysis-Projection (MAP) System. The data bank of trend information open to MAP subscribers is one of the largest in the United States; it includes the following files:

—The GE Economic Forecast data bank, which contains quarterly and annual time series for 200 macroeconomic variables and selected indicators of the U.S. economy. Long-range annual forecasts cover the next ten years, while quarterly benchmark forecasts cover the next eight quarters.
—The NBER data bank of more than 2000 time series, which is maintained by the National Bureau of Economic Research. It covers all National-Income Account series published in the *Survey of Current Business*, all cyclical and economic indicators published in the Commerce Department's BCD and CEA Economic Indicators, plus many more series on prices, employment, banking, and other topics.
—Federal Trade Commission and Security and Exchange Commission data banks, which contain quarterly financial statements representing the aggregate of all enterprises classified as manufacturers who are required to file U.S. Corporation Income Tax Form 1120.
—The NPA regional economic and demographic data

bank, which is maintained by the National Planning Association and contains historical data plus five- and ten-year projections for eight regions, all 50 states, the District of Columbia, and 230 Standard Metropolitan Statistical Areas.

— A regional Industrial Production Index data bank, which is maintained by U.S. Engineers and Consultants, Inc. and provides two types of monthly data: an index of activity by state and manufacturing industry, and a measure of the contribution of each industry in each state to the Federal Reserve Index of Manufacturing.

— The Southeastern Regional Data Base, which is maintained by the Southeastern Economic Project and contains detailed regional information on the Southeastern economy.

— The Standard Industrial Classification data bank, which comprises series on ten measures of business activity (such as employment and shipments) for over 500 industries.

— Specialized industry files, including those of the Association of Home Appliance Manufacturers and the National Electrical Manufacturers Association.

MAP users have full access to these data. Moreover, they may also introduce parameters of their own, reflecting conditions unique to their own organizations or special assumptions about future prospects. (No one else has access to these original data while they are in the system.) When the user has defined a problem, selected the time series of interest to him, and entered his own data, he then can exploit the forecasting capabilities of the MAP System, which has a variety of built-in extrapolation procedures. MAP is highly flexible and easy to use; for these reasons, plus the richness of its data base, it may be the most powerful tool of its type in the United States today.

In addition to MAP, GE has offered MAPCAST, an information service based on GE's own projections about future economic and business developments in the United States. While this service is no longer available to new subscribers, plans to make it available again were being considered in 1975. MAPCAST is thus being used primarily by GE personnel. The previous (and possibly revived) MAPCAST service has four parts: (1) the MAPCAST *Quarterly Review of Economic Prospects* (a journal that discusses the possible course of existing and emerging trends and gives baseline and contingent extrapolations of major economic indicators); (2) the MAPCAST *Mid-Quarter Review* (a periodic newsletter that notes any deviations from the baseline projections in the preceding *Quarterly Review* and explores their significance); (3) two-way communication with GE (the opportunity to phone GE economists for further information about the projections in the preceding *Quarterly Review*); and (4) special in-depth studies (the opportunity to commission GE personnel to perform proprietary research on specific markets and industries). In its earlier promotional literature, GE emphasized that the trend forecasts provided in MAPCAST were derived through wholly new techniques, as well as "unique combinations of more advanced [versions of conventional] methods." Provision could be made for including "the likely impacts of changes in governmental policies, public attitudes, and the interaction among industries"; in ways not specified, "considered expert judgment" on these matters could also somehow be factored into these extrapolations.

Institute for Trend Research

The Institute for Trend Research (ITR) is a private research corporation organized to increase scientific knowledge of economic trends and patterns. *The 35 Pressure Indicators* is a monthly publication of ITR and is available by subscription for $150. For each of 35 individual series (including housing starts, stock prices, textile shipments, GNP, short-term interest, and auto sales) trend forecasts are prepared monthly. These charts have been published since 1970, and ITR claims to have determined that the 35 "pressure" indicators turn upward in approximately the same order, cycle after cycle. In light of this conclusion, these indicators are offered as an up-to-date description of the current business situation and as a means of identifying cyclical changes that may be developing.

National Planning Association

The National Planning Association (NPA) publishes three reports that are of interest here: (1) the Regional Economic Projections Series (REPS), (2) the National Economic Projections Series (NEPS), and (3) *Looking Ahead & Projection Highlights*. The price for REPS and NEPS is $350 per year; a joint subscription to both is $600. *Looking Ahead & Projection Highlights* is free with membership in NPA. REPS is an annual that provides 5-, 10-, and 15-year forecasts of economic and demographic information for eight general regions, all 50 states, and more than 240 metropolitan areas in the United States. Among the variables projected are population and labor force size by age and sex, personal income, personal consumption expenditures for 83 consumer product items, and housing stock by occupancy status and structural characteristics. REPS undertakes to forecast the entire national system of small areas simultaneously.

> The projection system works through a series of iterations and successive approximations, in which a small area's projected share of the national total of employment in certain so-called "basic" industries is a prime determinant of employment in other secondary and tertiary sectors in that area and therefore net migration, while conversely, the population of that area and its surrounding "market" areas helps determine demand for goods and services in the public sector and in the secondary and tertiary industries.[6]

REPS is derived from results presented in the national survey, NEPS, which is also an annual. NEPS looks 5, 10, and 15 years ahead at such subjects as GNP by major components, industry sales, exports and imports by industry group, output and employment, investment, government spending and revenues, personal income by type of source, personal expenditures by product categories, and so on. The long-range projections produced at NPA are generated by a series of models, some maintained by NPA and others by Chase Econometric Associates. Some modification of CEA's assumptions is done as deemed necessary in the judgment of the analysts. Table 1, drawn from an NPA flyer, compares some of the organization's earlier long-range projections with actual outcomes.

Looking Ahead & Projection Highlights is intended as a supplement to REPS and NEPS. Each issue of this newsletter focuses on a particular subject addressed in REPS or NEPS (e.g., "Income and Consumption in Metropolitan Areas: Projections to 1985"), summarizing key findings from these reports with a minimum of statistical and economic jargon.

McGraw-Hill Publications

The Economics Department within McGraw-Hill publishes numerous report series that present (and provide interpretations of) business and economic trends, along with analyses and industry studies. The publications of the Economics Department include:

—*Annual Survey of U.S. Business' Plans for New Plants and Equipment:* Planned capital investment data for 26 major industries for four years ahead. Breakdowns of investment into buildings, motor vehicles, and machinery and equipment into

Table 1
Comparisons Between Previous NPA Economic Projections and Actual Developments, 1950, 1960 and 1965[1]

(dollars in billions)

Indicator	1950[2] Actual	Projected 5 years earlier	Projected minus actual as a percent of projected	1960[3] Actual	Projected 8 years earlier	Projected minus actual as a percent of projected	1965[4] Actual	Projected 10 years earlier	Projected minus actual as a percent of projected
GNP	$163.5	$170.0	3.8%	$419.5	$425.0	1.3%	$624.6	$632.9	1.3%
Personal consumption expenditures	109.0	116.3	6.3	286.4	291.1	1.6	407.7	408.8	0.3
Gross domestic investment	26.6	25.3	-5.1	57.0	55.9	-2.0	97.9	92.2	-6.2

Source: NPA

[1] The comparisons between projected and actual developments have been adjusted for periodic revisions in the national income and product data which have occurred in the postwar years. The comparisons shown above are based on the definitions prevailing in the year in which the projection was made.

[2] *National Budgets for Full Employment* (Washington, D.C., National Planning Association, 1945). Both the actual and projected GNP data are expressed in terms of 1945 constant prices.

[3] Gerhard Colm, *The American Economy in 1960* (Washington, D.C., National Planning Association, 1952). The actual and projected GNP data are expressed in terms of 1951 constant prices.

[4] *Long-Range Projections for Economic Growth. The American Economy in 1970* (Washington, D.C., National Planning Association, Planning Pamphlet No. 107, 1959). The GNP data are expressed in terms of constant 1958 prices.

modernization versus expansion, and by six major regions are also included. Data on capacity trends, operating rates, sales, and price and employment expectations are also available in the report. $8.50.

—*Detailed Industry Breakdowns:* Data for approximately 50 3- and 4-digit SIC industries (unpublished figures) are available for trends in capital spending plans and sales forecasts for four years ahead. $35.

—*Capital Spending Forecast for Four Years:* Forecast of capital expenditures for 23 major industries and groups for four years ahead, based on mathematical relationships between McGraw-Hill survey results and actual data. Issued in June of each year. $35.

—*Preliminary Survey of U.S. Business' Plans for New Plants and Equipment:* Planned capital investment data for 26 major industries for two years ahead. Sales and price expectations are included. $7.50.

—*Detailed Industry Breakdowns:* Trend data for approximately 50 3- and 4-digit industries (unpublished figures) are available for trends in capital spending plans for two years ahead. $35.

—*Capital Spending Forecast for Two Years:* Forecasts of capital expenditures for 23 major industries and groups for two years ahead, based on mathematical relationships between McGraw-Hill survey results and actual data. $17.50.

—*Annual Survey of Overseas Operations of U.S. Industrial Companies:* Planned capital investment data for 14 major industries in eight major areas of the world for two years ahead. Breakdowns of overseas investment into property, plant, and equipment, as well as data on capacity trends and operating rates are also included. Sales and profit expectations are also covered. $8.50.

—*Summary of U.S. Industrial Companies' Spending Plans:* U.S. industrial companies' total planned capital expenditures both at home and abroad for two years ahead. $8.00.

—*American Economy, Prospects for Growth to 1988:* Economic and industrial projections to 1978, 1983, and 1988. Regional data on population and income are also included. Approximately 60 pages including growth tables covering nearly 175 3- and 4-digit SIC industries. $50.00.

—*U.S. Business Outlook, Short-Term:* Key factors in the short-term business outlook along with a detailed table of forecasts of significant economic indicators. Issued four times a year—January, May, August, and November. $6 per report.

—*U.S. Business Outlook, Long-Term:* An annual updating of long-term trends covering 15 years ahead or more, including two pages of general tables. $6.

—*World Business Outlook:* Annual economic forecasts for most of the major industrial nations of the world. Includes detailed statistics on gross national product and industry trends. $17.50.

—*World Business Review and Outlook at Mid Year:* An update of annual economic forecasts for the next year for most of the major industrial nations of the world. Includes statistics on gross national product and overall industry trends. $17.50.

—*A Decade of Growth in Western Europe to 1984:* Economic projections for 14 Nations in Western Europe for 1979 and 1984. Includes detailed statistics on gross national product and population. $10.

—*Migration of Industry:* Projections of employment for 1977 and 1982 for 22 major manufacturing industries in six major regions of the United States. $12.50.

—*Annual Survey of Pollution Control Expenditures:* Pollution control expenditures of 26 major industries for the current year and third year ahead. Breakdowns of air versus water pollution control expenditures, as well as total costs of bringing each industry's existing facilities up to current standards, are also included. R&D expenditures for pollution control are also covered. The results are based on data gathered in McGraw-Hill's annual survey of business' plans. $7.50.

—*Annual Survey of Investment in Employee Safety and Health:* Employee safety and health expenditures of 26 major industries for the current year and third year ahead. Results are based on data gathered in McGraw-Hill's annual survey of business' plans. $6.

—*Annual Survey of Corporate Profit Trends:* Profits, profit margins, and financial data for 22 industrial categories for the year ahead. Results are based on a survey of financial executives in industry. $7.50.

-*Annual Survey of Research and Development Expenditures:* R&D expenditures in 20 manufacturing industries and in nonmanufacturing as a whole for the current year and the third year ahead. Data on new products as a percentage of sales four years ahead are also included. Breakdown of Federal versus private funding of R&D is covered. Results are based on data gathered in McGraw-Hill's Annual Survey of Business' Plans. $7.50.

The Survey Research Center at the University of Michigan

The Survey Research Center conducts a quarterly survey of consumer sentiment. The survey, which involves a representative sample of U.S. households, is intended to quantify the attitudes of the U.S. consumer toward the outlook for business conditions, unemployment, inflation, interest rates, and buying conditions. This "Index of Consumer Sentiment" is often reported in the more general publication, *Economic Outlook U.S.A.*, which is published quarterly by the Survey Research Center. According to the Center, the aim of this publication is to "aid decisionmakers in achieving a better understanding of the economic and social environment" by providing analyses that incorporate "direct measurements of the expectations, attitudes, and plans of both consumers and business firms with the economic and financial variables traditionally used in forecast models." Articles are devoted to such topics as food prices, business investments, attitudes toward violence, and fertility trends. An annual subscription is $18.

The Conference Board

The Conference Board, despite its size, resources, and support by industry, does surprisingly little forecasting. In 1974, however, it began a series of studies to examine future trends and developments in specific industry sectors (e.g., primary metals); these studies are developed through use of the Maryland Interindustry Model, an input-output model. One of the Board's other efforts is a bimonthly one-page summary report entitled *Consumer Attitudes and Buying Plans*. Based on surveys conducted for the Board by National Family Opinion, Inc., these reports examine expectations over the coming six months regarding charges in business conditions, employment, and income, as well as plans to purchase cars, homes, and major appliances. Questions are also answered about vacation plans. This series is comparable to those prepared by many other organizations, notably the Survey Research Center at the University of Michigan.

CAP Reports

The Institute for the Future, an independent nonprofit research corporation dedicated to comprehensive studies of the long-range future, is the originating agency for two services which regularly monitor the future. One service is Project AWARE; the other is the Corporate Associates Program (CAP). Project AWARE is discussed later in this chapter. The Corporate Associates Program provides information about changes in the corporate environment and in planning techniques. The primary means of dissemination is the bimonthly CAP Report. The reports are very brief (approximately six pages); among the titles that have been published since the program's beginning in January 1975 are the following:

— CR1-3, "Attitudes and Value Change—Impact of Future Energy Crises"; "Strategic Planning: Eliciting Expert Information."

— CR1-2, "Equal Employment Opportunity: The Long-Range Impact on Corporation"; "Strategic Planning: Identifying Critical Issues."

— CR1-1, "World Interdependence: A Revaluation"; "Strategic Planning: Penetrating the Corporate Barriers."

ADL IMPACT Program

Arthur D. Little, Inc. is a profitmaking consulting organization. One of its services is the ADL IMPACT Program. The total program includes briefings, *Outlook Reports* on topics of major interest (energy, chemicals industry, metal resources, etc.), *Comment Letters* (which tend to be shorter analyses of narrow topics or fast-breaking developments), and personal consultation. The annual fee for the program is $7500. Single reports may be purchased for approximately $1000 and *Comment Letters* are available for $250 per letter. *Outlook Reports* and *Comment Letters* assess the impact of developments—economic, social, regulatory, technical, and commercial—which have a direct bearing on the businesses of clients.

Yankelovich Monitor and Corporate Priorities

These services are provided by a private firm, Yankelovich, Skelly, and White, which is engaged largely in polling. The first, *Yankelovich Monitor*, is an ongoing research program which projects 35 social trends organized into five broad categories:

1. Psychology of Affluence Trends.
2. Antifunctionalism Trends.
3. Reaction against Complexity Trends.
4. Trends Relating to the Weakening of the "Protestant Ethic."
5. Trends Reflecting Permissiveness in Child Rearing.

Monitor studies are intended for use in planning future marketing moves and for understanding how consumers currently buy and use products, and how they respond to advertising, packaging, promotions, and other marketing tactics. The initial entry fee for sponsors is $18,800. Thereafter a yearly fee of $13,800 entitles sponsors to receive this proprietary service.

The second Yankelovich service is *Corporate Priorities*, a continuing study designed to highlight and to analyze possible resolutions to the conflicts between the competitive market system and pressures on business coming from the ecology movement, the consumer protection movement, government, the youth movement, and similar sources. *Corporate Priorities* is proprietary to its sponsors, who pay a fee of $24,000 for the first year and $22,000 yearly thereafter. *Corporate Priorities* is designed to assist corporate management in adapting realistically and with foresight to new public demands and issues which are challenging traditional business concepts and corporate organization. The study measures 30 demands and 150 indicators of demands by means of personal interviews among key leadership groups and a large-scale sample of the general public. A total of approximately 3200 individuals are surveyed. The respondent groups include stockholders, college students, union members, city officials, federal legislators, state agency executives, media executives, and officials in activist organizations. In recent advertising literature, Yankelovich, Skelly and White point out that *Corporate Priorities* goes beyond measuring "support" for a public policy issue which can affect business autonomy, and uncovers the degree to which an issue has been crystallized, the role seen for business, types of solutions under consideration, recognition and acceptability of "trade-offs" involved, and the vulnerabilities of specific products and industries.

Futures Conditional

In 1973, Robert Theobald began publication of *Futures Conditional*, a highly idiosyncratic monthly intended, in Theobald's words, to explore "the interlinked issues raised by the transition from the industrial era to the communications era." *Futures Conditional* has described itself as a "participatory trendletter" rather than as a magazine or a conventional newsletter, primarily because readers are urged to help shape the contents of each issue. There are no limitations on subject matter; the first issue, for example, contained an interview on the need to explore the future, a survey of problems facing the underdeveloped countries, a parable on the myth of creation, a report on problem solving by a community in the Minneapolis area, a description of a program for Canadian youth, etc. Subsequent issues have considered equally diverse subjects. Also included in each issue is a list of organizations or projects that readers might wish to take part in.

Since the beginning of 1975, *Futures Conditional* has been published by the Northwest Regional Foundation. An annual subscription is $20. It is now described as "an information service to help you organize programs to get people thinking about the future, to give you ideas to think about and discuss, and to direct you to other resources. The publication is specifically designed for individuals and groups and communities who are using the bicentennial for thinking about and planning for their future." Despite its wishes to the contrary, *Futures Conditional* is essentially a magazine, but it occupies a previously unfilled niche between some of the general newsletters described here and *The Futurist*, the Journal of the World Futures Society.

Footnotes to the Future

Futuremics, Inc., a Washington-based organization formed originally to advance Alvin Toffler's ideas in *Future Shock*, publishes the newsletter, *Footnotes to the Future*. An annual subscription is $15. *Footnotes* seeks to alert readers to new books and articles, forthcoming meetings, and the availability of services provided by futurists (including some competitors of Futuremics). Only a few prospective trends and developments are mentioned in a typical issue; these appear under the headline "News Notes." In all of these respects, *Footnotes* is comparable to several other services, such as *Alert*, published by the Research Institute of America.

Bulletin of the World Future Society

The World Future Society (WFS) publishes both *The Futurist* and its bimonthly *Bulletin*. The WFS, a nonprofit organization founded in 1966, is an association for the study of alternative futures. The *Bulletin* is coming to serve as the professional journal of the WFS, presenting current theories and results of futures research, as well as book reviews and news items about the current activities of WFS chapters and about people who are conducting research on the future. In many respects, the *Bulletin* is comparable to *Footnotes to the Future*, yet its larger size allows for longer articles on futures research theories and applications to be presented.

Future-Abstracts

A service introduced by Futuremics, Inc. late in 1975 is *Future-Abstracts*. It is designed to provide a concise system for accessing information on futurist publications, programs, and people. Abstracts of relevant publications (including books, magazines, films, tapes, games, and bibliographies) are prepared and published on 5 by 8 in. file cards. Descriptions of organizations dealing with the future and bibliographical abstracts of priminent futurists are also included. Approximately 48 cards are distributed to subscribers each month. An annual subscription is $110.

Future Report

One of the most ambitious newsletters in the field is *Future Report* (formerly *Tomorrow's World*), published every third Monday by an organization called Foundation for the Future. in Cambridge, Massachusetts. Each issue consists primarily of an unstructured list of predictions, projections, plans, patents, anticipated developments, summaries of research studies, and research results. Many of the items are drawn from published sources, which may or may not be cited. A typical issue also includes one or more brief essays ("profiles") which bring together many anticipated developments in a major subject area (e.g., defense or transportation). The orientation of the newsletter is technological, but political, economic, and social developments increasingly are being interwoven in the "profiles" and in the list of forecasts. The annual subscription fee is $36.

Food Industry Futures

Another newsletter available is *Food Industry Futures* published twice a month by Executive Strategy Services Co. This timely, interesting, and informative publication contains summaries of reported developments and activities affecting the food industry, as well as specific forecasted events published in other sources. All areas related to the food and beverage industry are dealt with, including food supply, consumer preferences, crop yields, fertilizers, and labor. Results of studies conducted by government agencies such as the U.S. Department of Agriculture, and private institutions concerned with food are reported. Meetings and conferences are also discussed and books of interest are reviewed. An annual subscription is $50.

Technology Forecasts and Technology Surveys

The best newsletter covering emerging trends in technology is *Technology Forecasts and Technology Surveys*, published 12 times a year by the Professional Writers Group since 1969 and available by subscription for $62 per year. The aim of this publication is to report on work in progress in business and industry, the universities, government, and independent research organizations. Virtually all of the items presented reflect original research by the staff, usually through interviews. Hence, *Technology Forecasts and Technology Surveys* is an excellent "early warning system." A variety of fields is reported on in each issue. Topics such as energy, micrographics, plastics, X-ray scanners, futures research, and consumer patterns may be treated in a single issue.

* * * *

This survey of trend watching organizations and services, while superficial and assuredly incomplete, does illustrate the principal types of activities being undertaken or contemplated today to help bring more of a futures orientation into the conventional monitoring system. We turn now to examine in more detail four other services which are distinctive in several important ways: not only do they involve special approaches and unique data banks, but, significantly, they are offered by organizations having a full-time concern with the study of the future. In and of themselves, these services are comprehensive "futures monitoring" systems, though they are different from each other in perspective, scope, and objective. The four are the AWARE program of the Institute for the Future, the PREDICASTS® service of Predicasts, Inc., and the SCOUT® and PROSPECTS® services of The Futures Group.

Project AWARE

One of the very few "futures monitoring" services that brings the immediate intended user into the process of forecasting so that he can help shape the final product is the Institute for the Future's Project AWARE. This is a multiclient effort available by subscription ($40,000 a year for each of three years). It is supported presently by four or five major private corporations in the United States.[7] The immediate users in this case are key executives and planners in these organizations. Each year's work involves these persons and outside experts in the identification and systematic assessment of specific potential events and trends within a broad framework of five categories: technology, society, government regulations, economics, and the international environment. Within each of these categories, problems as diverse as business practices, health care, transportation, basic science, energy, and food resources are singled out as topics of particular concern. (In the first year's program, some 70 participants found well over 1000 specific issues in these areas, of which 73 were dealt with in detail.) Additionally, attention is paid to the forces that may influence the nature and likelihood of the forecasted possibilities, and to policy options that may be worth attention if these events do or do not occur. Each client receives a common annual report on the results, as well as a report that tailors these results to his company's unique concerns. The common report, divided into separate documents on major themes, will be made available to the public beginning in 1976 in a bimonthly series of short reports, entitled PERSPECTIVES.

Project AWARE concerns itself with futures that lie at least five years ahead, centering aroung 1985. The principal method of investigation, at least during AWARE's first year, involved a carefully designed sequence of interviews, data collection, polling of outside experts (usually through questionnaire), data integration and analysis, and reporting. Of greatest interest, perhaps, was the ingenious structure of the interviews and of the poll of outside experts.

The purpose of the interviews, which were two-part, individual, hour-long sessions with as many as 20 persons from each of the client organizations, was not to identify directly the specific threats and opportunities most urgently requiring forecasting, but rather to gain insight into the *kinds* or *categories* of information that the interviewees would most like to have—and to determine the potential value of this information if it actually were in hand. To these ends, the interview moved through the following three activities, the first two in the initial interviews, and the third in the follow-on interview:

1. During the first few minutes of the interview, the interviewee was asked to pretend that he was with a clairvoyant (i.e., a person with perfect information about the future), and to pose as many questions as he wished about futures important to him. The typical interviewee, after some initial hesitation, generated some 15 questions, and these, it appeared in subsequent analysis, fell into rather plausible categories, such as regulatory behavior by government, competitive threats from abroad, the availability of energy, and so on.
2. Next, the interviewee was asked to create two scenarios, one "dismal" and one "rosy" in its impact on his corporation if it actually materialized. The interviewees were asked to define each of these two worlds as consisting of a set of trends and events such that there was a 10 percent chance that the actual future world might be even better (in the "rosy" case) or worse (in the "dismal" case) than the worlds he described. The Institute found that while most of the interviewees were readily able to define the dismal world, they had greater difficulty with the rosy one—a result which would probably be true today of the answers that would be given by most persons, though it stands in some contrast to the often observed optimism of futures researchers themselves.
3. Finally, the interviewee was asked to review the collated and analyzed responses on the first two questions from all of the individuals in his corporation, including his own. From these he selected a subset within the control of his company, especially those which, if not dealt with, would have a significant financial impact on the company's operations in the period of interest.

Thus, out of the interviews came lists of trends and events sorted as to relevance to the particular corporation and as to actionability by that corporation. From the rank-ordered lists for each client, dropping the items repeated in two or more organizations, it was possible to derive a list of 73 issues for further study. With the assistance of outside specialists, the Insitute then examined each issue in order to identify the specific questions that seemed necessary to ask if the issue were to be explored in depth. (Among the kinds of questions explicitly ruled out in this step were those that were judged to have "already been answered in the published literature"; presumably, the references in such cases were passed along to the clients, though it is not clear from published subscriptions of Project AWARE that this was done.) The results were incorporated into a questionnaire, which was administered, after one intervening step, to a panel of outside experts selected jointly by the Institute and the clients.

The intervening step represents one of the most interesting aspects of the AWARE project and may, with a good deal of further experimentation, be a prototype for similar kinds of investigations performed by others. It involved meeting with the experts individually and testing them on their "assessing ability." The test made use of 20 almanac questions, the answers to which could be looked up but were almost certainly unknown to any of the candidate respondents (e.g., What is the longest jump ever recorded for a flea?). For each question, the respondent was asked to estimate the range of values which would have at least an 80 percent chance of including the correct value. An ideal respondent would have provided the proper range for at least 16 of the 20 questions. As it turned out, in the first application of this test, only one of the experts did so; the average was about 6 questions out of 20. These results were used by the Institute to compute a "correction factor" for each expert, so that his quantitative answers on the questionnaire itself could be adjusted to reflect the likelihood that he might be outside of the range that would include the "right" answer.

On the questionnaire, the respondents were not only asked to answer questions about the 73 issues, but also to indicate their substantive expertise on each question, using a 5-point verbal scale that was later translated into logarithmic values, with "expert" receiving a 16 and "unfamiliar" a 1. Thus, in the analysis of the responses to derive a consensus judgment from the respondents as a group, the Institute was able to take each estimate, adjust it with the appropriate "correction factor," and then weight it by the "expertise" value. Final values for the group were then normalized; from the normalized estimates, the expected value of each forecast, as well as its 10, 50, and 90 probability percentiles, was computed.

Nonquantitative information was also generated through the questionnaire, of course. The analysis of this material and the derivation of consensus were performed judgmentally; an effort was made to give greater weight to the self-appraised "expert" answers, though this necessarily had to be done subjectively by the investigators.

Project AWARE certainly will undergo substantial development in the years ahead as new clients are added and the methodology is refined. As one of the most ambitious of the systems for monitoring and coming to grips with the challenges of the future, no doubt it will be watched closely to see if it escapes the fate of other extremely broad forecasting systems (such as TRW's ill-starred PROBE experiment), especially on the questions of credibility and utility. If early press reports are any indication, interpretation also may prove to be an issue. Thus, two reporters viewing the same results saw some of them quite differently. One said, for example, that AWARE forecasted full socialization of medicine by 1982 and an unlocking of "the mysteries" of wrinkling and baldness by 1985. The other gave the same events probabilities of 90 percent and 40 percent respectively, by 1985.[8] More seriously, actual users may be forced to wonder about the meaning of such vaguely expressed forecast statements as "Community review of factory locations" or "Many chemical pesticides phased out" or "Wide use of computers in elementary schools." A basic continuing problem in futures research is the imprecision (vagueness or ambiguity) of forecast statements. Few organizations have been as attentive to this problem as the Institute, and it may be expected that the staff will be more successful as time passes in dealing with it in the AWARE program, which in many respects promises to fill important needs.

PREDICASTS

Predicasts, Inc., located in Cleveland, Ohio, describes itself as "the leading integrated business information-research firm in the world." It publishes reports on the technology and business outlook in particular industries or product groups; performs original studies on a proprietary basis for individual clients; offers special library services (such as custom bibliographies); and issues several abstract journals (e.g., *Chemical Market Abstracts* and *Electronics and Equipment Market Abstracts*, as well as a series, under the general title PREDI-BRIEFS, which involves a separate bulletin in each of some 35 areas, from agricultural chemicals to transportation electronics). Predicasts also publishes the well-known F&S Indexes (five different titles), and, what is important here, publishes two major forecast series, PREDICASTS and WORLDCASTS.[9] Because PREDICASTS and WORLDCASTS differ significantly only in geographical coverage, the former being limited to the United States and the latter covering the rest of the world, the following discussion of PREDICASTS can be taken as a description of WORLDCASTS as well. The annual subscription fee for PREDICASTS is $375; for WORLDCASTS it is $600.

PREDICASTS (like all of the other services provided by the company) is based on a regular reading of about 1000 trade, professional, and business journals. When forecasts on particular economic matters or manufactured products are encountered, they are extracted by readers who record the source and date, translate them into common units (e.g., short tons rather than long or metric tons), rewrite them in a standard single-line format, and arrange them by SIC code number. PREDICASTS reports appear quarterly; each typically includes 10,000 such business forecasts. A portion of a representative page is shown in Table 2.

No claim is made as to the accuracy or reliability of such individual forecasts, but the company does argue that these data are, in general, "more accurate than the sources from which they come"—thanks to the fact that in the five editing stages through which each item passes, the meaning of terms can be clarified, typographical errors in the original can be caught, and authors can be contacted in especially perplexing cases. On the other hand, the very act of publishing these forecasts does carry with it some implicit assumptions that are crucial. First, there is the assumption that the data about the future that are of greatest value or significance to the user are precisely those market-related quantitative forecasts that fit within the SIC scheme. This is patently absurd, as Predicasts would admit, but the opposite impression is reinforced by systematically excluding all forecasts of social, political, administrative, scientific, value-related, and other kinds of change pertinent to business affairs. Second, it is assumed that all of the included events are equally probable; indeed, they are treated *as if* their probability were unity. This, too, is false, as the sources themselves would be the first to say. Finally, it is assumed that all of the sources are equally credible—an especially dangerous notion, if believed. A sophisticated user would of course, be aware of these dangers; the only problem is that a novice (or even an experienced planner unaccustomed to thinking in terms of probabilities) would not.

The second of the two sections in a PREDICASTS report builds on these forecasts in order to provide what the company calls a "composite forecast" of each of 500 key time series out to 1985. Here a claim is made—namely, that these are "the most accurate" forecasts of any on the same time series from any source. We shall return to this remarkable assertion after reviewing the method used to prepare a composite forecast.

According to Predicasts, six types of information are taken into account in their forecasts:

1. Historical data on the relevant indicator. (These data, particularly on rates of change in the indicator, come into play only in the final stage of projection, and serve to provide a check on the plausibility of the projection. If the forecast is judged to be unreasonable—that is, too far out of line with the historical record—it is done over again.)
2. Published forecasts on the indicator in question. (These are forecasts of the sort described above.)
3. Published forecasts of the components of the indicator in question. (For example, if the series of interest were domestic consumption of rubber, the component indicators would include synthetic rubber production, imports, exports, and so on.)
4. Published forecasts of broader aggregates of the indicator in question. (For rubber consumption, these might include automobile tire requirements per car, automobile registrations, replacement tire demand, and so on.)
5. Mathematical relationships between forecasted indicators that are causally related. (These estimating relationships are the key to the exploitation of the component and aggregate forecasts.)
6. Mathematical or judgmental relationships between analogous indicators and the data of 2, 3, 4, or 5. (These analogies consider either rates of substitution of one product for another or "international crossovers," i.e., the extent to which developments in one country or area may be a precursor of similar developments in the country of interest.)

With reference, then, to this list of elements, a composite forecast is made as follows: First, the published forecasts (item 2) are averaged; this result is set aside for the moment. Next, each set of component forecasts (item 3) is averaged and the results are then analyzed using the relationships of items 5 and 6 to derive a value for the indicator in question. The same is done for the aggregate forecasts (item 4), at which point the two estimates are compared. If they turn out to be different, adjustments are made on either side. Once the two are brought into agreement, the projected value is compared to the averaged projection for the indicator itself (item 2) and to the historical growth rate for the indicator (item 1). If these comparisons prove implausible, the entire process is repeated. Some actual composite forecasts are presented in Table 3.

Obviously, this technique, which has been used under other names by other researchers,[10] depends heavily on the quality of both the estimating

Table 2

Types of Information Included in the "Abstracts" Section of a *Predicasts* Report

WHAT PRIMARY PRODUCT? — STATUS, OR WHAT HAPPENS? — AFFECTING WHAT? (WHEN APPROPRIATE) — BASE PERIOD DATA: YEAR, QUANTITY — LONG RANGE FORECAST: YEAR, QUANTITY

SIC NO	PRODUCT A	EVENT	PRODUCT B	YEARS B	YEARS S	YEARS L	QUANTITIES B	QUANTITIES S	QUANTITIES L	UNIT OF MEASURE	SOURCE JOURNAL	SOURCE DATE	SOURCE PAGE
	Polystyrene (Cont)												
28213 105	Polystyrene	used in	furniture	69	70	75	75.	85.	200.	mil lbs	C&E News	11/16/70	18
28213 105	Polystyrene	used in	furniture	-	-	75		-	250.	mil lbs	Mod Plast	1/ /71	44
28213 105	Polystyrene	used in	furniture	69	70	75	75.	90.	300.	mil lbs	Mod Plast	8/ /70	60
28213 105	Polystyrene	used in	furniture	70	75	80	90.	200.	600.	mil lbs	Plast Tech	9/ /70	40
28213 105	Polystyrene	used in	molding & extruding	70k	-	75	2000.	-	3440.	mil lbs	OPD Rep	5/17/71	11
28213 105	Polystyrene	used in	injection molding	70	-	75	1500.	-	2500.	mil lbs	Mod Plast	12/ /70	47

SIC CODING WITH ALPHABETICAL ACCESS — INDICATES TIME PERIOD IF NOT CALENDAR YEAR — GIVES DETAILED DISCUSSION — BACK PAGES TELL WHO SAID IT

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Table 3
Typical Composite Forecasts by Predicasts

GROSS NATIONAL PRODUCT

ITEM	HISTORICAL 1957/9	1960/2	1963/5	1966/8	1970	1971	1972	1973	FORECASTED 1975	1980	1985	ANNUAL GROWTH 1960/70
Billion Dollars at 1970 Prices												
GROSS NATIONAL PRODUCT	623.1	685.3	786.5	919.2	976.4	1001.2	1063.5	1122.0	1215	1473	1749	4.2%
PERSONAL CONSUMPTION EXPENDITURES	383.3	423.0	485.6	560.5	616.8	639.3	675.5	711.0	769	936	1115	4.3%
Durable Goods	44.7	50.1	65.1	82.0	90.5	100.3	112.0	119.5	128	158	189	5.7%
Nondurable Goods	181.2	196.2	217.6	244.5	264.4	269.6	281.5	294.5	314	373	434	3.5%
Services	157.4	176.7	202.9	234.0	261.8	269.4	282.0	297.0	327	405	492	4.5%
GROSS PRIVATE DOMESTIC INVESTMENT	91.4	98.6	119.1	138.2	137.1	143.5	162.0	171.5	184	221	258	4.9%
Residential Structures	31.0	31.4	34.0	30.3	31.2	40.7	48.0	44.5	44	51	50	5.0%
Nonresidential Structures	25.9	26.8	30.1	35.6	36.0	34.8	34.5	36.5	40	46	51	2.5%
Producers Durable Equipment	32.9	35.8	46.8	61.2	64.9	64.9	73.5	81.5	91	113	141	5.7%
Change in Business Inventory	1.7	4.6	8.2	11.1	4.9	3.1	5.5	9.0	9	11	14	8.4%
NET EXPORTS OF GOODS & SERVICES	3.9	6.0	8.6	4.1	3.6	1.1	1.0	1.0	3	4	5	1.1%
Exports	29.4	34.2	42.6	51.4	62.9	63.4						xx%
Imports	25.5	28.2	34.0	47.2	59.3	62.3						xx%
GOVERNMENT PURCHASES GDS & SERVICES	144.5	157.8	175.2	216.3	219.0	217.3	227.0	238.5	259	312	373	3.6%
Federal Purchases	78.3	82.5	87.3	108.5	96.5	90.7	93.0	93.5	96	104	112	.8%
National Defense	68.0	68.3	67.1	85.8	75.1	66.2	67.0	65.5	65	67	69	1.1%
Other Federal Purchases	10.3	14.2	20.2	22.7	21.5	24.4	62.0	28.0	31	37	43	5.6%
State & Local Purchases	66.2	75.2	88.0	107.8	122.5	126.6	134.0	145.0	163	208	261	5.4%
DISPOSABLE PERSONAL INCOME	417.3	456.1	527.7	618.7	689.5	717.3	748.3	794.0	843	1025	1220	3.9%
Billion Current Dollars												
GROSS NATIONAL PRODUCT	457.3	528.0	635.9	802.7	976.4	1050.4	1151.5	1261.5	1442	1878	2665	7.3%
PERSONAL CONSUMPTION EXPENDITURES	294.2	338.5	403.0	498.2	616.8	664.9	721.0	784.0	890	1219	1641	7.0%
Durable Goods	41.0	46.3	59.8	76.0	90.5	103.5	116.5	127.0	139	183	233	7.3%
Nondurable Goods	140.8	156.6	179.5	217.6	264.4	278.1	29.8	324.0	363	479	623	6.1%
Services	112.4	135.6	163.7	204.6	261.8	283.3	305.5	333.0	388	557	785	7.8%
GROSS PRIVATE DOMESTIC INVESTMENT	68.1	76.5	96.4	121.3	137.1	152.0	179.0	196.5	224	309	405	8.5%
Residential Structures	22.2	23.6	27.1	26.7	31.2	42.6	53.0	51.0	55	77	90	9.5%
Nonresidential Structures	17.1	18.6	22.1	28.9	36.0	38.4	42.0	46.5	55	76		
Producers Durable Equipment	27.3	30.5	40.1	55.6	64.9	67.4	78.0	89.0	103			
Change in Business Inventory	1.5	3.9	7.1	10.0	4.9	3.6	6.0					
NET EXPORTS OF GOODS & SERVICES												
Exports												
Imports												

relationships and the underlying model of the input and output characteristics of the "system" being explored. While Predicasts declares that "everything is related to everything," an assumption that most forecasters would share, the company has not revealed the details of its model. As noted before, however, the company does declare that its composite forecasts have no peers in confirmed accuracy, either in short-term projections (e.g., GNP one year hence) or in the long-term (10 to 15 years ahead). The latter case is based principally on the outcome in 1970 for a series of estimates made by Predicasts in 1960; these projections and outcomes are compared in Table 4, which has been published widely in Predicasts' advertising literature.

Several observations can be made about the results shown in this table. First, though most of the individual percentages for deviation are wrong, the average discrepancy is in fact quite low (3.4 percent as opposed to the 3.3 percent indicated; the median remains the same).[11] Nevertheless, the accuracy is not outstanding, as witness, for example, the forecast of GNP by the National Planning Association over a similar ten-year period (presented earlier in Table 1). NPA's error in its projection was 1.3 percent; Predicasts' was 3.2 percent. Comparisons of this sort are misleading, however, because Predicasts has used a three-year average as the "actual" 1970 value. If the 1970 value itself is used, the error on GNP is 4.5 percent. Indeed, when only the 1970 value is employed, the discrepancies for the other indicators change as well, sometimes dramatically, with the general result that the *average* error for these forecasts rises to 9.2 percent and the median error to 4.5 percent—an indifferent performance in comparison to the projections by other organizations of the same kinds of indicators.[12]

While Predicasts' assertions of phenomenal accuracy should thus not be taken too seriously, it should also be clearly recognized that these assertions rest on forecasts made a good many years ago. Since then the composite forecasting technique has no doubt been steadily refined, the Predicasts data bank has become much richer (it now includes time series data back to 1960 on some 15,000 indicators), and a good deal of practical experience has been gained in bringing one to bear on the other. For any of a number of purposes, the PREDICASTS service can be valuable today, and as the years pass, it will be possible to check and improve its accuracy as a device for monitoring the future.

SCOUT®

As far as is known, there are only two large-scale, formalized data banks of forecasted events in the world. The first is the PREDICASTS file; the second is the SCOUT file of The Futures Group. The major simularities between the two files are that each is based principally on forecasts found in the published literature; each has been designed for computer

Table 4
Claimed Accuracy of Predicasts' Long-Range Forecasts

ITEM	1960 Actual	1970 Forecasts: Made Oct. 1960	Actual*	% Deviation
General Indicators				
Population (millions)	179.7	208.7	205.4	1.6
No. of Households (millions)	52.4	62.9	62.9	0
GNP (bil 70 $)	662	1018	985	3.2
FRB Index Industrial Production	109	163	169	-3.7
Housing Starts (thousands)	1360	1800	1683	6.5
Product Production				
Paper & Paperboard (million tons)	35.5	52.0	53.4	-2.7
Raw Steel (million tons)	104	145	131	9.7
Primary Aluminum (thousand tons)	2050	4000	3906	2.3
Automobiles (thousand units)	6600	8000	7782	2.7
Electric Power (billion kwhrs)	755	1525	1529	-0.3
Average Discrepancy	3.3%	Median Discrepancy		2.7%

* Average 1969, 1970, 1971.

storage, analysis, and retrieval; each is used to prepare systematically organized listings of possible future developments; each is global in scope; and each limits its attention only to future *events* (trends are not included). In all other respects, the differences between the two are substantial. Characteristics of entries in SCOUT file and of reports based on these entries which are unique to the system are described in the following paragraphs.

First, no limitation is placed on subject matter. The SCOUT file includes forecasts on economic, business, and product-related developments, as does PREDICASTS, but it also encompasses prospective scientific and technological changes; developments in political, demographic, and personal affairs; institutional changes; and changes concerned with administrative practice, research capabilities, and so on. Some of these entries are general and cover topics which are difficult to quantify, such as future values or geopolitical developments; others are concerned with specific detail in precise domains, such as per capita energy consumption in the United States by 1980. As of December 1975, some 45,000 forecasts from about 8500 sources were in the SCOUT file, and additional forecasts were being added at the rate of 100-300 a week.

Second, each SCOUT report is custom made, typically to the specifications of a single client. To be specific: SCOUT reports are issued under one of three arrangements: (1) A report may be proprietary to the client; the subject of the search is not revealed publicly and the client has exclusive rights to the material, which means that The Futures Group will not undertake a search on the same subject for another client. Compilations of this sort cost $1950 each. (2) A report may be prepared on a proprietary basis, with the understanding that after three months it may be announced publicly and be made available to anyone else who requests it. Such a report costs $1450. (3) Finally, there are reports of the second sort that have become publicly available, as well as occasional reports prepared by The Futures Group on subjects that seem to be of general interest, such as future Japanese technology or the future of leisure. These reports cost $925 each. A typical report in any of these three series will include 300-500 forecasts from 100-300 sources.

Third, an overall judgment is made about the quality of each forecast before it is used in a report. For this purpose, several properties of the forecasts are considered. Is it expressed precisely enough so that it will be possible to determine after the fact whether or not the event actually occurred? How objective was the method used to make the forecast? (While the source does not often reveal the method, an educated guess is made by The Futures Group, using a scheme of about 20 method types.) How important is the forecasted event? That is, if it occurs, is it likely to make a significant difference to those who will be affected? Lastly, is the forecasted event, as stated, consistent with other images of the future? (For example, a forecast of widespread availability of antigravity machines for personal travel by 1981 would be judged low on this criterion.) These are not easy standards to apply, if only because they can require a special expertise or an insider's knowledge. Yet services like SCOUT would seem to demand a quality screen, and these criteria are no doubt a step in the right direction. Retrospective searches on the file—a possibility that has been discussed at The Futures Group—are one means that can be used to improve the criteria and their application in assigning a quality rating.[13]

Fourth, any relevant material provided by the source in addition to the forecast itself is also recorded and may be included in the SCOUT listing if requested. Five kinds of supplementary material are captured: *consequences* (developments that are suggested as being likely to transpire if the event occurs); *subforecasts* (associated forecasted developments that appear important but are too vaguely expressed to be included in the file as separate entries); *background information* (any qualitative information, such as a definition, provided to clarify the meaning of the event being forecasted); *historical data* (any quantitative information, such as a data point or a time series); *identification of the ultimate source* (bibliographic information acknowledging that the source has derived the forecast from still another author).

Fifth, a record is made not only of the date of possible occurrence of the event, but also of its probability by that date. SCOUT is unique among futures monitoring services in deriving probabilities for events found in the literature, and the procedure by which this is accomplished is worth attention, since probabilities are often useful and the sources themselves rarely provide them. No attempt is made, of course, to determine what was in the author's mind; this would be impractical at best, and usually impossible in fact. Rather, what is recorded is the probability which someone very familiar with forecasting or futures research would be likely to impute to the event when he reads it. Thus, the author's *received* meaning as opposed to his *intended* meaning is presented in SOCUT, the assumption being that the former is a close approximation of the latter—indeed, the closest approximation one can probably ever obtain.

The actual quantitative values used for probability assignments in SCOUT were established through an experiment conducted in the fall of 1971.[14] A list of words and phrases commonly used in stating future events was drawn from the literature. Suppose that X is an anticipated state of affairs; then examples of such locutions would include:

X is *inevitable*
X is *certain*
X *will happen*
X is *virtually certain*
X is *highly certain*
X is *likely*
X is *probable*
X is *as likely as not*
X *might happen*
X is *possible*
X is *not too likely*
X is *unlikely*
X is *improbable*
X is *almost impossible*
X *will not happen.*

For the initial experiment, 29 expressions of this sort were evaluated. Each was written on a separate 3 by 5 in. card, along with a sentence illustrating its use in context. The resulting deck of cards was scrambled and then presented individually to a number of persons, who were asked, first, to arrange the cards so that the expressions would be in order of ascending probability. (Terms considered to be synonymous were simply to be put next to each other.) After this was done, the person being interrogated was given a pencil and asked to go through the deck card by card and to indicate, using a scale running from .01 to .99, the actual probability he would assume the author had in mind in making a forecast in these terms. (Synonyms could thus be identified explicitly.)

Medians were found for all of the estimates obtained—not only for the group of participants as a whole, but also for a subset of persons (approximately 70 percent of the total) judged by the experimenter to be highly familiar with, or expert in, the making of forecasts and the handling of probabilities. The overall median results were as shown in Figure 1—a striking outcome in that it indicated that the sample of expressions covered the entire probability spectrum without any serious gaps. (The average difference between adjacent nonsynonymous terms is only 5.4 percentage points.)

More important, the consensus within the group as to the relative position of the terms in the spectrum was quite strong.[15] Still more important, however, it appeared that the expert subgroup was better able than the group as a whole to identify and estimate the probability of a set of nonsynonymous terms across the spectrum. This result is shown in Figures 2 and 3. Figure 2, which presents the results for the entire group, shows the distribution of median probability estimates for the 19 terms interpreted by the group as having unique values; note the unusual overestimation of the likelihood of terms with a probability greater than .50. Figure 3 on the right (for the expert subgroup) includes 17 terms, and clearly reveals no such tendency.

What can be made of this result? While the experiment was flawed in a number of important ways, it appeared at the time that two safe conclusions could be drawn: (1) it would be improper to assume that fine quantitative distinctions could be drawn between different words,[16] but (2) it was nevertheless possible in fact to derive an acceptable probability estimate from a wholly qualitative forecast. Thus encouraged, The Futures Group has since pursued this notion in the development of its SCOUT file.

As of this writing, the original list of 29 words and phrases has grown to about 60; each is taken to imply a particular probability estimate, and these probabilities are used in SCOUT reports. Most of the terms are considered, of course, to be synonymous. (Thus, for example, 11 of them are given a value of .75, and there are no terms with a value of .76, .77, .78, and so on.) Moreover, it is recognized that a good deal of additional experimentation is required to put this approach on a better footing.[17] For this reason, the probability of every forecast entered into the SCOUT file carries with it an additional code, keying the value to the specific term on which it is based. (For instance, a forecast might read, "Commercial availability of diagnostic kit for simple blood and urine tests in the home is as likely as not by 1985.' When entered into SCOUT, the associated probability code would be .5007, where .50 is the probability itself, and 07 refers uniquely to the phrase "as likely as not.") In this way, it will easily be possible to update the imputed probabilities if further research suggests that particular terms have been misinterpreted.[18]

Another distinctive feature of SCOUT is that each forecast is indexed using the Legislative Indexing

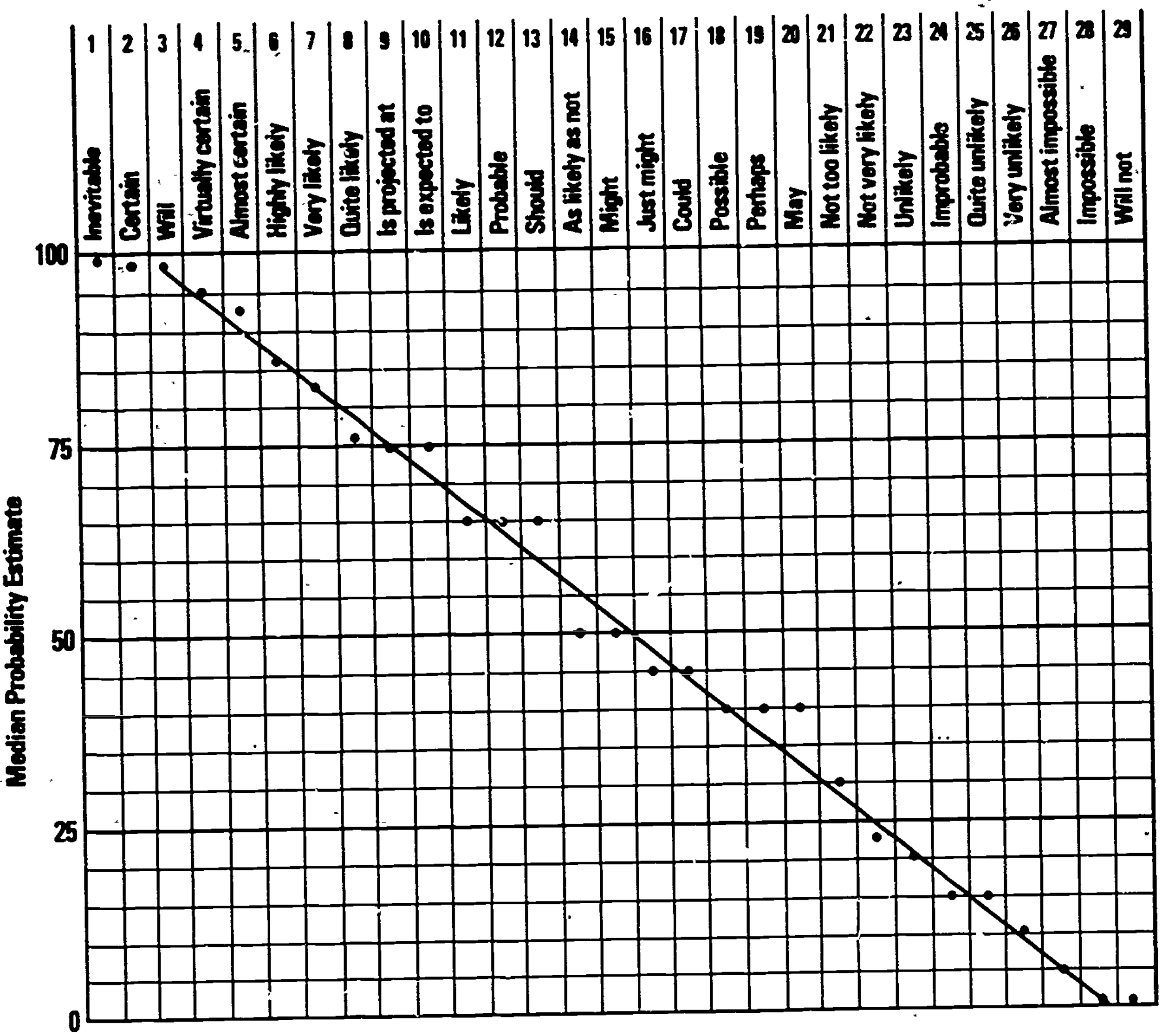


Figure 1. Distribution of overall median responses

Vocabulary (LIV) developed by the Congressional Research Service of the Library of Congress. Forecasts are then filed by each term used. Thus, many different types of searches can be performed using these terms singly or in combination. Indeed, the variety of searches is remarkably large, since a user can frame his request by specifying not only a particular combination of indexing terms, but also a precise time limit, a certain quality level, a limit on probability, a combination of methods, and so on.

One last feature of the SCOUT service that should be mentioned is that only rarely is a report based exclusively on the forecasts already in the file. In the usual case, the file is scanned to extract forecasts pertinent to the search request. These results are compared against an outline developed earlier in consultation with the client, and, where weaknesses are apparent, additional reading is done by the staff of The Futures Group to round out the final listing. To put it differently, SCOUT reports typically involve original. directed research; indeed, preparation customarily requires three to four weeks, only a few days of which are spent in tapping the data bank.

To illustrate how some of the features of SCOUT come together in an actual SCOUT listing, we present an extract from such a report in Table 5.[19]

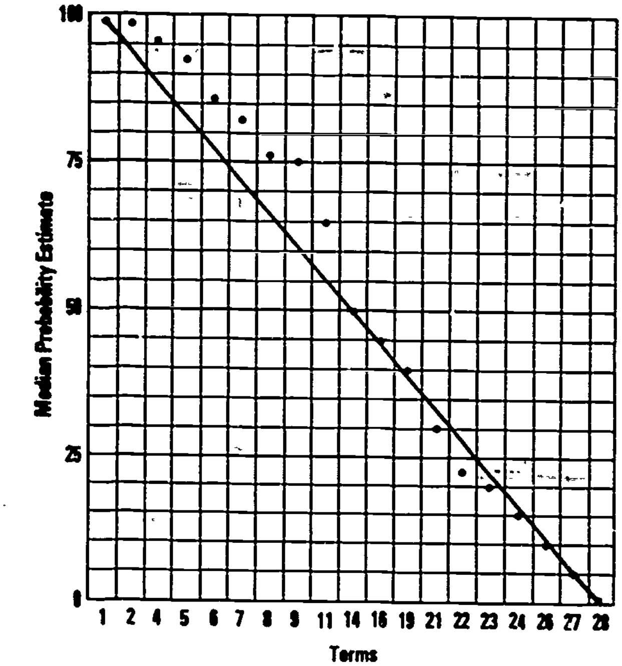


Figure 2. Distribution with synonyms removed (overall medians)

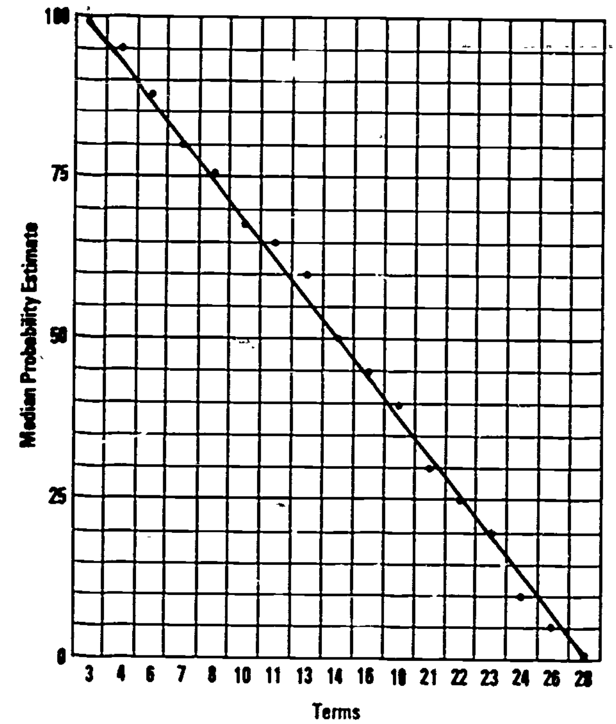


Figure 3. Distribution with synonyms removed (expert medians)

PROSPECTS®

PROSPECTS is a data and forecasting service that was first announced by The Futures Group in the fall of 1973. Methodologically, its uniqueness and interest lie principally in its being based on the use of trend impact analysis (TIA), an approach to trend extrapolation in which a computer-generated projection of a time series is systematically perturbed by taking account of judgmental estimates of the possible occurrence and influence of unprecedented events relevant to the time series.[20] Substantively, its uniqueness lies in its attempt to explore the future of entire industry sectors, going well beyond the usual economic analyses by examining social, legislative, managerial, technological, and other forces or developments that have a probability of shaping the future of these industries. In considering so broad and complex an interplay of factors, in making extensive use of the opinions of groups of experts, and in providing means for regularly eliciting feedback from users of the service, PROSPECTS resembles Project AWARE of the Institute for the Future. But PROSPECTS is less diffuse (in that it is limited to specific sectors); its structure is more formal (in that it is organized around a predetermined set of indicators for each of the sectors); and its approach is more objective (in that all major assumptions underlying the extrapolations are stated and thus can be examined for reasonableness in the present and accuracy in the future).

PROSPECTS is available only through subscription. The first business sector analyzed in this service was the pharmaceutical industry. For this specific PROSPECTS, The Futures Group teamed in September 1975 with IMS America, Ltd., a well known market research firm, to produce a jointly-researched service. Subscribers receive about 140 forecasts of time series indicators each year (approximately 25 indicator analyses are sent at 60-day intervals). A single data unit package is devoted to each indicator forecast. Data units are self-contained multipage presentations of six different types of information: a graph of the indicator itself, a projection of a range of values for the indicator out to the end of the period of interest,[21] a table giving historical data on other time series pertinent to the indicator, a brief description of forces and events that have influenced the indicator in the past or may be influencing it currently, a list of all of the forecasted events used in the TIA extrapolation (along with the actual estimates regarding the future likelihood and

Table 5
A Sample Page From a Scout Report

Category: Social Developments: The Social Setting: Public Services (Health Care)

Scout File Number	Source Code	Forecasted Event	Prob/year	Meth	Qual
000309	00016	Automated medical diagnosis and individual monitoring.	.50 / 1975	01	02
016751	00018	The implementation of a comprehensive federal health insurance program.	.50 / 1976	01	03
010151	00388	Widespread use of monitoring devices attached directly to the patient in his home.	.50 / 1978	06	02
010150	00388	Insurance covers 75% of all health costs. Historical Data: - 1971 = 25%	.50 / 1978	06	04
004144	00121	Computer-assisted interpretation of quantifiable medical tests, e.g., EKG, EEG, etc., for 20% of the physicians.	[illegible]		
004121	00121	Implementation of Health Information systems in 20% of U.S. metropolitan areas.	.99 / 1980	06	
004462	00215	90% of the United States population are enrolled in health maintenance organizations. Consequence: These organizations would encourage efficiency and place emphasis on keeping people healthy rather than on treating illnesses after they occur.	.75 / 1980	16	03
0[illegible]379	02145	Computers are used for medical diagnosis in most hospitals	.50 / 1980		

Index number in the SCOUT system

Code number identifying source of the forecast (a complete bibliography is presented at the end of the report)

Examples of supplementary information

Forecasted event

Estimated probability of occurrence of the event

Year by which probability of occurrence of the event is anticipated to reach the value indicated

Method number (keyed to a code of about 20 method types)

Quality evaluation by The Futures Group (scale: 0-5, where 5 is highest) impact of the events),[22] and a bibliography showing the source(s) for each of the events.

The 140 indicator projections cover six subjects important to the future of this industry sector: demography, personal and national economics, sales of drug products, the pharmaceutical industry, health statistics, and retailing and distribution. In addition, clients receive individual forecasts for sales of each of their major products. Thus the service is tailored to meet the demands for environmental forecasts, as well as the need for specific company planning information.

The cost of a one-year subscription to PROSPECTS is based on each company's sales volume and ranges from $7500 to $15,000. The service also includes consultation with The Futures Group and IMS America about the projections and their implications.

PROSPECTS is too new a service to be evaluated. Major problems regarding the TIA approach have been discussed earlier in this volume,[23] and work is underway at The Futures Group to overcome them. The crucial test, however, will be how and with what consequences PROSPECTS is actually used by its subscribers.

The same test applies, of course, to the other futures monitoring services considered in these pages. Indeed, the most important research questions raised by these services concern their value to users. What uses are being made of these services? How often? By whom? to what effect? What can be done to increase the utility of these services? What guidance can be given to help users compare and evaluate alternative services? What can the services themselves do to help ensure that they are being used properly, in full view of their strengths and limitations? These are not easy questions, but they merit investigation. Such a study would be valuable even if it contributed no more than a detailed insight into how information is captured by these services—an issue that has been addressed only lightly in this chapter and is never discussed in the directories and guidebooks on "the future" that appear from time to time. But if the study were performed by disinterested analysts who were granted reasonable access to the organizations involved, the results could be much more significant, in that they would allow users to tell whether a service occupies its niche because it is good or is good merely because it occupies its niche. Our impression is that this matter is still undecided in nearly every case.

FOOTNOTES

[1] This general definition, it will be noted, implies that an environment is context-dependent: environments can and do vary with decisions and policies, as well as with policymakers. The definition also implies that the conditions comprising an environment can include those that are entirely out of man's control and those that are man-made but unintended. Other writers have taken a much narrower point of view; for example, Theodore J. Rubin posits a single environment and characterizes it "as a set of outcomes, situations, or social conditions which is the policy consequence of the interaction of technology and social institutions." (See his "Toward Information Systems for Environmental Forecasting," in James R. Bright and Milton F. Schoeman (eds.), *Technological Forecasting: An Academic Inquiry* [Canoga Park, Calif.: XYZYX Information Corp., 1970], p. 362.) This restricted definition may make monitoring simpler, but it virtually ensures that important considerations will be missed or ignored.

[2] One of the few available descriptions of a business system is H.E. Sorrows, "Industrial Technical Intelligence," *Research Management*, Vol. 10, No. 4 (1967), pp. 217-227, which describes the monitoring system he helped to establish at Texas Instruments. The TI approach—an elaborate but apparently productive one—involved four phases: (1) data collection (through the literature, personal contacts, professional meetings, and searches of the Patent Office files); (2) classification, abstracting, and filing of this material (rapid retrieval was a goal); (3) evaluation (screening of the information against company interests, judging its quality and reliability, forecasting its implications, and synthesizing accumulated information to produce new intelligence); and (4) dissemination (through several series of regular documents, special analyses, and consultation).

[3] See Jay S. Mendell and Alfred W. Mueller, "Social and Technological Intelligence," *Technology Assessment*, Vol. 2, No. 1 (1973), pp. 47-59. See also James R. Bright, "Evaluating the Signals of Technological Change," *Harvard Business Review*, Vol. 48 (January 1970), pp. 62-70.

[4] This discussion updates to January 1976 (and substantially extends) a similar review in Wayne I. Boucher, *Description of a System for Identifying and Evaluating Important Social Trends*, Report 67-37-01 (Glastonbury, Conn., The Futures Group, January 1973), pp. 4-23. It should be emphasized, of course, that change is as much a feature of these systems as it is of society in general. The best source of current information is the sources themselves; these paragraphs are intended only to point toward representative sources.

[5] From a talk by Blake T. Newton, Jr. at a TAP Conference, November 19-21, 1972.

[6] Wilfred Lewis, Jr., "Long Term Economic and Demographic Forecasts for States and Regions" (Washington, D.C., National Planning Association Reprint).

[7] See "A Think Tank That Helps Companies Plan," *Business Week*, No. 2294 (August 25, 1973), pp. 70-71, from which some of the details in these paragraphs were drawn. Extensive use has also been made of material in Roy Amara and Andrew J. Lipinski, "Project AWARE, Societal Trends and the Corporation," *Tomorrow Begins Today* (New York, The Chemical Marketing Research Association, November 1973), pp. 42-49.

[8] Compare the *Business Week* article cited in fn. 7 with Richard Boeth, "Updating the Crystal Ball," *Newsweek* (November 27, 1973), pp. 101-102.

[9] Actually, there are two WORLDCASTS reports: WORLD-REGIONAL-CASTS, which arranges forecasts by country, and WORLD-PRODUCT-CASTS, which arranges them by industry. The same forecasts are included in each series.

[10] For example, much the same approach was developed independently by Harold S. Becker and Theodore J. Gordon at The Futures Group for use in a 1971 study of Japanese steel production and consumption. Becker called the technique the "balanced extrapolation procedure."

[11] It may be noted in passing that many of the 1960 "actual figures" are also wrong; for example, steel production was 99.3 million tons rather than 104 million, aluminum production was 2.034 million tons rather than 2.050 million, housing starts were 1.296 million rather than 1.36 million, and so on. These errors need not have affected the forecasts, of course, but they do raise questions about the quality of the Predicasts data base and the reliability of the procedures used in its manipulation.

[12] These percentages were derived using only nine of the ten indicators. The FRB Index of Industrial Production was omitted because the Predicasts values for 1960 and 1970 could not be found in checking such standard sources as the *Statistical Abstracts*.

[13] Needless to say, many other kinds of retrospective tests can and should be performed on the SCOUT and PREDICASTS files. A series of publicly-reported experiments (conducted, say, every two years for a period of ten years) would give profound insight into the actual state-of-the-art in forecasting—and into the possibilities, if any, of improving it.

[14] This work was done by W.I. Boucher, building on an idea first advanced by Andrew J. Lipinski of the Insitute for the Future, who was interested in determining if wholly qualitative scales used in Delphi studies could be satisfactorily converted to quantitative ones by the intermediary (i.e., the individuals conducting the study).

[15] To gain some insight into the possibility that this agreement was only a peculiarity of the group, a check was made in an unabridged dictionary to determine whether or not relevant meanings of any of the 29 terms were defined by using other terms on the list. If so, and if the terms considered synonymous in the dictionary were widely separated in the group's final list, then the results might be questioned. As it turned out, there were 17 cases of circularity, and all but two of them confirmed the group's opinion. Thus, "certain" was defined by "inevitable," "likely" by "possible," "impossible" by "unlikely," and so on.

[16] This is indicated by the fact that the experts recognized more synonyms than the group as a whole and by the fact that the spread in percentage points between adjacent terms in the experts' list was 6.1, as opposed to 5.4 for the group's.

[17] In the original experiment, the terms were evaluated without reference to a time horizon. Yet it seems plausible that the probability attributed to, say, "unlikely" might change if we were speaking of 2025 versus 1985. Similarly, no distinction was made in the earlier work between the probability of an event *in* a particular year and its probability *by* that year. Presumably, however, the distinction changes the meaning. It might be mentioned in passing that a similar and also very promising line of research still awaits its first investigation. Here, the task would be to ascertain, if possible, the extent to which words and phrases used to describe time horizons can be translated into actual periods of time, such as numbers of years. (Examples include "in the near future," "soon," "in this generation," "in a matter of years," "at some point," "before long."

and so on.) The problem is much more complex in this case, since the analysis of such phrases is extremely dependent on a knowledge of the personality and position of the speaker, the subject being forecasted, and the general environment, but it is easy to demonstrate that even a rough solution could produce highly valuable data for planning and policymaking.

[18] This capability will have to await the day when the SCOUT file is fully computerized. At present only about a third of the recorded forecasts have been stored on disc, and all searches of the file are done manually. In contrast, the PREDICASTS data bank has apparently been totally automated. It is now possible to purchase the PREDICASTS service on tape, and the company is working to make all of its files available to customers via terminal.

[19] In mid-1975, The Futures Group introduced a new service based on the SCOUT system, *The SCOUT INDEX*. *The SCOUT INDEX* is a monthly publication presenting all the forecasts extracted during the preceding month and arranged by the LIV subject headings. An entry includes all of the data of a SCOUT report, excluding quality and method. *The SCOUT INDEX* provides current information about what events are being forecasted and where to locate the information. An annual subscription is $375.

[20] Trend impact analysis is described briefly in Chap. 4. Incidentally, since TIA draws on the SCOUT file, PROSPECTS also rests to some extent on SCOUT.

[21] A projection of this sort from Pharmaceutical *PROSPECTS* appears in Chap. 4.

[22] Also illustrated in Chap. 4.

[23] See Chap. 2.

Part IV
On the Future of Futures Research

Chapter 16

Forecasting and/or Futures Research

John McHale

The given brief for this chapter was to look at the range of areas in which forecasts are made, to assess what kinds of gaps and weaknesses might be identified, and to ask how these might be influenced by factors within the field of futures research and by other factors external to the field.

This seems, at first glance, to be a relatively straightforward approach to an important topic. Closer consideration, however, raises several latent questions which may have significance for the larger study of which this paper forms a part. For example:

1. There is a tendency to overemphasize forecasting in itself, either by regarding it as one of the main objectives of futures research[1] or, in some cases, by assuming that the terms "forecasting" and "futures research" are almost interchangeable.
2. Forecasting is also tied very closely to its predictive character. This may be explicitly qualified by those who make various kinds of forecasts, whether they be economic, technological, or social. But the implicit criteria for evaluating forecasts tend to overemphasise prediction by the use of such terms as the "success," "failure," or "level of confidence" which may be accorded particular forecasts.
3. There are a number of unanswered questions regarding the directness of relationship between forecasting, futures research, planning and policy-making. Again, most of these questions would be directed towards the unexamined assumptions which link these activities together in some direct and linear fashion when, in fact, we do not know very much about the relationship at all.

I shall not deal with all of these questions, but the first one is somewhat central to my enquiry. In my opinion, forecasting is not identical with futures research, nor need it be the primary objective of futures research. There are some important differences between the two areas of activity.

Forecasting tends to assume that there is a given set of definable causal relationships between events which may enable one to predict their future state—within varying degrees of probability. It does not usually extend to questions regarding the structural premises or implicit assumptions about the world which might underlie the basis for forecasting and in which the nature of causality itself may be a prime focus for enquiry.[2] This latter set of questions I take to be within the larger province of futures research.

In more specific terms, one might also refer to the relative importance of "predictive" forecasting in futures research. Obviously, for some areas of work—e.g., short-term economic or technological forecasting—it is critically important even in the external predictive sense. In other areas, such as environmental planning, it may be less important as prediction than as "projected alternatives." And in some areas, such as social and cultural projection, it may not only be hazardous but counterproductive.

The point here is that in short-term forecasting for specific purposes the methodologies for projecting the costs and benefits of alternative directions may be relatively well advanced and provide valuable inputs to decisionmaking and policy formation.

The hazard is that when the same methods are applied to longer range social forecasting, they tend to place the emphasis on similar deterministic means of forecasting, i.e., on what *will be* under some set of given conditions rather on what *could be*—given the appropriate range of human choice and intervention. The use of the term "forecasting" here, even in the normative sense, may be counterproductive to the human ends of the enquiry.

My own bias in this regard is that the central function of futures studies is not to predict what *a* future or *the* future will be but to explore various alternative futures states which are resultant upon our individual and collective actions and may be accessible to our choice.

Another way to gauge the place of forecasting is to look at two examples of the extreme wings of futures work which might be represented by de Jouvenel's *The Art of Conjecture* and Kahn and Wiener's *The Year 2000*. The former contains little that might be labeled as forecasting in the predictive sense; the latter, though ostensibly predictive, is subtitled "A Framework for

Speculation." Somewhere in the middle ground, in work on technological and economic futures, as noted above, forecasting is used as a specific mode of predicting the outcomes of various alternative projections of quantifiable parameters.

The relative "functional" importance of forecasting in futures research would lie with:

1. *The particular class or method of forecasting* that we are talking about—a considerable variety running from directly extrapolative, to Delphi and cross-impact techniques, to simulation/gaming, intuitive projection, etc.
2. *The operative sector,* technological, economic, social, or cultural, as influencing the choice and viability of forecasting method.
3. *The range of futures projection*—short, medium, long—as also influencing the choice and validity of method.
4. *The reflexive effects of forecasts,* in themselves, as a variant of the observer/observed relationship; where forecasts function as self-fulfilling prophecies or as image constructs which influence attitudes, decisions, and actions in many different ways.

So much for my initial caveats on the role of forecasting in the broader context of futures research.

Areas of Emphasis in Forecasting

In reviewing the range and subject areas of forecasting, it may be useful to emphasize the recent and rapid growth of the field itself. One indicator of this is the increase in futures conferences and meetings. The actual range is too numerous to list here, but we may note the growth in size of the "official" world congresses. There were between 30 and 40 participants at the first Oslo meeting in 1967; Kyoto in 1970 had over 200, and the third congress in Bucharest drew even more. The Rome Special meeting still had over 100 people on a partially restricted basis. Even more significantly, the World Future Society 1971 Washington Congress drew over 2,000 participants, and the 1975 General Assembly attracted even more attendees.

This kind of growth, in itself, should not lead us to assume that we are now dealing with a large, well-organized, well-supported field of disciplined enquiry. In some senses, it is the reverse.

In 1969-1970 and again in 1971-1972, we conducted surveys of futures research in the United States[1] and some pilot surveys on the world scale. These were concerned essentially with who was doing what, for whom, and to what purposes. One interesting observation which emerged in relation to field growth over that period was that the actual number of persons engaged *full time* in formally defined programs of futures research had not expanded significantly *in comparison* with the vast increase in the numbers of people now "involved with the future" in many more different ways.

Our conclusion was that the field had moved from being a relatively small disciplinary enclave toward being part of a larger social movement. This may have an important bearing on the changing emphasis of forecasting areas.

For convenience, a summary of the first survey is given in Table 1. For example, in the general objectives and orientation of work the first survey ranked economic, technological, and social forecasting in that order with technological and economic as appreciably higher than social. In the second survey, approximately 2 years later in sampling time, the forecasting had shifted as in Table 2.

Though both individual and organizational profiles ranked social "forecasting" high, it should be noted that when this and other categories were regrouped together the balance was slightly different (Table 3).

When questioned as to more *specific foci* of work (see Table 4) the divergence between individual and organizational forms becomes somewhat clearer. There would appear to be some evidence to suggest that though the general objectives may have shifted toward more explicitly social and cultural concerns, the forecasting methodologies remain somewhat biased toward technological, economic, and other more objectively determinable measures (Table 4).

Returning to our earlier caveat regarding forecasting and futures research, it would seem that the major shift has been from predictive forecasting toward the more normative mode of exploring alternative futures in both general and specific senses.

It would seem obvious, however, that we will get a blending of both predictive and normative modes and that these may increase in sophistication as they are applied in areas of technology assessment, social accounting, and other policy-oriented work. The current emphasis on "alternatives" as such may simply be a "buzzword" phenomenon such as "systems," "participatory," etc.

TABLE 1.
SUMMARY OF FIRST PHASE SURVEY 1969-1970

1. *Objectives of work.* Two-thirds reported major economic and technological focus to their work. Others ranked social, political and environmental foci in that order, with concern for the individual occurring very low in the scale.

2. *Range of forecasting.* Major concentration on short range (next decade), medium concentration on next thirty years, about 1% on long range, ie beyond next thirty years. ('Forecasting' is used here to indicate general range of concern or interest rather than as prediction *per se.*)

3. *Funding support.* Was inversely related to length of forecasting – most, for very short term; least, for long term. Allocation by sectors was approximately as follows:

Government (in house and contractual)	50%
Corporate (mainly internal)	30%
Academic	10%
Foundations and other agencies	10%

Major support allocated to client defined work in short range with specifically focused objectives.

4. *Disciplinary profile of workers.* Dominated by engineering, economics, physics, chemistry and political science.

5. *Age profile.* Showed concentration between 36 and 45 years, suggesting professionals whose major work had been accomplished in their defined fields before moving into the futures research area.

6. *Sex.* Almost 90% male, with remainder including non-professional "support" staff.

7. *Generalized conclusions*

   a. Apart from few established institutions, most of significant work in the field done by individuals–not necessarily tied to their institutional setting.
   b. Major emphasis and funding allocated to short range economic and technological forecasting.
   c. Lack of adequately supported work in terms of "whole system(s)" and multi-variate approaches to national and world problems.
   d. Little systematic research on values and other areas of socio-cultural change.
   e. Weak linkage of futures research to actual policy planning process and to action.

In terms of the overall shift in emphasis some of the main characteristics affecting this may be listed as follows:

1. *The general change in social climate* in the 2 year interval due (a) to the rise of the environmental movement with its emphasis on the social and environmental consequences of technology and (b) to the ethical and value questions arising out of the youth, minorities, and war protest movements, etc.

   This change also has a direct effect on the futures field itself. For example, as the successive social movements and "activist" areas have peaked or burned out, many of their participants move on to the next movement or cause. This reflects itself in the conference topics, general literature, etc.—and becomes critical when futures becomes a social movement in itself.

2. *The accompanying shift in funding and support patterns.* This appeared to play a direct role in

TABLE 2.
ORIENTATION OF WORK (GENERAL)*

Forecasting		Research		Planning	
		Individuals			
Social	17.6%	Alternative futures	14.4%	Social	15.0%
Technological	13.3%	Social impacts/technology	12.0%	Technological	12.0%
Economic	10.2%	Value systems	10.9%	Educational	11.3%
Cultural	9.8%	Futures methodology	10.2%	Corporate	8.8%
Educational	8.2%	Social priorities	9.7%	Urban	8.5%
Resources	7.2%	Policy research	8.8%	Scientific	7.5%
Market	7.0%	Individual in future	7.5%	Political	6.8%
Scientific	7.0%	Environmental	6.4%	Resources	6.8%
Ecological	6.3%	Resource utilization	4.5%	Regional	6.3%
Manpower	3.9%	Family in future	4.2%	Ecological	5.5%
Military	3.3%	Manpower	3.6%	Architecture/Design	4.6%
Population	3.1%	Population	2.9%	Military	2.8%
Other	3.1%	Consumer affairs	2.1%	Labor	1.3%
		Other	2.8%	Other	2.8%
		Organizations			
Social	12.9%	Alternative futures	11.9%	Educational	12.0%
Economic	11.8%	Social impacts/technology	10.9%	Social	11.8%
Technological	11.8%	Social priorities	10.6%	Corporate	11.1%
Cultural	9.9%	Policy research	10.6%	Technological	10.6%
Educational	8.9%	Value systems	9.8%	Urban	9.3%
Market	7.8%	Environmental	9.3%	Regional	8.6%
Scientific	7.0%	Futures methodology	8.5%	Resources	7.6%
Resources	7.0%	Resources utilization	7.1%	Scientific	7.2%
Ecological	5.9%	Individual in future	5.3%	Ecological	5.8%
Manpower	5.7%	Population	4.4%	Political	5.6%
Population	5.7%	Manpower	4.1%	Architecture/Design	3.2%
Military	3,2%	Family in future	3.9%	Military	2.3%
Other	2.8%	Consumer affairs	2.8%	Labor	2.3%
		Other	0.8%	Other	2.6%

*John McHale, "Typological Survey of Futures Research in the U.S." (II), sponsored by the Center for Studies of Metropolitan Problems, National Institute of Mental Health, November 1972.

TABLE 3.

	Percent		Percent
Social	17·4	Technological	13·3
Cultural	9·8	Economic	10·2
Educational	8·2	Market	7·0
Ecological	6·3	Resources	7·2
Population	3·1	Military	3·3
		Scientific	7·0
	44·8		48·0

TABLE 4. SPECIFIC FOCUS OF WORK

Categories	Numbers in category	Categories	Numbers in category
Individuals		*Organizations*	
Alternative futures	19	Science and technology forecasting	8
Methodology	21	Resources	13
Planning	37	Fodo supply systems	1
Social change (general)	21	Environmental quality	2
Institutional change	6	Transportation	2
Family	2	Public service systems	1
Leisure	2	Policy research	21
Education	38	Business	43
Religion	3	Economic forecasting	5
Health	6	Communications/media	13
Law	2	National security	1
Values/ethics	7	Military	7
Politics	4	Documentation	3
Individual	1	Science fiction	2

determining the organizational orientation to forecasting, e.g., in the first survey, funding levels were in inverse ratio to the length and scale of forecasting. Most long-range, multivariate work was done by individuals independently of their organizational locus and support structure. Organizational work tended toward highly specific short to medium range forecasting.

3. *Fashionability* may also play a considerable role not only in terms of social climate directly but with the growth in visibility of futures in the mass media and the appearance of futures books on their best sellers lists.

Academic and educational interest in futures grows as a "new field" defines itself, gains in respectability, and affords new outlets for courses, publications, and careers.

4. *The "Future of X" syndrome,* in which a great deal of relatively conventional work in various other fields is "futurized" by adding the prefix or by assembling material under the rubric of "X in the year 2000" or "... in the twenty-first century."

Though associated with fashionability, this is possibly a much more direct response to a larger social need—the search for reassurance in a period of diffuse social anxieties regarding change. Though we are more concerned with its relationship to the shift in forecasting emphasis, as a phenomenon this would require a much wider appraisal in a social context which includes renewed popular interest in astrology, parapsychology, magic, etc.

5. *Change of field personnel.* The change in fields represented in futures research was quite marked between our first and second surveys (see Table 5). The influx of social and behavioral scientists, humanists, educators, etc. may be due to some of the factors listed above and also to the more recent growth of interest in the future by specific field workers themselves. For example, close to one third of our survey respondents had entered the field less than 3 years ago.

Gaps and Weaknesses in Forecasting

Taking these various shifts in emphasis into account, we might raise several questions regarding the areas in

TABLE 5. ACADEMIC FIELD OF RESPONDENT

Second survey (period June 1971-June 1972)		*First survey (period 1969-1970) (in number of responses)*			
Social/Behev. sciences	28.0%	Engineering	36	Finance	4
Physical sciences	13.4%	Economics	24	History	4
Engineering	12.7%	Physics	16	Law	4
Humanities	8.5%	Political science	12	Statistics	4
Education	6.8%	Chemistry	11	Computer science	3
Mathematics	6.2%	Psychology	11	English	3
Design	4.2%	Mathematics	9	Geography	3
Arts	3.3%	Business administration	8	Medicine	3
Life sciences	3.3%	Education	8	Humanities	2
Law	2.6%	Philosophy	7	Journalism	2
Journalism	2.0%	Business	6	Landscape architecture	2
Cinema/TV	1.3%	Social science	6	Management science	2
Others	7.7%	Architecture	5	Pharmacy	2
		International relations	5	Psychiatry	2
		Urban planning	5	Regional planning	2
		Anthropology	4	Theology	2

which forecasts are made or the range of topics with which futures research is more broadly concerned. In a cursory review—of the subject areas developed in our surveys, of the conference literature, and of the extraordinary growth of futures books, newsletters, and journals of various kinds—it would seem that the whole gamut of human concern is covered. The list of topics is certainly very long! Accompanying this, we have also the ancillary growth of bibliographies, documentation centers, clearinghouses, and networks.

The problem here is one of critical evaluation. It is not enough to list what is being done. When we look behind the often grandiose titles to the substance it is, in all too many cases, insubstantial, repetitive, and rhetorical. One may assume that the more technologically oriented wing of futures research may look after itself in this regard. My own concern is with the social and cultural area.

Though we might be gratified by the renewed emphasis on social and cultural themes, their treatment is still characterized by the conventional constraints and attitudes to change brought over from the various parent disciplines.

This is expressed most strongly in the "problems" syndrome of much of the work—in its preoccupations with the increase of complexity in "the technological society," in the trends toward the uniformity and anonymity of "mass society," in the breakdown in norms, values, and traditions, etc. These unexamined premises are projected in sets of problem scenarios—or countered by the advocacy of equally dubious and uncritical acceptance of needs for human potential, actualization, the renewal of face-to-face community, the preservation of the family, etc.

This is not to say that many of these problems are not real! But there is:

1. Little attempt other than in such problem-oriented terms to spell out the *specific* social and cultural changes which might take place according to individual and collective choice—or what the *content* of various kinds of individual social and cultural experience may be in different future ranges and conditions.
2. While eschewing the more technical approach of the economic, technological, or other forecasters, there is little attempt to integrate the substantive work of such forecasts in a manner which might inform and enrich the quality of social and cultural projection.
3. The major emphasis is almost entirely focused on macrosocial change—changes in society, organizations, and institutions down to the small community level. The least emphasis and least attention is on the individual, on the authenticity of individual experience as contrasted with the abstract image of the individual—other than as the overconstrained performer of sets of social roles determined by society. New styles of life as reflecting changes in institutional forms tend to be oriented to their connotations of "deviance" contingent upon the breakdown of past and present norms.

   Simon, in his explication of the "fallacy of sociomorphism," underlines this particular weakness, which, in his words, "imputes to the individual organism that which is properly a function of social life, (and) which allows us to accept uncritically as constants, as socio-cultural universals, or 'natural laws' for social life that which may, in fact, represent little more than the outcome of the sociohistorical moment."[4]
4. The treatment of new cultural forms is often so naive as to be considered a marginal component of futures work. Much of it suffers less from a failure of imagination about the future than a lack of substantive knowledge about the present and the past. The closest approximation in attitude to today's cultural futurist is probably the late nineteenth century Pre-Raphaelite Brotherhood—rather than even the Futurism of a Marinetti or Bocceoni in the early 1900's!

Some of the more generalized problems in social and cultural forecasting may be summarized as follows.

Theoretical

There is insufficient rigorous attention to the reexamination of traditional models and stereotypes. Much of our theory of social, economic, cultural, and even political change, where it exists, is still overhung with earlier social science reactions to, and misunderstanding of, the disruption of traditional forms and the social disorder attendant upon rapid change—and an admission of the specific social and other costs which change entails.

Lurking behind the labels of much discussion are various models which lay claim to all sorts of universality and historical continuity—linked to sets of latent assumptions regarding human nature, needs for collective security, social health, pathology, etc.

There is still evident in much futures work a rather diffuse value indictment of modern society which persists even, or perhaps more so, in those studies which purport to be concerned with values in the future. While it may be necessary to criticize the present on the way to the future, many of our value critiques tend to offer no more than nostalgic forays into the past. That the more valued elements in human social and cultural experience are associated with past periods of (apparent) stability and permanence may obviously be comforting, even if largely illusionary.

One specific example of theoretical weakness, related to the orientation of past, present, and future, is the assumption of *time* itself as an invariant and universal constant. It is certainly empirically evident that the ranges of experiential time and life space are indeed highly variable and pluralistic amongst different societies, even for social groups and individuals. As Gilb suggests,

> For any particular society, also to be explored are the variations, conflicts and similarities of the time and change imageries of men, women, different age and vocational groups, social and economic classes, and ethnic and other special groups. How have these differences helped to structure the institutions of the family, church, work, play, politics, economics, government and the law, and—in addition—what time and change images have these institutions had qua institutions?[5]

The overdependence on an idealized past and the failure to reconceptualize ongoing changes in the present leads also, for example, to various fixed assumptions about the roles and centrality of our core institutions—i.e., the economy, the polity, the military, etc—and to their being held relatively constant both in function and centrality for the future. There is, in general, an undue reliance on "structural certainties" which are assumed to persist as basic norms in regulating and modifying various changes in society.

Social and cultural expectations and objectives are similarly held constant, or only marginally variable, so that the established interests and preoccupations of the present are projected, largely unexamined, into future forms. Again, it is empirically evident that expectations, tastes, desires, and objectives, both of societies and individuals, are highly variable over time.

> Even if long term predictions were demonstrably accurate, which they are not, it is now coming to be seen that each generation has its own tastes, and that the tastes of the next generation are neither certain to be ours nor is it believed that they should be ours.[6]

This kind of projection of present demands into future needs may be relatively harmless in individual speculation. When linked to social and economic planning, and the commitment of present resources and energies to long-range purposes, it requires more vigilant questioning. It is not enough that we free the present from the shackles of the past if, in turn, we sacrifice too much of the present to the supposed exigencies of the future.

In summation here, we may underline that in almost all areas of forecasting and futures research, we need to be more rigorous regarding the implicit and explicit assumptions upon which we are operating. Our assumptions pre-select the range of questions which we address and the types of data we use and predetermine to a considerable extent the kinds of futures we project.

Methodological

Even with the influx of social and behavioral scientists into the field, the range of methodological skills is still relatively inadequate—both in monitoring the indicators of change with which we are dealing in the present and in the assessment of their import for the future. This also selects and shapes the kinds, and contents, of the forecasts and projections we make.

There is still a strong tendency to rely upon "canonical" data which is administratively convenient for, and manipulable within, conventional methodologies. We are still not very good at transferring our quantitative data into qualitative contexts and forms—in ways that may be meaningful for the projection of qualitative changes in social and cultural forms.

For example, most of our methods and monitoring skills are lacking in the qualitative assessment of symbolic communication in various media—in the vast profusion of visual images and social metaphors through which social and cultural changes may be perceived, and in whose terms they require projection.

There is a latent hierarchical ranking of methods from "soft" to "hard" in terms of rigor, numeracy, and quantifiability. This does not recognize that, in fact, it may require as much sophisticated expertise to monitor and manipulate the soft indicators of socio-cultural change as to do technological, economic, and such forecasts. Unfortunately, those most expert in the latter areas of "hard" analysis may be correspondingly inept and unskilled when confronted with data which is not couched in their traditional forms. Novels, movies, television, art, comics, ads, etc. are no less difficult to interpret and use than factorial matrices and computer models. The range of data which is admitted to our wider concern in futures research is, therefore, somewhat inhibited--and this in turn leads to a considerable poverty of "content" in many of our social and cultural projections.

Organizational

We have already noted the rapid growth of the field and some of the problems associated with this phenomena. Other aspects of this do bear upon the range of topics forecasted and the general content of futures work.

The field is somewhat polarized between the "professionals" and the "activists." There is, on the one hand, the drive for professional respectability and rigor via more formal organization etc.—and, on the other, a more vocal concern with widening the base of nonprofessional participation in futures work and linking futures studies more directly and actively with ongoing societal problems.

The former direction tends to skew the topic emphasis toward more highly defined and restricted forecasting within strong substantive frameworks of disciplinary expertise. The latter widens the range of concerns to the extent that they mirror the current issues and needs of the widest participation—this makes for a richer set of topics but is certainly somewhat "thin" in terms of substance.

Joining both polar aspects is the pressure for more documentation, bibliographical services, academic programs, and courses. At times, however, it seems as if we have more documentation going on than there are useful documents to classify!

What seems to be rather necessary to reconcile both wings of the field, and to advance the state of the art generally, are the following:

1.The establishment of some *futures critical* function wherein the substantive assessment and critical judgment of work being done could be conducted in an ongoing fashion. To a certain extent this critical evaluative apparatus has been most visible in science though it also operates powerfully in the arts, i.e., in the function of the individual critic and the general field criticism affo[illegible] by colleagues, public attention, evaluation, etc.

2.Some *clearer definition and discussion of the educational and public information* aspects of futures research. Both of these areas also bear upon the field emphasis question. The efflorescence of futures courses and academic programs has led to a proliferation of texts and papers designed for use in such courses and to a situation in which "educational futures" has become a variegated topic area in its own right.

3.*The "Future of X" syndrome,* referred to earlier, would suggest that we may be at a developmental point in the field when it might be useful to convene sets of representatives from the different disciplines to discuss the future of their disciplines—as a possible focus for their own research and teaching and as a field contribution to the study of the future. This is already going on, to a certain extent, in sociology, anthropology, and several other areas.

4.*Futures as a self-defined field.* We tend to forget that futures research, futurology, futuristics, etc. is peculiarly self-defined as a field in itself. Recent expansion has tended to reinforce that self-definition to an extent which may be hazardous for its own future development.

a.It would be quite misleading if we were to gauge the substantive balance and character of the field solely on the basis of the number of entries or studies listed in our various surveys and directories. Closer acquaintance with the content of such listings and more rigorous evaluation is also most necessary.

b.There is also an acute "ahistorical" tendency within the field itself which leads to the neglect (or lack of knowledge) of earlier work in the area which was not labelled appropriately as "futures," e.g., apart from the literary utopianists and other theoreticians, one could indicate a large body of work produced by various legislative committees, independent commissions, and individual workers which projects and explores future needs and requirements in many areas.

c.In the range of present activities, it is also evident that much more work than we generally allow is already being done quite outside of the futures field as currently defined. Some of the more obvious examples here would be in the field of social indicators, certain areas of national and international planning, critical work on communications, the arts, cultural development, historical reassessment, etc.

The funding and support structure has been an important influence in the organization of futures research—and will obviously continue to be.

One interesting point which emerged in our first survey was that the bulk of the pioneering work in large-scale, long-range, and multivariate futures projection was done by individuals and groups who were not in institutionalized programs of futures research. They were not *primarily* supported for this work but "bootlegged" the time from other activities. In many senses this still holds true. Whether it may hold in the future is questionable.

As the field gains in fashionability and wider interest the individual's position becomes more difficult. In order to retain access to necessary materials and tools and to keep pace with field growth etc., such workers have had to become more institutionally bound. (Institutions fund institutions,not individuals.[7]) Meanwhile, the larger institutions which failed to support such long-range, large-scale exploration when it was "unfashionable" now move into the area with greater resources and personnel—but most often still lacking the essential insights and vision of the individual. The latter gets progressively squeezed out just as the work which he, or she, pioneered gains wider validation and support!

What we face here, in a sense, is a kind of *institutional imperialism* in which large intellectual conglomerates comes in at a specific point in field evolution to develop the large-scale, well-funded projects, whose substantive merit and accomplishment usually falls somewhat short of the rhetoric with which they are proposed and reported. This is rather similar, in some senses, to the "Little Science, Big Science" problem.

The institutional funding record in futures research is actually rather dismal but continues to skew the field emphasis considerably. The funding agencies have been particularly characterized by timidity and lack of foresight. While evincing due concern for "original," "innovative," and "exploratory" work, they then set up elaborate constraining conditions for its support and conduct. This somewhat is like asking Lewis and Clark to draw up a set of state highway maps before they start out!

This brings in the "second stringer" problem. Those who conduct the initial explorations in an area are seldom able to lay out very clearly the detailed rationales, objectives, and plans which funding agencies require. When the unsupported work is published, however, it is all too easy for the less innovative individual or institution to pick out the more immediately "fundable" ideas for elaborate proposal writing. This may be quite defensible in itself because the overall work is thereby advanced. But the problem remains that the more creative and original workers are pushed successively into more marginal positions in relation to support, and the substantive field contributions are correspondingly diluted. This process is certainly not confined to futures research, but the peculiar nature of the field intensifies it considerably.

Integration of Forecasts

As in other fields, future research tends to suffer from an imbalance between analysis and differentiation—and synthesis and integration. One reason may be that the disciplinary training of most persons has favored the former rather than the latter. In a field, and a period, however, when the "cross impact" and "whole system" effects of change are critically important, it would seem necessary to redress the balance.

It would appear to me, therefore, that a key item on the research agenda for futures research would be the substantive assessment, interrelated synthesis and integration of forecasts, and projections as a more central activity. Of necessity this kind of exercise would require more "hindsight" work on the validity of past forecasting as well as ongoing inputs from the "futures critical" function described earlier.

This returns us in somewhat circular fashion to the beginning of this chapter. It is probably only through the development of such integration on a fairly large scale and continuous basis that we could have some kind of ongoing review of the areas in which substantive forecasts are being made. To attempt such a comparative review in terms of individual forecasts, in themselves, would be much less rewarding than being able to gauge their interrelated effectiveness within a more holistic context.

Through such a series of linked activities we also should be able to monitor over time the changing emphases, weaknesses, and strengths of the overall futures research field in itself.

FOOTNOTES

[1] One might even qualify the use of the term futures "research" in this regard. It conveys notions of methodological rigor, objectivity, and exactitude which may not only be misleading but constraining upon a field in which speculation and conjecture play a much larger role than they do in any other area of so-called research. The less overblown term "futures study" might be more appropriate.

[2] For an extended discussion of the latter point, see "Theories of Industrial Society: Reflections on the Recrudescence of Historicism and the Future of Futurology," John H. Goldthorpe, paper for 7th World Congress of Sociology, September 1970.

[3] John McHale, "Typological Survey of Futures Research in the U.S." (I and II), sponsored by the Center for Studies of Metropolitan Problems, National Institute of Mental Health, June 1970 (I) and November 1972 (II).

[4] William Simon, "Reflections on the Relationship between Man and Society," Rome Conference, September 25-30, 1973.

[5] Corinne Lathrop Gibb, "Time and Change in Twentieth Century Thought," *Journal of World History*, Vol. IX, No. 4 (1966), p. 876.

[6] Edward Shils, "The Intellectuals and the Future," *Bulletin of Atomic Scientists* (October 1967), p. 11.

[7] This phenomenon is also evident in other areas, e.g., in the arts—museums, conferences, histories, and "documentation" of art are much more lavishly funded than the individual artist. The same is true in theater, literature, and other arts.

Chapter 17

An Agenda for Futures Research

Olaf Helmer

Futures research, as an organized activity, is only a decade old. It was in the early sixties that the Futuribles group, under Bertrand de Jouvenel, began to carry out future-directed social and political studies; that Daniel Bell formed the Commission on the Year 2000; that Dennis Gabor wrote *Inventing the Future*; and that the first major technological forecasting study was carried out at RAND by Ted Gordon and the author.

True, there were earlier, precursory developments, such as the work of the Prospective group in France led by Gaston Berger and the invention of the Delphi technique in the early fifties by the author in conjunction with Norman Dalkey. Another, more philosophically oriented contribution might be seen in the essay "On the Epistemology of the Inexact Sciences," on which the author collaborated with Nicholas Rescher in the late fifties. But these were just signs in the wind, and it was not until the mid-sixties that the efforts particularly of the Futuribles and the RAND-sponsored futures groups began to be emulated throughout the world, resulting in what has now grown into a full-fledged futures movement, with institutes, journals, professional societies, and such governmentally supported offspring as the Educational Policy Rsearch Centers and the Office of Technology Assessment.

This proliferation of futures studies, while gratifying in some respects, has had some decided drawbacks. The reason for the explosive adoption around the world of Delphi-type explorations of the future, of simulation techniques for long-range planning, and of other future-oriented methods is not so much, I think, the quality of the original, prototype studies but rather a recognition of the urgent need for the social sciences to begin to catch up with the physical sciences in their ability to cope with the problems of the real world—a need made more urgent by the growing complexity and the accelerated pace of change in our operating environment. The widely felt requirement for better long-range planning in the areas of social and political institutions, of the physical environment, and of international relations has caused too many people to place too great hopes in the benefits to be derived from the new discipline of futures research. Its failure to live up to these expectations has begun to produce a backlash, in that some new doubts have been raised as to whether futures research is altogether an activity worth pursuing.

Many people forget that a decade is a very short time period in the development of any intellectual movement. Not only has there been insufficient time to place the discipline of futures research on a solid conceptual foundation, but the pursuit of this desideratum—though well recognized as such by some—has been encumbered simply because demands for pragmatic results have had priority over a solidification of the foundation and because the rapidly increasing number of practitioners has of necessity included some charlatans and incompetents along with serious researchers. The time has come, therefore, to reflect on what futures research is all about, what genuine promise it offers, what can be done to improve its intellectual basis, and what priorities should be assigned to its applications.

Futures research is a branch of operations research. And like that of traditional operations research, its function is to provide decisionmakers with operationally meaningful assistance in the form of information and analysis that can facilitate better decisionmaking. Operations research originated in World War II, where it proved to be of imense help in the conduct of military operations. Most of its later applications to problems of the civilian sector of government and to problems of commerce and industry, while different in subject matter, were similar in purpose. They concerned management decisions affecting either the daily operations of an agency or firm or its relatively short-range planning activity—short range in the sense that the operating environment at the time the plans were to be implemented could be expected to be essentially the same as those at the time the planning decisions were being made.

The main characteristic that differentiates futures research from standard operations research is that its objective is to improve decisionmaking in the case of long-range plans. Here "long range," in contrast to "short range," clearly means that the operational conditions at the time of implementation are expected to differ substantially from those prevailing at the time of planning.

We note in passing that more and more of the important decisions are moving from the short-range into the long-range category. The reason for this is twofold: first, many of our decisions concern increasingly complex situations, where the time horizon is farther in the future, as illustrated by our attempts to solve the energy crisis or to plan a new city or to develop an underdeveloped country; second, as we all know, we live in an age of ubiquitous future shock, in which changes in our living environment occur with ever increasing rapidity, due mostly to technological advances and their societal implications. Together these two trends, by mutual reinforcement, have given futures research the premature prominence which we observe today. Once we recognize that futures research is indeed a branch of operations research, and specifically that branch concerned with plans which are to be implemented in a changed operating environment, a number of points become evident.

Because of the pragmatic nature of futures research, its function is primarily predictive rather than explanatory. By forecasting the future environment and the consequences of alternative plans for coping with that environment, it attempts to improve the decisionmaking process. Often its findings may merely be based on observed correlations between phenomena or on the intuitive, pretheoretical judgment of experts, falling far short of a causal explanation fo. he expected consequences of proposed actions. While a fuller understanding of the underlying causes would surely always be welcomed, the success of futures research has to be measured in terms of the quality of the decisions it makes possible rather than of its explanatory force.

Like operations research generally, therefore, futures research is prescientific in nature. The urgency of the task—be it the conduct of military operations (the context which gave rise to operations research) or the management of the energy crisis or the search for cures for our sick society—is so great that there is no time to wait for the construction of neat scientific theories which would, by logical derivation, permit the identification of the optimal plan of action to be followed. Instead, in the tradition of operations research, the futures researcher constructs ad hoc models as best he can, well knowing that they may be imperfect and in need of later correction and improvement as more data are obtained and more experience is accumulated.

No claim is made that such imperfect models produce perfect foresight. That the resulting decisions are often nonoptimal should be no reason to reject the futures research effort. Its aim is to produce the best practically attainable decisions, even if not necessarily the theoretically best decisions. Considering the often enormous payoff attached to decisions regarding the long-range future, even a small improvement in the quality of such decisions may yield very appreciable returns.

Next, it seems to me that the following observation ought to be made: A rigorous scientific approach involves three interactive elements: theory construction, empirical data collection, and controlled experimentation. I have already pointed out that the model-building endeavor of operations research, and hence of futures research, falls short of the rigor demanded of scientific theory construction. Not only are its models comparatively "soft," so are, at times, its data, as well as its experimental procedures. There are, in fact, no hard data about the future; and the futures analyst has to rely on soft, judgmental data instead. That is, he must use probabilistic pronouncements by experts about the future operating environment as a substitute for firm, observational data. And real experimentation often is replaced by pseudo-experimentation, using a simulation model in which players, acting the parts of real-world decision-makers, test the implications of alternative decisions in the model in the hope that the simulation has sufficient verisimilitude to permit a transfer of the model results to the real world.

Reliance on the intuitive judgment of experts thus is not just a temporary expedient but a necessary ingredient of futures research, because they are needed in all phases of the effort. They are called upon (1) to supply judgmental data about the future, based on their intuitive, though often theoretically unstructured, insights into real-world phenomena; (2) to construct ad hoc models or to judge the suitability of existing models; (3) to apply their expertise as role players in simulation games; and, additionally, they are often required (4) to use their imagination and inventiveness to design the instrumentalities and long-range strategies that result in appropriate action programs for dealing with the problems of the future.

Despite its relative softness compared to a hard, scientific approach and, in particular, its inevitable reliance on expert judgment, futures research aspires to be objective. This looks, at first glance, like a contradiction in terms, since objectivity of a discipline can almost be defined as "the intersubjectivity of its findings independent of any one person's intuitive judgment."[1] To preserve the claim to objectivity, it is necessary for the futures researcher to dissociate himself logically from the experts, in the sense of regarding himself as an experimenter who uses the experts as measuring instruments of reality, whose pronouncements about the future world are taken in the same spirit as, say, the readings on a measuring device are taken as an indication of some property of the present world. The experts, thus, are viewed as black boxes which receive questions as inputs and produce answers as outputs. Different experimenters, using different but comparable sets of such "black boxes," should be expected to arrive at comparable results. This places a special obligation on the discipline of futures research to devise suitable measures for the degree of reliability of these instruments and to seek ways and means of improving their quality (i.e., of helping the experts to increase the reliability of their performance).

Another point to be made is that, since futures research is concerned with the planning of human activities, it is, of necessity, multidisciplinary. Even if the planning objective itself is unidisciplinary (which is seldom the case), attention must be given to the technological, the socioeconomic, the cultural, and the physical environmental circumstances, in consideration of which plans have to be made, as well as to the psychology of the decisionmakers. Usually, the more important a planning decision is, the more complex is the area to be surveyed and the more intricately are the considerations interwoven that different disciplines have to contribute. In planning a transportation system or a reform of the educational or the health-care delivery systems or a new city, there is hardly a single discipline that will not be required to supply specialized inputs to the planning process in the form of assessments by experts of probable future developments. Futures analysis, therefore, has to develop effective and efficient means of facilitating cross-disciplinary cooperation toward common planning objectives.

Finally, to fulfill its purpose of improving the decisionmaking process regarding long-range plans, futures research has to address itself explicitly to the criteria problem. There are several aspects to this: first, there is the question of identifying the planning criteria, that is—technically speaking—the payoff function or functions or the utility vector to be maximized. This involves several extremely difficult subproblems; specifically, that of exchange ratios in the case of multidimensional utilities, that of relative weights in the case of a multiplicity of beneficiaries who need to be satisfied, and that of changing value systems, which may cause future criteria of optimization to be different from present such criteria. Second, given a set of criteria, there is a question of determining the comparative degree to which alternative action programs can be expected to satisfy these criteria. All of these tasks are analytical in nature, require cost-effectiveness and systems analysis, and must be clearly distinguished from the normative problems of what ought to be done about the future. (There is a large faction among futures researchers who wish to emphasize this normative aspect of their work. In contradistinction to futures analysis, this segment of futures research, which is devoted to what is sometimes called "normative forecasting," might perhaps best be described as "futures synthesis." To date, this portion of futures research, which covers the spectrum from abstract utopian writing to belligerent demagogic activism, is poorly organized and badly in need of conceptual systematization.)

These, then, are some of the features of futures research, as seen by one of its practitioners. It is a methodological approach for which there is a great, pragmatic need in our society, which is full of promise and calls for an immense amount of basic research effort in order to bring it to full fruition. This constellation of urgency, of great potentialities, and of wide-open opportunities for original research ought to represent an attractive lure for some of our best intellects to get involved in and for our public and private funding sources to support.

Let me now make an attempt to outline an agenda for some of the specific research tasks that lie ahead, as suggested by the analysis just presented of the purpose and present status of futures research. I shall organize this agenda under the following headings:

- Data Collection
- Model Construction
- Experimentation
- Systems Analysis
- Substantive Applications: Exploratory
- Substantive Applications: Normative

In each of these categories I shall list some of the areas which seem to me to be particularly in need of research.

Data Collection

All our knowledge about the future is ultimately derived by some form of extrapolation, however subtle, from the past. The kind of empirical data on which such extrapolations might be based are no different from the data that provide us with whatever insight into our present world we have, and their collection is the task of individual scientific disciplines, such as psychology or demography or meteorology. Leaving aside whatever standard difficulties there may be in amassing such basic empirical information, the analysis of the future, as I pointed out before, requires as inputs another kind of quasi-data in the form of judgmental estimates about future operating conditions. While these judgments are formed by experts through some implicit process of extrapolation from the past, this process is rarely based on a well-articulated theoretical procedure but more often is intuitive and pretheoretical in nature. Epistemologically, their function is comparable to that of measuring instruments.

This whole area of judgmental data collection is in considerable need of further exploration, suggesting the following specific research items:

1. *Rating and improving the performance of individual experts as forecasters:* How can the performance of an expert be calibrated? How do self- and peer-ratings compare with one another and with other, more "objective," rating systems? How can an expert's performance be enhanced? What data, data processing facilities, models, simulations, or communication devices would be most helpful to him?
2. *The utilization of groups of experts:* If two or more experts supply probability distributions over time for the occurrence of some potential future event, what is the appropriate mode of combining their individual estimates into a joint probability distribution? What use can here be made of any available performance calibrations or information about systematic bias of the experts? How can the judgments of experts belonging to different fields of specialization best be combined into an interdisciplinary estimate? How do the anonymity and feedback features of Delphi compare with other modes of using experts, such as polling, face-to-face discussion, or other conventional conference procedures?
3. *Improvements in the Delphi technique:* What degree of anonymity is most helpful to the performance of a Delphi panel? What influence does the wording of questions have? How should the entire questioning process be structured? If the subject of inquiry is multidisciplinary and if differential calibrations of the experts with regard to each discipline are available, how can the best use be made of this information? How does a hierarchical panel structure compare with a homogeneous one? How stable is a panel's judgment over time? What is the optimal panel size? Is a panel of two or three top-notch experts preferable to a panel of a dozen reasonably good experts?

All of these questions call for extensive experimentation. There is an obvious obstacle to carrying out such experiments, because good experts are too rare a commodity and their time is too precious for them to be readily available as experimental laboratory subjects. Some of the indicated experiments have indeed been carried out, notably by Norman Dalkey, but using graduate students as surrogate experts. There remains a gnawing doubt whether all of the results obtained in this manner carry over to the case of real experts, and at least some careful experimentation is mandatory to establish this crucial point. (Parenthetically, the kind of spectacular convergence of opinions obtained by Dalkey and myself in our first Delphi experiment in 1953 in connection with a military problem[2] has rarely been repeated in subsequent applications. My intuition tells me that the technique's success in this regard at the time was due to a fortunate combination of circumstances, namely, the discriminate use of specialists in several relevant disciplines and the availability of at least a crude model structure which facilitated interaction of the panelists' inputs. Many of the later applications and experimental explorations of Delphi have, by contrast, been much more simplistic, prohibiting the subtle kind of interplay which contributed to the success of that first Delphi study, and a return to a more sophisticated approach may well be worthwhile.)

Model Construction

Futures analytical models, as I indicated before, tend to be more tentative than rigorous scientific models are. Because of their soft, ad hoc character, their quality—like that of soft data—depends greatly on the good sense of their constructors. The question arises whether, as in the case of judgmental-data collection, a group effort might be superior to an individual effort in selecting or inventing a model. This suggests the following, fourth, research area:

4. *Application of Delphi to model construction:* How should a Delphi inquiry be structured if its aim is to produce a model for a specific forecasting or decisionmaking purpose? In particular, how can the facility of a remote-conferencing network best be used toward this end?

The construction of models of any kind is not an easy undertaking when they are intended to cope with a subject matter within the general domain of the social sciences, and hardly any futures analytical topics are devoid of social science aspects. In this context, it would be helpful to have a simple, general purpose model kit available that would permit the construction of at least a rudimentary model, no matter what the subject matter. One such kit, however inadequate in some respects, in fact exists in the form of the cross-impact approach. Its prescription is very simple: If your concern is with the future of Topic X, take a list of potential future developments whose occurrence or nonoccurrence would either make a decisive difference to the future of X or would be a significant indicator of X's future status. Make probability estimates for the occurrence of these developments. Then look at them in pairs, and estimate how much the occurrence of each would affect the probability of occurrence of the other. Finally, use a Monte Carlo process to decide for each development whether it does or does not occur, making prescribed adjustments in the probabilities of the others as you go along. The result, in effect, is a modeling process which produces scenarios of the future, incorporating all important aspects relevant to the phenomena that were intended to be explored.

There are numerous open problems in connection with this handy-dandy tool for the future-oriented social scientist, some of which I am about to list. Yet its promise seems far from negligible and a pursuit of improvements in this technique well worth while. Specifically, this suggests the following, fifth item of study:

5.*Basic cross-impact concept:* How can double-accounting in a cross-impact matrix be properly avoided; that is, if Event A has a direct impact on Event C but also has an indirect impact on it via another event, B, how can we make sure that this indirect impact is not also reflected in the direct impact of A on C, and thus counted twice? How can multidimensional aspects be handled, where two or more occurrences jointly affect the probability of another event?

Since the cross-impact approach attempts to deal with causal rather than merely correlative effects, the traditional case distinction for two attributes A and B into

$$A.B,\ A.\overline{B},\ \overline{A}.B,\ \overline{A}.\overline{B}$$

needs to be replaced, in the case of two events, A and B, by

$$A.B.,\ B.A.,\ A.\overline{B}.,\ \overline{A}.B.,\ \overline{A}.\overline{B},$$

where the first two instances differ in that either the occurrence of A precedes that of B, or vice versa. Hence the following item:

6.*Causal probabilities:* Is there a need for a special "causal probability" calculus, or is the traditional, correlational probability calculus adequate for causal cross-impact applications?

The original approach to cross-impact, introduced by Ted Gordon and myself in 1965, dealt with events only, that is, with occurrences at specific points in time. In considering potential future developments it is important, however, to give some attention also to trends, that is, to gradual fluctuations over time. In fact, most of the traditional econometric models, and similarly the system dynamics approach utilized in the limits-to-growth study, go in just the opposite direction by examining interrelations between trends only and neglecting point events altogether. In the cross-impact approach, it is conceptually easy (though troublesome in detail) to adjoin the consideration of trends to that of events. In fact, in the case of a trend, the equivalent of an "event," for cross-impact purposes, is a deviation of the trend value from its anticipated course. Some of the problems arising in this context are the following:

7.*Trend-enriched cross-impact concept:* Since the impact of a trend T (on either an event or another trend) depends on the amount ΔT by which it deviates from its anticipated value, should the effect be considered a linear function of ΔT, or what other functional form is approriate? Does an impact on a trend cause a persistent shift in its future course or a gradually declining shift or merely a momentary blip? How does one decide which of these, in a particular case, is the most appropriate form of the impact? Can the separate, independent impacts caused by two or more developments be considered additive?

8.*Production of cross-impact inputs:* What efficient procedures are there for eliciting large numbers of entries into a cross-impact matrix from a panel of experts? How can this process be expedited with the help of a network of computer terminals?

9.*Continuous cross impacts:* How can the present discrete cross-impact format, where an occurrence in one time period has an effect in the next or some later time period, be made continuous so that occurrences can take place at any time and their impacts be registered at any time thereafter?

Experimentation

In the absence of a time machine, true experimentation about the future is a logical impossibility. Data obtained from an experiment always are data about the past, and statements about the future can only be derived from them by extrapolation. Hence we have to resort to pseudo-experimentation, which means the construction of a simulation model about the future and experimentation within that model, in the hope that results thus obtained carry over to the real world of the future. There are some obvious questions in this connection:

10. *Validity of pseudo-experimentation:* How can the validity of results obtained by pseudo-experimentation be checked or measured? By what means can at least the relative validity be enhanced? How can the effectiveness of stimulation gaming as an aid to the forecasting performance of an expert be increased?

Let me turn now specifically to a particular form of simulation gaming, namely, cross-impact gaming. Just as the basic cross-impact approach is a (perhaps somewhat simplistic) general purpose type of modeling, cross-impact gaming similarly is a general purpose mode of simulation. A cross-impact model of the future of Topic X is turned into a simulation game by introducing one or more players into the model and giving them the option of intervening in the normal course of developments by interjecting an action program. The effects of such actions are handled in the model just as those of any other events are in that their impacts on the events and trends represented in the model are estimated and superimposed on the cross impacts already present in the model. Perhaps the most important application of this idea is to planning situations, where a single player, the planner, intervenes in the normal course of events by trying out alternative action programs. But there is no reason why the simulation model cannot also be used by several players, representing different interests, who can use the model to study the interactions of their separate interventions. Some of the questions to be answered in this context are summarized in the following items:

11. *Cross-impact gaming:* How can the realism of cross-impact gaming be increased? How can it best be used to aid the forecasting and planning performance of the participating players? Can it be an effective stimulant to the invention of new strategies for meeting future contingencies? Can the simulation be used, by having the players observe the intuitive acceptability of the results of their actions, to correct and thereby improve the probability and cross-impact estimates on which the cross-impact model is based? What would be the legitimacy of such a self-correcting procedure? In the multiplayer case, can effective use be made of a network of computer terminals for processing the interactions of the various players' interventions?

Systems Analysis

The application of futures research to long-range planning involves the choice of policies and the design of action programs. These, in turn, if they are intended to be rational and practical rather than mere exercises of the imagination, have to take into account both the resource constraints on the actions and the expected payoff in terms of improved conditions at some future time. Thus, cost-effectiveness considerations enter into the picture and, with them, all the problems of systems analysis:

12. *Systems costs:* How can nonmonetary social costs be appropriately related to monetary costs so that overall systems costs can be meaningfully estimated? How can future costs be reliably estimated? What discount rate is appropriate, considering present interest rates, uncertainties regarding future costs and benefits, and our diminishing concern over the future as a function of the time-horizon? Hence, how should future costs be compared to present costs? How should expenditures in the private sector be compared with public sector expenditures? How should outright expenditures be compared with investments made in the expectation of future repayment? Since future costs depend on future contingencies, is it possible to design a cost cross-impact model, in which the impacts of events and trend changes on costs are systematically analyzed?

13. *Systems benefits:* Given a topic X, for the future of which plans are to be made, what is an appropriate set of social or other indicators in terms of which a future condition of X can be described and different future conditions can be compared? What are the interest groups whose differential desires regarding the future condition of X must be considered? What rational methods are there for assigning weights to different interest groups? What are the dimensions of satisfaction in terms of which gratification with the future condition of X can be measured? How can such multidimensional utilities be combined, and what are their exchange ratios? What is the appropriate discount rate for future benefits, again considering increasing uncertainties of, and decreasing concern over, the future as the time horizon recedes? In the absence of a single payoff function, how can we measure the overall degree to which a given action program can be expected to comply with a variety of goal criteria? Can the kind of cost cross-impact model mentioned under the preceding item (systems costs) be extended to include benefit cross impacts, reflecting diminished or enhanced benefits associated with substitutabilities and complementarities among proposed actions?

14. *Representative scenarios:* Considering the usually vast number of possible scenarios of the future, no specific one of which has a more than negligible chance of

occurring, is it possible to single out a small, manageable set of "representative" scenarios, in the sense that any scenario is sufficiently similar, for planning purposes, to one of the representative scenarios, so that a planner can confine himself to planning an action program against the contingencies depicted in the representative scenarios and still be confident that his program will stand up to every possible contingency? How can the scenario-generation mechanism of the cross-impact approach best be utilized in this context?

Substantive Applications: Exploratory

The research agenda outlined thus far has been concerned with methodology. Turning now to substantive applications, one might cite virtually all the woes of our society as suitable subject areas for futures research. However, I will confine myself to listing only some of those areas which seem to me both to be of outstanding importance and to have a specific requirement for the long-range aspects of futures research, in the sense that the operating environment at the time when the effects of any reform plans might be felt are expected to differ substantially from present operating conditions. Thus, for example, the subject of a reform of our criminal justice and our health care delivery systems, while of unquestioned importance, will not be listed, because the changes in technology and in our value systems, which may indeed affect these societal institutions, will do so in only a very minor way, considering the very major deficiencies which are already in evidence.

The listing is broken down into four subcategories:

15. *Public sector applications:* How should the educational system at all levels be reformed, utilizing new technologies now available or on the horizon, so that to those being educated the results will be meaningful during the rest of their lifetime—much of which may be in the first half of the twenty-first century? What should be done to regulate and to preclude mismanagement of our anticipated ability to intervene in the human genetic structure through molecular engineering? In what direction should we concentrate our efforts to open up new sources of energy? What economic controls should be applied to cushion our society against the effects of a severe crisis in the supply of energy and other resources and, conversely, against the effects of an eventual abundant supply of such resources? What previsions should be made through the planning of new cities for an increase in the U.S. population by another 50 million within the next generation? What measures should be taken to reduce all forms of pollution in the long run to acceptable or even comfortable levels? What transportation and communication networks should be planned for the future? How can governmental inefficiencies be reduced without endangering our democratic institutions?

16. *Private sector applications:* What major research and development efforts should be supported? What power plants should be built? In what direction should the automobile industry move? What forms of collaboration with the public sector should be sought to facilitate the building of new cities and of new transportation networks? What are the potentialities of the still-exploding communication and computer industries? What will be the potentialities of space exploration, if and when it is resumed on a full scale?

17. *Technology assessment:* What will be the direct and, particularly, the indirect effects on our society of anticipated developments in the communications area, such as the general availability of portable telephones and the ability to dial anyone in the world at negligible cost, the installation of automated libraries and of national data banks, the availability in most homes of two-way television terminals, and the introduction of a national computer utility? What will be the societal consequences of possible technology-induced crises, such as a severe energy or raw materials shortage, the necessity to ban automobiles from the central cities, or a radioactive or biological warfare mishap that kills more than a million people? What would be the indirect and unexpected implications of certain social engineering measures, such as the introduction of a negative income tax, of major penal reforms, or the abandonment of the Congressional seniority system? What would be the social consequences of certain major technological breakthroughs, such as genetic intervention, fusion power generators, artificial protein, or the commercial feasibility of transmuting chemical elements into one another?

18. *International applications:* What are the global prospects regarding energy and raw materials resources (the Club-of-Rome problem), and what can be done to avert a catastrophic crisis? What reasonable international agreements can be made to reduce the pollution of the oceans? Similarly, what reasonable international agreements can be made to extend national control to larger areas of the oceans than is presently the case? What would be the implications of adopting a single world currency? What can be done to effect an orderly development of the underdeveloped countries? What is the future of arms control and disarmament? What would be the implications of a large-scale joint effort at space exploration? What would be

the long-run effect of the introduction of an international social-welfare system? What are the exchange ratios, in terms of expected long-term benefits to the United States, of Federal expenditures in areas related to foreign affairs, such as defense, espionage, diplomacy, international development, cultural exchange, and joint ventures?

Substantive Applications: Normative

There are those in the futures movement who consider social reform as the overriding objective of futures research. While I thoroughly sympathize with their motivations, there is a deplorable and self-defeating righteousness about their position, which seems to imply a resentment against any attempts at an objective exploration of what the future has to offer.

Futures research, in my opinion, can well make a profound contribution to social reform, and indeed it may rightly be considered to have an obligation to do so. But as a research activity it has to remain as objective as possible, drawing a clear line between analytical exploration and political activism. A political activist, in fact, if he wishes to be constructive rather than merely destructive, will derive all the more guidance and support from futures research, the more objectively such research clarifies the distinction between real potentialities and merely wishful thinking.

Aside from activist reformers there are others whose concern for the future takes a different form. They are the utopians and anti-utopians, who describe fictitious future states of the world that they regard as desirable or detestable, and the inventors, who use their imagination to design specific social innovations intended to change our society for the better. None of these, either, can be considered futures researchers as such, but futures research is very relevant to their interests, as it is to those of the activist social reformers. Specifically, we can note the following topics which bear upon the normative aspects of exploring the future:

19. *Critique of present conditions:* What are the dimensions of the quality of life (the "satisfaction indices") in terms of which satisfactions and dissatisfactions with the state of the world can be measured? With regard to which of these dimensions are dissatisfactions particularly strong, and among which segments of the public? What are the great issues of our time, and what are the anticipated issues of the future? What proposals for their resolution have been made, how feasible are these, and what would be their side effects? What social inventions are needed to help resolve some of these issues?

20. *Criteria for social reform:* What planning criteria in the form of ethical standards, political platforms, social goals, societal movements, and utopian ideals have been proposed? How internally consistent and technologically feasible are they? What priorities and exchange ratios do they imply among the quality-of-life components? How would the attainment of such goals differentially affect different segments of the public?

21. *Scenarios and utopias:* Given a utopian description and interpreting it as a fictitious future state of the world that can be approximated but never fully attained, what realistic, or at least plausible, scenarios are there that could lead constructively from the present state of the world to an increasingly closer approximation of the utopian state? What are the relative satisfactions felt by different segments of the public during the transitional stages presented by such a scenario? What implementation strategies and social inventions are required to bring such a scenario about? In other words, what are its political and socio-technological prerequisites?

These, then, are some of the questions to which futures research might address itself. The list, I am sure, is incomplete, and others may place the emphasis differently. But I hope that the compendium I have presented is representative in the sense of illustrating the large variety of topics which need and deserve to be probed and with which researchers in this field might profitably concern themselves.

FOOTNOTES

[1] Olaf Helmer and Nicholas Rescher, "On the Epistemology of the Inexact Sciences," *Management Science*, Vol. 6 (1959).

[2] Norman Dalkey and Olaf Helmer, "An Experimental Application of the Delphi Method to the Use of Experts," *Management Science*, Vol. 9 (1963).

Chapter 18

A Statement of Research Needs

Wayne I. Boucher

The thesis running through most of the chapters in this book has been that, while futures research represents an approach that can usefully be adopted in the analysis of complex problems of choice, there are many difficulties yet to be faced—and yet to be overcome—before this approach can make its fullest contribution. The fundamental premise of the book has been that, while some of these difficulties can be eliminated immediately by greater honesty, for example, or by more judicious use of the established methods of forecasting, there nevertheless remains a core of difficulties that can be minimized or removed only through original research. The promise of the premise is not that such research, however careful or inspired or well-funded, will make all of these core issues vanish or somehow transform futures research into a science. None of the contributors to this volume would endorse so pollyannish an opinion. But all would agree, I think, that research of one kind or another provides the only rational means available by which to test certain crucial and presently ill-understood beliefs about the who, what, how, when, where, and why of forecasting.

What research is required? Each of the preceding chapters has identified, explicitly or implicitly, a number of candidates. Additionally, several dozen projects were defined during the discussions of the chapters that were presented as papers at the conferences held in January 1974. Still other projects were specified by the respondents in our worldwide survey of research in progress (see the appendix to this volume), and others emerged as a result of our review of the literature (both the items in the bibliography and several hundred other sources). When these candidates were brought together, the total number of unique suggestions and recommendations came to well over 300—a virtual cornucopia, sufficient to occupy every forecaster for the foreseeable future. In short, the possibilities were interminable, overwhelming, and utterly unmanageable.

Evaluation of Research Suggestions

To bring order to the list and discover which of the candidates had the highest merit, we took a step without precedent in futures research (and almost without precedent in other, more well-established fields): a number of persons who had suggested ideas were asked to critique their own proposals and those made by others. Specifically, each of the participants in the January conferences was invited to review formally that subset of research recommendations which appeared to be the most significant contenders for serious study and scarce resources. The method used for this review was a poll, administered by questionnaire. This chapter presents the results from this survey; in effect, these results define an agenda—and quite possibly the *basic* agenda—for future research on futures research.[1]

The questionnaire was divided into three parts. In the first, the respondents were presented with a form giving the five major headings from an outline used throughout the project to organize the discussion of research needs in futures research. They were asked to assume that these headings were exhaustive, in the sense that every research project of any serious interest in efforts to advance the field of futures research could be made to fall under one of them. They were then requested to provide an estimate of what they would consider to be a reasonable distribution of the total research effort among these categories. That is, given the needs as they perceived them today, where *should* the emphases fall? Their answers were to be given by allocating 100 percentage points among the categories.

The results for the group as a whole are shown in Figure 1, in which the dots indicate the extremes in individual estimates, the shaded area indicates the interquartile range of estimates, and the diamond indicates the average estimate. Opinions obviously differed widely, especially on the need to do more work on basic theoretical issues (Category I) and on methods (Category II). But the mean estimates, which, of course, sum to 100, seem to convey a reasonable message: the total program of research should be balanced, no category should be ignored entirely, and no category should be given undue emphasis (i.e., more than about 10-15 percent greater support than that given to any other category).

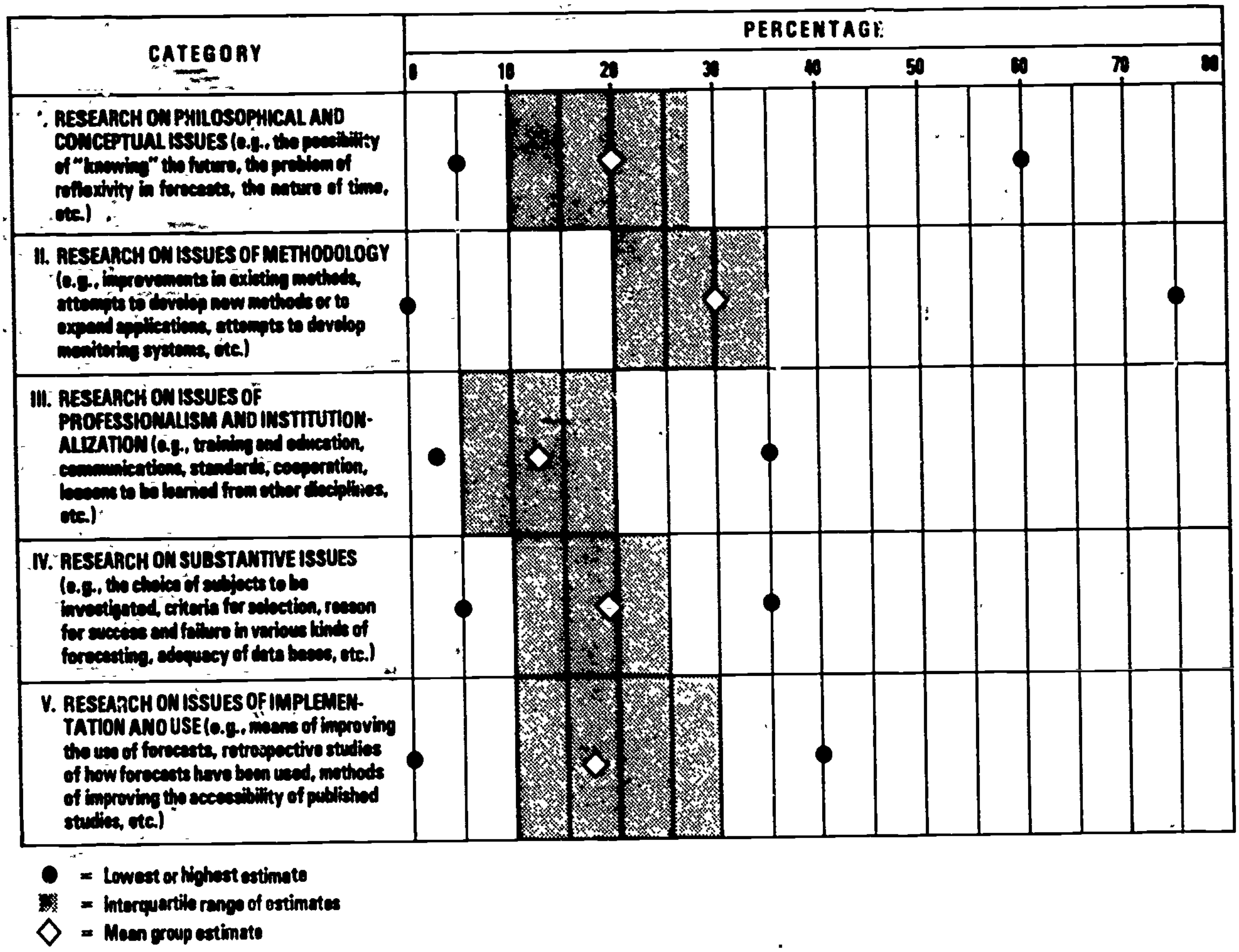


Figure 1. How should the total research effort be structured?

So plausible is this idea that it is necessary to add at once that a rather different message emerges if one simply distributes into the appropriate categories all 300–400 of the research suggestions that have been made and then calculates the percentage of items in each category. On this basis, the results change as follows:

I. Research on Philosophical and Conceptual Issues	20 percent
II. Research on Issues of Methodology	40 percent
III. Research on Issues of Professionalism and Institutionalization	20 percent
IV. Research on Substantive Issues	5 percent
V. Research on Issues of Implementation and Use	15 percent

These percentages are probably a far less reliable guide to the structuring of a research program than the group evaluation of the categories themselves because they involve no assumption about the relative importance of the recommendations made in each category. It is, for example, remarkably easy to invent an impressively long string of research candidates in methodology, all of which can actually be modest in scope or trivial in concept. A single suggestion in the area of, say, theory can readily swamp such a list in overall significance. Hence the value of stepping back and attempting to view the categories in context.

In the second part of the questionnaire, the respondents were presented with 86 research recommendations drawn from the sources mentioned earlier. Items were presented in fairly random order within the five categories, and space was provided at the end of each category in case the respondent wished to reintroduce a project of his that had been omitted in the preliminary screening or in case he wished to suggest an idea that had not been mentioned before. Opposite each item, given or new, were columns that would permit the subjective evaluation of the intrinsic merit of each project against each of four distinct criteria:

1. Urgency—that is, the extent to which improvements in the practice and usefulness of futures research depend on an early resolution of the issues involved. Obviously, some projects may be worth doing, but they can safely be deferred; these were to be judged to have a "low" urgency. Others must be addressed as soon as possible if futures research is to change significantly; these could be said to have a "high" urgency.
2. Payoff, if successful—that is, the extent to which the overall field of futures research will be advanced if the work is performed and the results are satisfactory. A "high" payoff project was defined as one that will have a profound or fundamental effect throughout futures research; a "low" payoff project as one that will produce, for example, a new understanding of an issue or perhaps a new capability, but the effect will not be pervasive in futures research. Obviously, "low" payoff need not mean that the project is unimportant.
3. Tractability—that is, the extent to which the project lends itself to research, given current awareness of the issues involved, current techniques, current data resources, and so on. The respondents were asked to note that tractability is not a function of complexity; even highly complex projects can have "high" tractability if, say, enough experience on similar problems has been gained.
4. Cost—that is, the level of professional effort required by the project, as measured in man-years worth approximately $50,000 each (including all direct and indirect costs). Here the respondents were asked to assume that a "low" cost project was one requiring less than one man-year; a "medium" cost project, one to two man-years; and a "high" cost project, more than two man-years.

A portion of a typical page is shown in Figure 2 to illustrate the format. A check was to be placed in the columns that came closest to representing the respondent's own opinion. (The notation "UNC" meant "uncertain.")

In the third part of the questionnaire, the respondents were asked to pause for a moment, to reflect on the evaluations they had just made, and then to select from the entire list of projects—given and newly added—the three to five items which they considered to be the most important, "all things considered." Having made these judgments, the respondents were then requested to select from the three to five items the one project which they considered to be "most interesting and important."

On the following pages we present the results for each of the original 86 projects (which are given in the same order as they appeared in the questionnaire). The numbers in the columns show the percentage distribution of the vote under each criterion. Following the "given" items in the five categories are projects shown in italics; these are the newly suggested research candidates. The evaluation given by the person who proposed them is shown by means of "X's" in the columns.

An analysis of these results follows the tabulation.

I. CONCEPTUAL AND PHILOSOPHICAL

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
1. Evaluate the adequacy of current theories of social change.																
2. Conduct rigorous philosophical examination of the relationship between value systems and technology.																
3. Analyze how values have impacted on socio-cultural changes in the past. (A project like HINDSIGHT or TRACES.)																
4. Analyz																

Figure 2. Sample of form used to evaluate research projects.

CATEGORY I – PHILOSOPHICAL & CONCEPTUAL ISSUES

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	NO	UNC	HI	MED	LO	UNC
1.01 Evaluate the adequacy of current theories of social change.	.57	.05	.33	.05	.62	.19	.14	.05	.05	.38	.48	.10	.29	.48	.14	.10
1.02 Conduct a rigorous philosophical examination of the relationship between value systems and technology.	.29	.33	.33	.05	.43	.24	.24	.10	.05	.65	.30	0	.33	.52	.10	.05
1.03 Analyze how values have impacted on sociocultural changes in the past. (A project like HINDSIGHT or TRACES.)	.10	.48	.38	.05	.29	.48	.19	.05	.11	.58	.26	.05	.24	.48	.24	.05
1.04 Analyze the role of values in shaping the future development of values.	.11	.37	.42	.11	.16	.42	.26	.16	0	.33	.50	.17	.16	.37	.21	.26
1.05 Analyze the impact of different ideologies on forecasting.	.14	.43	.33	.10	.24	.57	.14	.05	.15	.50	.35	0	.15	.50	.30	.05
1.06 Analyze the limits of policy control; attempt to clarify what portion of the future is within man's control and what portion is not.	.38	.33	.24	.05	.52	.33	.10	.05	0	.15	.75	.10	.62	.24	.05	.10
1.07 Devise new or better means for measuring the possible significance of future problems.	.38	.33	.24	.05	.57	.19	.14	.10	0	.25	.55	.20	.48	.24	.19	.10
1.08 Analyze the extent to which philosophical results in the study of ethics can contribute to the selection of social priorities.	.10	.20	.60	.10	0	.40	.40	.20	0	.16	.68	.16	.15	.25	.45	.15
1.09 Investigate the role played by societal images in social change processes.	.19	.33	.38	.10	.29	.38	.24	.10	.05	.48	.39	.10	.14	.43	.19	.24
1.10 Investigate whether various FR approaches can be integrated into a single paradigm.	.14	0	.71	.14	.29	.24	.38	.10	.05	.29	.62	.05	.33	.29	.19	.19
1.11 Analyze the possibility of developing a language and framework for characterizing social change processes (including determinative, formative, and random aspects, as well as values, goals, and indices of social performance).	.14	.38	.43	.05	.33	.43	.14	.10	.05	.43	.48	.05	.43	.33	.05	.19
1.12 Analyze the primary goals of FR.	.19	.19	.57	.05	.14	.33	.43	.10	.24	.43	.29	.05	.05	.38	.43	.14
1.13 Analyze how future events are discounted (as discussed in the chapter by Linstone).	.29	.43	.19	.10	.48	.33	.05	.14	.29	.52	.10	.10	.33	.33	.14	.19

CATEGORY I — PHILOSOPHICAL & CONCEPTUAL ISSUES (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
1.14 Analyze the question of how the validity of results obtained by pseudo-experimentation (construction of a simulation model about the future and experimentation within that model) can be checked or measured.	.38	.38	.19	.05	.71	.14	.05	.10	.14	.38	.43	.05	.52	.38	0	.10
1.15 Analyze the "internal" criteria of credibility (i.e., what properties of the forecast — whether probabilistic or deterministic — help by themselves to ensure credibility).	.14	.52	.24	.10	.24	.52	.14	.10	.14	.33	.38	.14	.19	.48	.19	.14
1.16 Analyze a representative sample of cases to clarify assumptions and conclusions regarding "self-fulfilling" and "self-defeating" forecasts.	.14	.52	.24	.10	.24	.57	.05	.14	.29	.57	.05	.10	.14	.43	.29	.14
1.17 Investigate common unconscious assumptions about the future using hypnotic "progression" to "place" panelists in the "future" and, e.g., have them read a newspaper 25 years from now.		X					X				X				X	
1.18 Use hypnosis, etc., to convince volunteers of some future inevitability (e.g., the death of capitalism). See what effect this has on their life decisions (e.g., by hypnotic interrogation).			X				X				X				X	
1.19 Reexamine the fundamentals of probability theory as they apply to forecasting unique events.	X				X					X					X	
1.20 Analyze policy issues and alternative futures, beginning with a thorough analysis of goals and values and their possible evolution.	X				X					X			X			
1.21 Establish uniform format for documenting and reporting.	X					X				X				X		
1.22 Establish guidelines for selecting the most suitable methodology for given classes of problems.		X			X					X					X	
1.23 Analyze the value paradox (policy effectiveness will be judged on the basis of future value determined in part by the policy and by other factors) and value dynamics in general.	X				X					X				X		
1.24 Explore means to effect time distortion (thereby extending individuals' planning horizon).	X				X						X			X		

CATEGORY II – ISSUES OF METHODOLOGY

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
2.01 Analyze the factors which brought about the acceptance and use of current methods of futures research.	0	.19	.76	.05	.05	.24	.67	.05	.25	.65	.05	.05	.14	.52	.29	.05
2.02 Identify the underlying similarities and differences, if any, among the principal data-gathering, forecasting, and synthesizing techniques (e.g., cross-impact analysis, trend extrapolation, scenario building, gaming/simulation).	.10	.48	.38	.05	.14	.57	.24	.05	.52	.38	.05	.05	.19	.48	.29	.05
2.03 Devise better ways of incorporating societal images important to social change processes into FR.	.33	.33	.29	.05	.38	.43	.14	.05	0	.43	.52	.05	.38	.57	0	.05
2.04 Design and conduct a critical experiment to test the validity of the "systems approach" in forecasting.	.14	.38	.38	.10	.33	.38	.19	.10	.05	.43	.43	.10	.43	.43	0	.14
2.05 Develop guidelines for the selection and use of social indicators, so as to avoid fuzzy conceptualization, faulty assumptions, etc.	.33	.43	.19	.05	.57	.29	.10	.05	.05	.52	.38	.05	.48	.29	.19	.05
2.06 Develop a means of using business and consumer expectation survey data more effectively in combination with forecasting techniques.	.05	.62	.29	.05	.29	.57	.10	.05	.29	.43	.19	.10	.33	.48	.10	.10
2.07 Devise or improve methods for forecasting the need for forecasts (i.e., for forecasting "the next crisis").	.62	.19	.14	.05	.76	.10	.10	.05	.10	.29	.52	.10	.38	.48	.05	.10
2.08 Develop criteria and methods of forecast evaluation.	.52	.29	.14	.05	.65	.25	.05	.05	.25	.40	.30	.05	.26	.53	.16	.05
2.09 Establish a system for periodically collecting and analyzing past forecasts by each of a number of professional forecasters, with a view toward discovering not who was wrong and why, but who was more often right and why.	.35	.25	.35	.05	.35	.50	.10	.05	.40	.35	.20	.05	.30	.30	.30	.10
2.10 Establish a system for periodically collecting and analyzing important occurrences that were not forecasted, with a view toward discovering what was missed and why.	.33	.48	.14	.05	.76	.14	.05	.05	.40	.40	.15	.05	.24	.71	0	.05
2.11 Analyze the extent to which people can give objective answers to questions about their values or future behavior.	.24	.29	.33	.14	.33	.38	.14	.14	.10	.24	.52	.14	.33	.33	.19	.14
2.12 Devise a means of identifying discontinuities in trendlines with sufficient accuracy and sufficient lead-time to permit action to be taken.	.33	.33	.29	.05	.57	.24	.14	.05	0	.10	.86	.05	.52	.38	0	.10

CATEGORY II – ISSUES OF METHODOLOGY (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
2.13 Analyze the value and accuracy of unconventional methods of forecasting (e.g., precognition, I Ching, etc.).	.05	.24	.67	.05	.43	.24	.29	.05	.05	.24	.67	.05	.38	.33	.19	.10
2.14 Experimentally assess the relative advantages of different methods of eliciting forecasts from a group (e.g., questionnaires, interviews, co[illegible]uter terminals, face-to-face discussion, etc.).	.05	.43	.48	.05	.30	.45	.20	.05	.40	.40	.15	.05	.30	.55	.10	.05
2.15 Experimentally assess how well the same methods work in different kinds of applications.	.24	.52	.14	.10	.24	.62	.05	.10	.29	.52	.05	.14	.33	.48	0	.19
2.16 Experimentally assess how well different forecasting methods work in the same application.	.24	.62	.05	.10	.43	.48	0	.10	.33	.57	0	.10	.29	.57	0	.14
2.17 Experimentally determine the extent to which the same methods produce the same results when used by different forecasters.	.24	.57	.14	.05	.48	.48	0	.05	.48	.43	.05	.05	.33	.38	.24	.05
2.18 Experimentally determine the extent to which particular "systematic" methods of forecasting are demonstrably better than "nonsystematic" methods.	.19	.62	.14	.05	.43	.48	.05	.05	.24	.38	.29	.10	.38	.48	.05	.10
2.19 Devise or improve means of obtaining good estimates or forecasts from persons who fear that the results may hurt them.	.05	.38	.48	.10	.29	.33	.29	.10	0	.48	.38	.14	.24	.48	.10	.19
2.20 Evaluate how self- and peer-ratings of forecasters compare with one another and with other, more "objective" rating systems.	0	.33	.57	.10	.10	.38	.43	.10	.29	.38	.24	.10	.10	.57	.24	.10
2.21 Develop better means of identifying nonmonetary social costs and relating them to monetary costs so that overall systems costs can be estimated.	.52	.33	.10	.05	.67	.19	.10	.05	0	.48	.48	.05	.62	.33	0	.05
2.22 Develop new or better methods for assigning weights to interest groups whose desires regarding a future condition differ.	.14	.48	.29	.10	.33	.29	.29	.10	0	.38	.52	.10	.48	.33	.05	.14
2.23 Develop means for combining multidimensional utilities, such as measures of satisfaction or gratification.	.29	.48	.10	.14	.43	.33	.10	.14	.05	.19	.57	.19	.38	.29	.14	.19
2.24 Develop, for use where there is no single payoff function, a means for measuring the overall degree to which a given action program meets a variety of goal criteria.	.33	.29	.24	.14	.43	.24	.24	.10	.05	.24	.57	.14	.40	.30	.10	.20

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
2.25 Undertake a project to resolve major outstanding problems associated with cross-impact analysis and related techniques, such as trend-impact analyses (e.g., the restriction to event pairs, the meaning of "probability" in these applications, the handling of cumulative impacts, the labor of completing a large matrix, etc.).	.19	.57	.19	.05	.33	.43	.14	.10	.19	.43	.33	.05	.45	.30	.10	.15
2.26 Undertake a project to resolve major outstanding problems associated with Delphi or Delphi-like techniques, such as SPRITE (e.g., role of anonymity, number of rounds, panel size and composition, wording or questions, study flow, etc.).	.14	.48	.33	.05	.29	.52	.14	.05	.24	.43	.29	.05	.35	.45	.10	.10
2.27 Develop better means for combining the judgments of experts from different fields into an interdisciplinary estimate.	.30	.55	.10	.05	.45	.50	0	.05	.10	.50	.35	.05	.35	.50	.10	.05
2.28 Given that one cannot define the complete sequence of events between the present and a forecasted state of affairs, develop methods for telling if any particular intermediary event is a deviation in the overall sequence.	.26	.26	.26	.21	.32	.26	.21	.21	.05	.16	.68	.11	.33	.39	0	.28
2.29 Develop new or better methods for identifying value tradeoffs so as to predict likely support or opposition among groups, and tailor satisfactory policies.	.19	.48	.24	.10	.48	.29	.10	.14	0	.38	.43	.19	.47	.37	0	.16
2.30 Develop means for distinguishing among events which are verbally similar but can be perceived by the policy-maker as having highly different value structures.	.20	.25	.35	.20	.20	.45	.10	.25	.05	.30	.35	.30	.10	.40	.15	.35
2.31 Test forecasting methodologies to determine how precision varies as a function of time horizon, uncertainty of data, etc.	.33	.29	.29	.10	.38	.38	.19	.10	.24	.43	.19	.14	.35	.35	.15	.15
2.32 Devise experiments to test and extend the degree of inherent credibility of particular forecasting methods.	.24	.57	.14	.05	.29	.62	.05	.05	.05	.43	.43	.10	.52	.38	0	.10
2.33 Develop new approaches to condition the individual to a longer time horizon (e.g., through altered states of consciousness methods, "mind games," etc.).	.19	.14	.57	.10	.29	.29	.33	.10	.05	.38	.52	.05	.55	.35	0	.10
2.34 Develop new or better means for evaluating, discriminating, and weighting data points to bias trends toward performance levels most indicative of the true state-of-the-art.	.05	.35	.35	.25	.10	.40	.25	.25	.10	.30	.25	.35	.20	.50	0	.30

CATEGORY II – ISSUES OF METHODOLOGY (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
2.35 Develop better methods for forecasting the quantitative cross-impact effects of technological progress and economic development.	.38	.48	.10	.05	.52	.33	.10	.05	.05	.60	.25	.10	.55	.35	0	.10
2.36 Investigate effects on forecasts of the future of correcting past trend lines to take into account unprecedented events in the past.		X			X				X						X	
2.37 Generalize the distinction between periodic and aperiodic trend extrapolation to cover a broader range of function classes (e.g., 0100110001110000111 . . .).		X			X					X				X		
2.38 Check the probabilities assigned forecasted events by determining the actual frequency ratios that emerge when the events do or do not occur.	X				X					X				X		
2.39 Study the psychology of probability assessment – what makes people assign particular probability estimates – so as to be able to control or correct for extraneous factors influencing such judgments.	X				X					X				X		
2.40 Try function-selecting forecasting methods.		X			X					X			X			
2.41 Develop polynomial curve-fitting techniques that minimize the number of components without giving preferences to low-degree terms.		X			X					X				X		
2.42 Generalize forecasting algorithms so that any set of past or future data points with any set of weights may be used.		X				X			X						X	
2.43 Develop forecasting techniques that distinguish uncertainty due to predictor unreliability and uncertainty due to weakness of the relation between data to be predicted and predictor data.	X				X				X						X	
2.44 Develop techniques for discovering isomorphisms among relations between different sets of variables, so that theoretical hypothetical constructs may be suggested.	X				X					X				X		
2.45 Develop criteria for deciding which kind of mathematical forecasting technique should be tried – e.g., Monte Carlo vs. closed-form, or Bayesian vs. non-Bayesian.	X				X						X			X		

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
2.46 Exploit information theory and analysis of variance decomposition of noise on environment in developing matrix for decomposing multiple impacts.	X				X					X					X	
2.47 Develop a psychometric scaling system for combining cross-impact judgments – e.g., Edwards scaling method, or Method of Triads.	X				X					X					X	
2.48 Investigate the social psychology of Delphi judgment change – what statements influence judgments, how? Start on theory of Delphi convergence forrcasting.	X					X					X			X		
2.49 Develop utility-maximizing, rather than accuracy-maximizing, forecasting.	X				X					X					X	
2.50 Develop decision-oriented rather than forecast-oriented forecasting.	X				X					X					X	
2.51 Develop self-modifying forecasting procedures.	X				X						X		X			
2.52 Develop recursive forecasting procedures.	X					X				X				X		

CATEGORY III — PROFESSIONAL & INSTITUTIONAL ISSUES

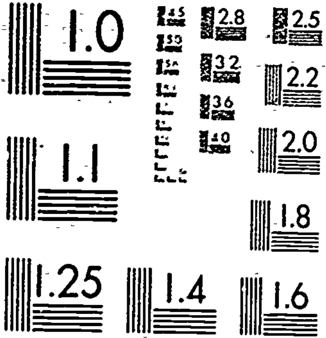
CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
3.01 Establish an annual census of technological forecasting practioners and of expenditures for forecasting.	0	.29	.67	.05	.05	.38	.52	.05	.76	.14	.05	.05	.20	.55	.20	.05
3.02 Develop techniques to enable entire community to participate in futures studies.	.24	.29	.43	.05	.38	.33	.19	.10	.14	.29	.48	.10	.67	.29	0	.05
3.03 Identify, through retrospective studies, factors (e.g., composition, size, and age) that affect group creativity and productivity in FR.	.05	.33	.52	.10	.14	.43	.33	.10	.10	.45	.35	.10	.25	.60	.10	.05
3.04 Assess the influence of funding structure (profit or non-profit) on the objectives and performance of futures organizations.	.05	.24	.62	.10	.14	.48	.29	.10	.25	.35	.30	.10	.20	.55	.20	.05
3.05 Devise means of measuring the "rate of return" — i.e., the cost/benefit relationship — for FR.	.19	.33	.43	.05	.43	.43	.10	.05	.10	.30	.55	.05	.40	.40	.05	.15
3.06 Analyze the extent to which FR can draw upon the experience, data, and methods of other disciplines. (Perhaps one such study per discipline.)	.10	.52	.33	.05	.24	.62	.10	.05	.10	.70	.15	.05	.45	.45	.05	.05
3.07 Evaluate the possibilities of establishing an American Society for Futures Research.	0	.19	.71	.10	.19	.33	.38	.10	.65	.25	.05	.05	.05	.25	.65	.05
3.08 Analyze the effectiveness of existing and possible mechanisms, both formal and informal, for quality control in FR.	.29	.43	.24	.05	.43	.43	.10	.05	.10	.48	.33	.10	.15	.55	.15	.15
3.09 Formulate a list of what appear to be the standards of professionalism in FR and assess their meaning, their worth, the extent to which they are shared, etc.	.14	.43	.33	.10	.24	.38	.29	.10	.29	.33	.24	.14	.10	.30	.45	.15
3.10 Design a code of ethics for professionals in FR.	.24	.29	.38	.10	.33	.24	.33	.10	.24	.33	.29	.14	.10	.15	.65	.10
3.11 Devise or improve means of obtaining international participation in studies that have a cross-national or global significance.	.14	.67	.14	.05	.38	.52	0	.10	.29	.43	.24	.05	.48	.43	.05	.05
3.12 Experimentally assess the value of present academic courses in FR.	.10	.33	.52	.05	.10	.43	.43	.05	.19	.52	.24	.05	.24	.57	.14	.05
3.13 Define what should be included in a four-year program leading to a Bachelor of Science Degree in Technological Forecasting.	.05	.38	.52	.05	.10	.43	.43	.05	.24	.43	.29	.05	.14	.24	.48	.14

CATEGORY III – PROFESSIONAL & INSTITUTIONAL ISSUES (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
3.14 Assess or invent alternative ways of helping present futures researchers to become better historians, to interact more with professionals in other fields, to broaden their understanding of what it is they are doing, etc.	.24	.38	.33	.05	.43	.43	.10	.05	.14	.33	.48	.05	.35	.25	.25	.15
3.15 Devise or improve methods of keeping forecasters better informed of FR work in progress.	.19	.57	.19	.05	.24	.57	.14	.05	.48	.43	.05	.05	.24	.62	.10	.05
3.16 Devise or improve methods for better managing and disseminating the literature of FR, both domestically and internationally.	.19	.43	.29	.10	.24	.57	.14	.05	.57	.29	.10	.05	.29	.48	.14	.10
3.17 Analyze the possibility of establishing a worldwide futures communications network: multimedia, available to all groups interested in the study of problems of the future on a continuous basis.	.14	.38	.43	.05	.43	.33	.14	.10	.24	.43	.24	.10	.62	.24	.05	.10
3.18 Devise or improve means of disseminating the results of FR to the public.	.29	.48	.19	.05	.38	.43	.10	.10	.24	.48	.19	.10	.43	.43	.05	.10
3.19 Establish a government financed information center available to all and having up-to-date data on futures research as well as making government data available.		*X*				*X*				*X*			*X*			
3.20 Design means to communicate complexity (e.g., holistic communications) to the public.	*X*				*X*					*X*			*X*			

CATEGORY IV — SUBSTANTIVE ISSUES

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
4.01 Develop one or more critical screening mechanisms to help the forecaster select among alternative publications, data and data banks, data-processing facilities, models, simulations, communications devices, etc.	.19	.33	.38	.10	.33	.38	.19	.10	.10	.48	.33	.10	.43	.29	.10	.14
4.02 Evaluate the possibility of establishing a center to monitor actual social experiments embodying an "alternative future."	.19	.33	.38	.10	.24	.48	.19	.10	.19	.29	.38	.14	.43	.43	0	.14
4.03 Develop a system for making international comparisons of experience with different types of forecasting.	0	.55	.40	.05	.15	.65	.15	.05	.32	.42	.21	.05	.53	.32	.05	.11
4.04 Conduct periodic surveys to determine what subjects are being investigated in FR, how the pattern is changing over time, and why.	.05	.40	.45	.10	.10	.65	.20	.05	.50	.30	.05	.10	.20	.50	.20	.10
4.05 Conduct periodic surveys to determine how attitudes of people toward the future are changing.	.19	.43	.33	.05	.24	.43	.19	.14	.60	.25	.10	.05	.43	.39	.05	.14
4.06 Forecast the future of FR.	X				X					X				X		
4.07 Research forecasting methods used in the past or in other cultures (e.g., the classical Delphi oracle).		X				X				X				X		
4.08 Investigate forecasting applications possibilities of superintelligent computers.	X				X						X		X			
4.09 Set up organization that tallies (records) statement of intended effect of forecasts, so that a box score can be gotten for forecasters and forecasting methods with respect to whether they accomplish what they are supposed to accomplish (e.g., a dystopic forecast — it may be expected to promote action to make it wrong).	X				X					X				X		
4.10 Set up a program to rate individual judges for accuracy, so as to weight their judgments when combining as in panel methods.	X					X				X					X	
4.11 Set up "futureland" preferences where forecasted scenarios are actually simulated live in real time — for recreation, education, enlightenment, and catharsis.	X				X						X		X			
4.12 Investigate models of the future inside the heads of politicians, top executives, etc., and relate their models to their policies.		X			X					X				X		



MICROCOPY RESOLUTION TEST CHART

NATIONAL BUREAU OF STANDARDS-1963-A

CATEGORY IV — SUBSTANTIVE ISSUES (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
4.13 Develop a radar-like system, internationally, to periodically identify major policy questions and decisions needed for preservation and improvement of the human species.	X				X				X				X			
4.14 Work to identify new policy science paradigms incorporating the need to consider values and to be sensitive to the future.	X				X						X		X			
4.15 Create an information system and directory for future research.	X				X				X					X		

CATEGORY V – ISSUES OF IMPLEMENTATION & USE

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
5.01 Develop means of presenting forecasts (whether probabilistic or deterministic) to ensure their maximum credibility for the user, especially if the events forecasted differ from those within his experience.	.38	.38	.14	.10	.43	.43	.05	.10	.05	.48	.38	.10	.24	.52	.10	.14
5.02 Analyze how and with what effect the value structures of policymakers and their constituencies affect the forecaster's search for a complete range of relevant futures to study.	.24	.48	.19	.10	.30	.35	.30	.05	0	.30	.65	.05	.40	.45	0	.15
5.03 Devise new or better methods for communicating forecasts to the policymaker so that he understands the value implications of different possible events (i.e., how can background differences between the forecaster and user be minimized?).	.62	.29	0	.10	.55	.40	0	.05	.05	.45	.45	.05	.40	.50	0	.10
5.04 Analyze the factors influencing credibility of research outputs as a function of personal and local "cultural" characteristics.	.19	.33	.33	.14	.20	.45	.20	.15	0	.29	.67	.05	.38	.52	0	.10
5.05 Analyze or catalogue different types of organizational structures to identify how forecasts can be structured to match the structure of the organization which must comprehend and utilize them.	.05	.45	.40	.10	.20	.35	.30	.15	0	.50	.30	.20	.20	.55	.05	.20
5.06 Analyze the role of advocacy and its impact on organizational effectiveness.	.15	.20	.40	.25	.20	.30	.25	.25	0	.40	.40	.20	.15	.45	.10	.30
5.07 Analyze how group judgments, in communicating and integrating results, influence and bridge organizational boundaries.	.11	.47	.32	.11	.21	.37	.32	.11	0	.53	.37	.11	.16	.68	0	.16
5.08 Analyze the role of futures research outputs in producing group or individual perceptual shifts.	.11	.53	.32	.05	.26	.47	.21	.05	.05	.63	.26	.05	.32	.47	.05	.16
5.09 Assess the psychological-behavioral impact, both short- and long-term, on participants in group forecasting activities (e.g., SYNCON, Synetics, Delphi, etc.).	.05	.29	.57	.10	.20	.40	.30	.10	.05	.20	.70	.05	.35	.40	.05	.20
5.10 Analyze how and with what affect the same forecasts (or forecasting studies) have been used in different kinds of organizations.	0	.50	.45	.05	.05	.60	.30	.05	.10	.70	.15	.05	.30	.50	.05	[illegible]

CATEGORY V – ISSUES OF IMPLEMENTATION & USE (cont.)

CANDIDATE RESEARCH PROJECT	URGENCY				PAYOFF, IF SUCCESSFUL				TRACTABILITY				COST			
	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC	HI	MED	LO	UNC
5.11 Analyze the possibility of developing new reward systems within organizations so as to extend time horizons, promote innovativeness, etc.	.38	.33	.24	.05	.48	.24	.19	.10	.10	.52	.29	.10	.24	.52	.05	.19
5.12 Develop simulations (games) to demonstrate the utility of technological forecasting in real world situations.	.14	.33	.48	.05	.19	.52	.24	.05	.29	.43	.24	.05	.48	.33	.10	.10
5.13 Study emotional, decision, etc., impacts of forecasts on clients (e.g., what makes a client angry).	X				X					X			X			
5.14 Set up education in the system starting in elementary school, so that FR becomes part of the general cultural background of tomorrow's citizenry.	X				X					X				X		
5.15 Develop a TV-program series to present popularized scenarios, forecasts, etc. to the general public.		X				X				X			X			
5.16 Make a forecast data bank available as a "utility" to the general public through a nationwide (worldwide)? time-share public terminal capability.		X			X					X			X			
5.17 Analyze the possibility of establishing a pool of skills to be available to policy and decisionmakers around the world to work to change paradigms to match transformed situations.	X				X						X		X			
5.18 Develop and publicize uniform definitions of "modeling," "gaming," "normative," and other terms used in FR which have different meanings to practitioners and outsiders.		X				X			X						X	

Analysis

Which of these projects are, in the opinion of the group as a whole, the most important? In what order, with what level of support, should these key projects be tackled? Clearly, a policy-maker concerned solely with making the most profound improvements in futures research would seek out the candidates having the highest estimated urgency *and* potential payoff. That is, these two criteria would dominate the initial selection; subsequently, the research agenda so defined could, if appropriate, be tempered in light of criteria like tractability and cost. How can the most urgent and most promising projects be identified?

This question can be answered in many ways, but one of the simplest—and the one used here—is to rank order the candidates on the basis of several tests for each criterion and then to determine which of the projects come out highest on most of the tests. Those that pass all of them—that is, those that surface at the top no matter which test is applied—can be said to have the greatest potential payoff or the greatest urgency. Similarly, the remaining items can be rank-ordered by the number of tests they pass. Finally, the two rank-ordered lists thus formed—one for "most urgent" and one for "most promising" projects—can be compared; items that are the same on both lists can then be rank-ordered, using precisely the same procedure. The result will be a single list of what can be called the most important projects.

To this end, four tests were defined and applied. First, the projects were rank-ordered for each of the two criteria according to a "total weighted vote." This figure was computed by assigning the "HI" position on the scale a value of 4, the "MED" position a value of 2, and the "LO" position a value of 1; the "UNC" votes were ignored. Next the percentages representing the distribution of votes in the HI, MED, and LO columns for each project were multiplied by these values. The products were then summed to derive the total weighted vote. Obviously, the higher this figure, the greater the estimated urgency or payoff.

The second test was simply to rank order the items in terms of the percentage of votes cast only in the HI column. It need hardly be said that this procedure provides a direct measure of the conviction within the group about the extent to which the project does in fact have a high urgency or payoff.

The third test involved rank-ordering the projects according to the sum of the percentages in the HI and MED columns. The major purpose served by this test is to identify polarities of opinion within the group; in general, the lower the rank, the greater the polarity, though in a few cases a low rank can be attributed to a relatively high level of uncertainty within the group.

The fourth test cut across all four of the criteria of project evaluation. In this case the candidates were rank-ordered on the basis of the total number of times they had been included by the respondents in their set of the three to five "most important" projects. Whereas the first three tests produced different lists for urgency and payoff, this final test gave a result that was, of course, the same for both; as such, it was intended to help ensure that the general feeling of the group was not ignored when looking at the projects in terms of only one of the criteria.

These tests produced four rank-ordered lists of projects for urgency and four for payoff. The top ten items on each of the four lists for urgency were then compared against each other, the goal being to find those items which appeared among the top ten on at least two of the lists. The same was done for payoff. The results are shown in Tables 1 and 2.[2]

These tables should be largely self-explanatory. Thus, on the question of urgency, the first item shown—1.01 "Evaluate the adequacy of current theories of social change"—placed first among all 86 projects in total weighted vote and third in percentage of votes cast in the HI column. It fell to a tie with six other projects for rank positions 34-40 on the test of combined HI and MED votes (thereby indicating that the group was actually quite divided on the matter of urgency). And it placed first among all items in the number of times it was selected as one of the three to five most important items. (Moreover, it received three votes as the single most important project.) On three of the four formal tests, therefore, it emerged as one of the top ten items—making it a very strong research candidate indeed.

It is an easy matter to synthesize these two lists and to derive a set of the most important projects, considering urgency and payoff simultaneously. As explained before, this can be accomplished by first isolating those items appearing on *both* lists that place among the top ten on each of the four tests, then drawing out those (like Item 1.01) which pass at least three of the tests, and so on. When this is done—always on the assumption that a tie on rank order is to be resolved so that the item is given the highest possible place—the heart of a research agenda is established. It consists of the nine projects listed in Table 3.

Table 1. The Most Urgent Research Projects

ITEM	Total Weighted Vote		% of Votes in "HI"		% of Votes in "HI" + "MED"		No. of Top Votes		No. of Times Voted First
	Value	Rank	Value	Rank	Value	Rank	Value	Rank	
1.01 Evaluate the adequacy of current theories of social change.	3.61	1	.57	3	.62	34-40	7	1	3
5.03 Devise new or better methods for communicating forecasts to the policy-maker so that he understands the value implications of different possible events (i.e., how can background differences between the forecaster and user be minimized?).	3.06	2	.62	1-2	.91	1	1	24-46	–
2.07 Devise or improve methods for forecasting the need for forecasts (i.e., for forecasting "the next crisis").	3.06	3	.62	1-2	.81	6-13	4	5-7	1
2.21 Develop better means of identifying nonmonetary social costs and relating them to monetary costs so that overall systems costs can be estimated.	2.84	4	.52	4-5	.85	4-5	3	8-14	–
2.08 Develop criteria and methods of forecast evaluation.	2.80	5	.52	4-5	.81	6-13	5	2-4	3
2.35 Develop better methods for forecasting the quantitative cross-impact effects of technological progress and economic development.	2.58	6	.38	6-11	.86	2-3	2	15-23	–
1.14 Analyze the question of how the validity of results obtained by pseudo-experimentation (construction of a simulation model about the future and experimentation within that model) can be checked or measured.	2.47	7	.38	6-11	.76	15-21	1	24-46	–
1.02 Conduct a rigorous philosophical examination of the relationship between value systems and technology.	2.45	8	.29	19-24	.62	34-40	3	8-14	–
1.06 Analyze the limits of policy control; attempt to clarify what portion of the future is within man's control and what portion is not.	2.42	9-13	.38	6-11	.71	25-27	3	8-14	–
1.07 Devise new or better means for measuring the possible significance of future problems.	2.42	9-13	.38	6-11	.71	25-27	5	2-4	1
2.10 Establish a system for periodically collecting and analyzing past forecasts by each of a number of professional forecasters, with a view toward discovering what was missed and why.	2.42	9-13	.33	12-18	.81	6-13	1	24-46	–
5.01 Develop means of presenting forecasts (whether probabilistic or deterministic) to ensure their maximum credibility for the user, especially if the events forecasted differ from those within his experience.	2.42	9-13	.38	6-11	.76	15-21	3	8-14	1
5.11 Analyze the possibility of developing new reward systems within organizations so as to extend time horizons, promote innovativeness, etc.	2.42	9-13	.38	6-11	.71	25-27	3	8-14	–

Table 2. The "Highest Payoff" Research Projects

ITEM	Total Weighted Vote		% of Votes in "HI"		% of Votes in "HI" + "MED"		No. of Top Votes		No. of Times Voted First
	Value	Rank	Value	Rank	Value	Rank	Value	Rank	
1.07 Devise new or better means for measuring the possible significance of future problems.	3.76	1	.57	7-9	.76	39-44	5	2-5	1
2.10 Establish a system for periodically collecting and analyzing important occurrences that were not forecasted, with a view toward discovering what was missed and why.	3.37	2	.76	1-2	.90	8-10	1	24-46	–
2.07 Devise or improve methods for forecasting the need for forecasts (i.e., for forecasting "the next crisis").	3.34	3	.76	1-2	.86	11-21	4	6-8	1
1.14 Analyze the question of how the validity of results obtained by pseudo-experimentation (construction of a simulation model about the future and experimentation within that model) can be checked or measured.	3.17	4	.71	3	.85	22-26	1	24-46	–
2.21 Develop better means of identifying nonmonetary social costs and relating them to monetary costs so that overall systems costs can be estimated.	3.16	5	.67	4	.86	11-21	3	9-15	–
2.08 Develop criteria and methods of forecast evaluation.	3.01	6	.62	5-6	.86	11-21	5	2-5	3
1.01 Evaluate the adequacy of current theories of social change.	3.00	7-8	.62	5-6	.81	27-36	7	1	3
5.03 Devise new or better methods for communicating forecasts to the policy-maker so that he understands the value implications of different possible events (i.e., how can background differences between the forecaster and user be minimized?).	3.00	7-8	.55	10	.95	2-3	1	24-46	–
2.05 Develop guidelines for the selection and use of social indicators, so as to avoid fuzzy conceptualization – faulty assumptions, etc.	2.96	9	.57	7-9	.86	11-21	5	2-5	1
2.12 Devise a means of identifying discontinuities in trend-lines with sufficient accuracy and sufficient lead-time to permit action to be taken.	2.90	10	.57	7-9	.81	27-36	4	6-8	1

Table 3. A Basic Research Agenda for Futures Research

Items Passing All Four Tests on Both Lists	
None*	
Items Passing At Least Three Tests on Both Lists	
1.01	Evaluate the adequacy of current theories of social change.
1.07	Devise new or better means for measuring the possible significance of future problems.
2.07	Devise or improve methods for forecasting the need for forecasts (i.e., for forecasting "the next crisis").
2.08	Develop criteria and methods of forecast evaluation.
2.21	Develop better means of identifying nonmonetary social costs and relating them to monetary costs so that overall systems costs can be estimated.
5.03	Devise new or better methods for communicating forecasts to the policy-maker so that he understands the value implications of different possible events (i.e., how can background differences between the forecaster and user be minimized?).
Items Passing At Least Two Tests on Both Lists	
1.06	Analyze the limits of policy control; attempt to clarify what portion of the future is within man's control and what portion is not.
1.14	Analyze the question of how the validity of results obtained by pseudo-experimentation (construction of a simulation model about the future and experimentation within that model) can be checked or measured.
2.10	Establish a system for periodically collecting and analyzing past forecasts by each of a number of professional forecasters, with a view toward discovering what was missed and why.

*It should be remarked that Items 2.07, 2.08, and 2.21 did pass all four tests on the urgent list. These are clearly the most urgent projects.

Since these nine projects are the only ones that appear among the contenders on *both* the urgent and the highest payoff lists, they automatically define the basic agenda. To extend the agenda, it is necessary to select items on some other basis. For example, if in the absence of any other guidance urgency becomes the central criterion, additional projects can simply be selected from the remaining items on the urgent list. If anticipated payoff matters most, then that list can be used. But other measures or standards can readily be invoked, such as "number of importance votes" or "highest payoff for least cost." Before discussing some of these alternatives, however, it is necessary to say a bit more about the basic agenda.

Our hypothetical policy-maker, whom we have assumed to be dedicated to advancing futures research by addressing the problems of highest urgency and greatest potential payoff, does not exist, of course. In reality he is many people with widely different levels of authority and interest, motivated in part by values not even hinted at here. He is not completely rational, since the decisions he must make involve factors that can, at best, only be guessed at and, quite probably, will never be specified explicitly—and certainly never be quantified. He is, in short, all of us and, most especially, all of us who are concerned with the future of forecasting. The basic agenda, therefore, is less a program that could be adopted all at once by a single authority than it is a set of highly significant targets—problems and goals—that those with ability, interest, and the requisite resources should work toward as best they can, as regularly as they can. None of these problems, it is safe to say, can ever be "solved"; but they can and should provide a central theme and tendency for fundamental efforts directed toward making forecasting something more than the improvisatory and undisciplined discipline which it is today.

While progress toward this end will thus be made only by many persons, at varying rates, in different organizations or alone, it may be useful to continue the fiction of a single policy-maker and to look at the basic agenda from his viewpoint in an approximation of the real world, where dollars are presumed to matter and where the choice of research projects is also influenced by some estimate of the extent to which these projects lend themselves to research. Doing so will provide insight into the targets of greatest opportunity within the basic agenda.

The questionnaire responses on the issues of "tractability" and "cost" offer very useful first-cut answers. Taking the group's assessment on these two criteria and computing a total weighted vote for each of the nine projects (in the manner described earlier), we can plot tractability against cost and, on this basis, define reasonable priorities within the agenda. The resulting cross-plot is shown in Figure 3.

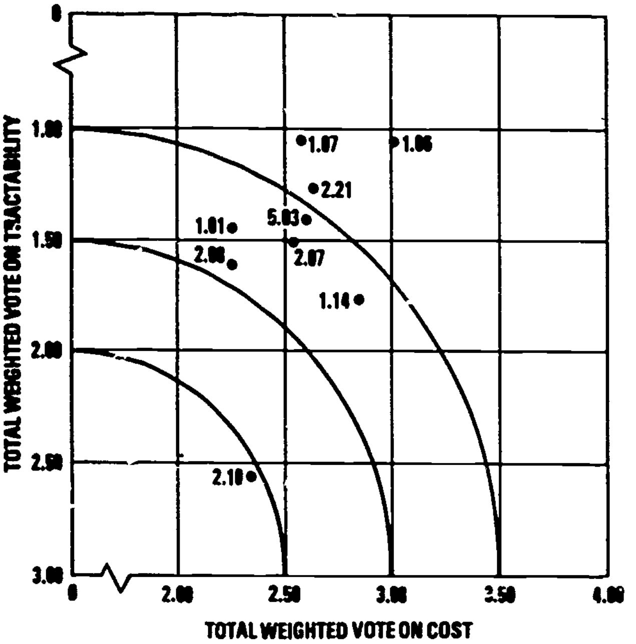


Figure 3. A ranking of projects.

If we assume for a moment that projects should be taken up one by one, and if we accept the notion that the lower the cost and the higher the tractability the sooner a project should be undertaken, then this figure indicates fairly clearly the order to be assigned to particular items. Unquestionably, the project to be chosen first is Item 2.10—a rather plausible place to start because it calls for gaining a deeper appreciation of how and why forecasters have failed to anticipate key developments in the past. Item 2.08 follows; it too is a plausible choice, since it would help clarify our criteria for evaluating forecasts. A good case could be made for the sequence of remaining projects as well. Of course, our policy-maker may wish to invest his budget differently to include, say, the six most important projects (1.01, 1.07, 2.07, 2.08, 2.21, and 5.03). The figure indicates something of the magnitude of the commitment that will be required.[3]

A similar analysis of cost vs. tractability could be used to structure the priorities of other items that might be added to the nine projects in the basic agenda. Several straightforward approaches are available for isolating these projects, using just the data from the questionnaire. As mentioned before, we might simply select the remaining items from the urgent or the highest payoff lists or both. This would have the advantage of satisfying directly the two most significant criteria. We might also select items from the set of projects judged by one or more of the respondents to be the most significant in the entire list of given and newly added candidates. While some of the respondents did not nominate such a project, the results for those who did are quite interesting, as indicated by the project statements in Table 4. Moreover, this approach (if combined with the preceding one) would also result in the inclusion of the dozen or so top-ranking projects as defined by the votes on the most important three to five items.[4]

All of these approaches would also have a further advantage in that the resulting agenda would contain a mixture of large and small, highly challenging and relatively easy projects. As pointed out by the person who suggested Item 2.36, "I know that this is a small task and a specialized one at that. But I believe we may be better off with a modest program that has a high likelihood of substantial payoff than with a more ambitious one that can flop. A series of successes in projects like 2.36 would build up the credibility of futures research and give us the opportunity later on to risk working on some of the more difficult and superficially more dubious projects." One strength of this argument is that it is premised on a recognition of the point made earlier about the fiction of a single decisionmaker with unlimited funds and perfect control.

Nevertheless, all of these approaches share the same fundamental defect: if followed, they would generate an agenda that would bear little resemblance to the balanced program of research which the group seemed to be recommending in its answers to the first question in the questionnaire. For example, if we took as the initial agenda the 15 unique items in the most urgent and highest payoff lists, we would find that five fall into the category of "conceptual or philosophical issues" (1.01, 1.02, 1.06, 1.07, 1.14), seven fall under "methodology" (2.05, 2.07, 2.08, 2.10, 2.12, 2.21, 2.35), none falls under either "institutional and professional issues" or under "substantive issues," and only three fall under "implementation and use" (5.01, 5.03, 5.11). This distribu-

Table 4. Projects Selected as Being the Single Most Important Ones in the Entire Set*

Item No.	No. of Votes	Item Statement
†1.01	3	Evaluate the adequacy of current theories of social change.
†2.08	3	Develop criteria and methods of forecast evaluation.
3.02	2	Develop techniques to enable entire community to participate in future studies.
1.04	1	Analyze the role of values in shaping the future development of values.
†1.07	1	Devise new or better means for measuring the possible significance of future problems.
1.13	1	Analyze how future events are discounted (see Linstone's chapter).
†2.07	1	Devise or improve methods for forecasting the need for forecasts (i.e., the forecasting "the next crisis").
2.12	1	Devise a means of identifying discontinuities in trend-lines with sufficient accuracy and sufficient lead-time to permit action to be taken.
2.25	1	Undertake a project to resolve major outstanding problems associated with cross-impact analysis and related techniques, such as trend-impact analyses (e.g., the restriction to event pairs, the meaning of "probability" in these applications, the handling of cumulative impacts, the labor of completing a large matrix, etc.).
2.35	1	Develop better methods for forecasting the quantitative cross-impact effects of technological progress and economic development.
3.08	1	Analyze the effectiveness of existing and possible mechanisms, both formal and informal, for quality control in FR.
3.11	1	Devise or improve means of obtaining international participation in studies that have a cross-national or global significance.
5.01	1	Develop means of presenting forecasts (whether probabilistic or deterministic) to ensure their maximum credibility for the user, especially if the events forecasted differ from those within his experience.
2.36	*(Newly suggested)*	*Investigate effects on forecasts of the future of correcting past trend lines to take into account unprecedented events in the past.*
3.19	*(Newly suggested)*	*Establish a govenment financed information center available to all and having up-to-date data on futures research as well as making government data available.*

*A few of the respondents did not answer this question

†Included in the basic agenda.

tion of effort among the categories obviously departs from the distribution presented at the beginning of this chapter.

What is needed, then, in order to round out the basic agenda is an analysis *within* the categories, preferably one that would take the newly suggested items into account. This analysis is not attempted here, because it would surely be an academic exercise without more information about how "balance" is to be defined—that is, unless our hypothetical decision-maker's criteria of balance were spelled out. And yet, since it does not seem unreasonable to suppose that all or nearly all of the ingredients of the most worthwhile agenda are present in the list of candidate research projects in this chapter, and thus the problem is merely one of pulling them out, the reader himself may wish to try his hand.

Who, after all, is the decision-maker? Indeed, as suggested by many of the contributors to this volume, where else does the final responsibility lie for the future of futures research?

FOOTNOTES

[1] The quality of the results is unusually high because of the high level of expertise and professionalism of those who completed the questionnaire, and it is a pleasure to acknowledge the contribution made by these individuals. Those who permitted use of their name here include Roy Amara, Harold Becker, Vary T. Coates, James A. Dator, Herbert Gerjuoy, Theodore J. Gordon, Arthur M. Harkins, Willis W. Harman, Olaf Helmer, David Hertz, Robert Lamson, Ralph C. Lenz, Jr., Harold A. Linstone, John McLeod, Joseph P. Martino, George A. Steiner, Murray Turoff, Richard Wakefield, Jonathan Ward, and Victor Zarnowitz.

[2] The column to the far right on each of these tables provides a fifth, though informal, test of the adequacy of the results. Shown in this column is the number of times the items were selected by the respondents as being the single "most interesting and important" project of the three to five outstanding projects in the entire list of candidates.

[3] No one should attempt to read too much precision into the data on the cost of particular items. Given the question that was asked of the respondents, it might appear that we could multiply a total weighted vote by $50,000 and learn what the group believed about the cost of each project. Yet if we believe that these problems do not have final answers (and if, in any case, we remember that these estimates are indeed estimates), the values shown can best be understood as indicating the level of funding that might be needed to launch a single serious and well-defined study on the subject named by the projects. The same probably holds true for all of the other projects investigated in the questionnaire; in the few cases where the respondents provided a specific funding level for a project, the range was between $20,000 and $25 million.

[4] It might be noted that Item 3.02 ("Develop techniques to enable the entire community to participate in futures studies"), which tied for second to fourth place among the 45 projects that were included in the respondents' three to five items, and which is in third place in the list of single most important projects, does not figure at all in either the most urgent or highest payoff list. In each case it failed to rank among the top ten items on any of the other three tests; in fact, its position on these tests never was higher than twenty-fifth and it sank well below fiftieth on one of them because of strong differences of opinion within the group.

Appendix:

Results From The Survey of Current Forecasting Efforts

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and
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The various chapters in this book have considered research needs in forecasting from many perspectives, in an attempt to determine what the literature and the experience of the last 10-15 years reveal about the practice and profession of forecasting today and how either or both might be improved. But to the extent that one would like to have an awareness of the present situation as it really is, in all its complex confusion and enthusiastic experimentation, these sources by themselves provide a truly inadequate answer to such questions—not only because experience is always incomplete or because of problems like delay times in publication, but also because so much that is presently happening has not as yet been documented and may never be. It seemed appropriate, therefore, to try to go beyond these sources, and to this end a worldwide questionnaire survey was conducted during August-November 1973. This appendix highlights the results from the survey.

As stated in the introduction to the questionnaire, the specific objective of the survey was "to obtain information on current research projects designed to advance the art and science of forecasting, whether by (1) clarifying its philosophical foundations, (2) refining or extending its techniques, (3) developing standards of professionalism, (4) improving opportunities for collaboration or the distribution and exchange of results, (5) assaying the appropriate scope of substantive concerns, or (6) enhancing the usefulness of results to executives, other planners, and the public at large." Respondents were asked to use one of the questionnaires provided for each such project, and to be as complete in their answers as possible; the form was designed so that even those questions which might touch on proprietary or otherwise sensitive issues could be answered generally. If none of the respondent's work fell under any of the six headings—and we felt that this would often be the case, since most forecasting work is oriented toward substantive matters (future energy systems, for example)—he was especially urged to answer one question on the form: What major research, in his opinion, should be undertaken that would enhance "the credibility, reliability, professionalism, etc. of forecasting?"

Altogether, some 6500 questionnaires were sent to 937 contacts (individuals and organizations) in 42 countries. The initial mailing list was derived from the files of The Futures Group, augmented by a careful search of standard references like Jantsch's *Technological Forecasting in Perspective;* the De Houghton, Page, and Streatfeild PEP report, . . . *And Now the Future;* and the 1972 IRADES directory, *Social Forecasting.* Additionally, names were drawn from the address list in the Wilcox bibliography on social indicators,[1] the membership directory of the Association of Consulting Management Engineers, a list of national academies of science, and the list of chapters of the World Future Society. Because the widest possible coverage was sought, the criterion for selection was simple: include any source which, by reputation or affiliation, could reasonably be expected to have a current interest in forecasting.

To further ensure adequate coverage, however, it was decided to send several copies of the questionnaire to each addressee, who was invited then to pass them on to others who were doing relevant research, whether in the same organization or elsewhere. Indeed, as many as 20 copies were enclosed with the letters sent to individuals in countries like the Soviet Union, the Peoples Republic of China, India, Brazil, and Australia, the work of which in forecasting is little known in the United States. Large numbers of extra copies were also sent to the leading centers of futures research, in the expectation that the recipient would be able and willing to distribute them among the organization's extensive contacts.

Table 1 summarizes by geographical area the data on invitees and responses. As indicated, the United States accounted for the highest percentage of contacts (56.8 percent), while the Eastern European nations and the Soviet Union accounted for the least number of questionnaires (6.9 percent) and the lowest number of contacts (3.8 percent). In general, organizations in the United States and in Western Europe dominated the list of sources and, hence, the distribution, with a total of 80.7 percent of the contacts and 74.5 percent of mailed questionnaires.

The percentage of contacts that responded (14.1 percent) was typical of surveys of this type, though considerably lower than we had expected, given the number of organizations and the apparent level of activity in forecasting in the world. Aside from the usual drawbacks of such surveys—questionnaires arrive when the recipient is traveling, the task of completing still another questionnaire proves abhorrent, and so on—the explanation for this poorer than anticipated showing may be straightforward: either people are averse to telling others about work not yet completed or very little of the kind of research we were concerned about is being done. The latter is quite probably the case. In any event, Table 1 reveals a decent though hardly overwhelming response. It also shows, incidentally, that including extra questionnaires did not increase the response—a fact open to several interpretations.

The questionnaire, which ran to four 8½ by 11 pages with instructions, asked the contacts to provide several kinds of information, the most important of which were as follows:

1. Name and address of the organization where all or most of the research was being performed. A companion question asked if other organizations or individuals were contributing to the effort, either as subcontractors or as unpaid advisors or co-workers.
2. Source of funding of the research. (Multi-client and self-supported research projects could, of course, be included.)
3. Duration of the project. (If the project was a continuing effort with no fixed date of completion, the respondents were asked to indicate this fact.)
4. Level of effort. (Answers here could be expressed either in man-months or by the project budget.)
5. A description of the substance of the research. ("What problem or set of problems is the work designed to help solve?")
6. A statement of the principal hypothesis or hypotheses being explored. ("What answers do you expect to test or establish?")
7. A general description of the research approach or method being used.
8. References to major work "accomplished at your organization or elsewhere that you view as an important precursor to this investigation."
9. References to major ongoing work that "is likely to support, augment, or perhaps challenge the findings from this project."
10. The open-ended question, mentioned earlier, which was "intended to gather your judgments on (a) how the results of your project might best be extended in subsequent research, or (b) other major research undertakings that should be begun. . ."

In general, the quality of the responses to the central questions in this list varied considerably. Many of the questionnaires demonstrated dubious awareness of relevant research already reported, little apparent communication with other specialists, and narrowness about possible developments in forecasting. Most, however, reflected significant and often sophisticated work and interests, and a more than passing familiarity with the literature. But the range in quality, coupled with the extreme diversity in subject matter and the relatively small sample size, makes it difficult to generalize from the results about the overall status, character, or directions of research in progress. On the other hand, the responses may be indicative of existing or emerging lines of interest. A review of the information offered by the respondents may, therefore, be helpful.

Answers to the first question provided insight into the kinds of organizations conducting the research. The answers, tempered by an occasional guess as to how they should be categorized, are summarized as shown in Table 2. This table indicates that while universities are in the lead, as one might expect, the

Table 1
RESPONSE PATTERN IN THE SURVEY

AREA SURVEYED	INVITEES				RESPONSES					
							RESPONSES BY TYPE			
	Number of Contacts	Average Number of Question-naires per Contact	% of all Contacts (n=937)	% of all Question-naires (n=6545)	Number of Contacts	% of Category Invited	Number of Question-naires	Responses Other Than Question-naires	Total	% of all Responses (n=183)
UNITED STATES	532	4.5	56.8	37.2	68	12.8	81	10	91	49.7
OTHER NORTH AND SOUTH AMERICA	90	5.7	9.6	8.0	6	6.7	4	4	8	4.3
WESTERN EUROPE	224	10.7	23.9	37.3	40	17.9	46	16	62	34.0
EASTERN EUROPE AND THE SOVIET UNION	36	12.5	3.8	6.9	5	13.9	6	0	6	3.3
PACIFIC, FAR EAST, AND OTHERS	55	12.3	5.9	1.6	13	23.6	11	5	16	8.7
TOTALS	937	6.9	100	100	132*	14.1	148	35	183*	100

* An additional 15 or 20 contacts responded too late for their questionnaires to be included in the tabulation

Table 2
WHERE THE RESEARCH IS BEING PERFORMED*

AREA SURVEYED	AFFILIATION OF PRINCIPAL INVESTIGATOR: Unaffiliated Individual	Private Business Corporation	Government	Independent Research Center	Association	University	Total Questionnaires Received
United States	8	17	3	25	3	25	81
Other North and South America	0	2	2	0	0	0	4
Western Europe	0	15	5	8	6	12	46
Eastern Europe and the Soviet Union	0	0	2	0	2	2	6
Pacific, Far East, and Other	0	1	0	2	3	5	11
TOTAL QUESTIONNAIRES RECEIVED	8	35	12	35	14	44	148
% OF TOTAL (n=148)	5.4%	23.6%	8.1%	23.6%	9.5%	29.8%	100%

* Subcontractors or cooperating organizations are not included (see text)

independent research organizations and private industry are close behind. The prominence of industry in this comparison may be encouraging in view of the mixed attitudes toward forecasting among business executives, as discussed elsewhere in this book.

In the aggregate, the projects reported on in the questionnaires appear to involve principally an in-house operation, rather than the use of outside contractors or the participation of other organizations as co-workers. Of the respondents who provided this information, 42.5 percent mentioned subcontracted work or noted relationships with cooperating groups. The types of partners or subcontractors, and the relative distribution by frequency of mention, are shown below:

SUBCONTRACTING OR COOPERATING ORGANIZATION	% OF THE TOTAL SUBCONTRACTING OR COOPERATING ORGANIZATIONS
Individuals/Individual Consultants	28.4
Business Corporations	16.0
Consulting Firms	13.6
University	13.6
Government	12.3
Associations	7.4
Research Foundations	7.4
Private Foundations	1.3

The second question concerned the source of funding for the research. From the 148 completed questionnaires, it would appear that business corporations and the governments of the nations reporting are the two primary sponsors of futures research internationally. A close third are the research organizations themselves—which should surprise no one, since these centers are often forced to use their own resources in order to investigate important basic issues that may never become fashionable or have a bearing on policy. (It might also be remarked that nonprofit research organizations, at least in the United States, typically devote some portion of their profit to self-supported studies in the public interest.) The distribution of support by source is presented in Table 3. Again, the reader should be warned that it sometimes required a guess to tell where a response should be included.

A third question asked when the research began and when it would end. While 54 of the respondents did not complete this part of the questionnaire, the answers of the others indicate an average duration for the projects reported of 2.03 years. (This figure was derived from data for both completed projects and those still continuing.) The data are somewhat skewed, however, so that the median period is 18 months, and the mode is 12 months.

Because the questionnaire was intended to capture information only on those projects in progress, it should be pointed out that of the 148 questionnaires, nearly 30 percent concerned projects that had already been completed or were scheduled to be completed by

Table 3
SOURCES OF SUPPORT OF THE RESEARCH*

AREA SURVEYED	Self-Supported by the Researcher	Government	Private Foundation	Private Business Corporation	Association	Research Institution	University	Unspecified
	TYPE AND NUMBER OF SPONSORS							
United States	9	16	7	16	8	17	10	2
Other North and South America	0	2	0	2	0	0	0	0
Western Europe	1	10	4	15	5	8	2	3
Eastern Europe	0	2	0	0	3	0	1	0
Pacific, Far East, and Other	0	1	0	3	1	3	2	1
TOTAL NUMBER OF SPONSORS	10	31	11	36	17	28	15	6
% OF TOTAL (n=154)	6.5%	20.1%	7.1%	23.4%	11.0%	18.2%	9.7%	3.9%

* Includes only those projects described in the questionnaires, total adds to 154 (rather than 148) because of multiple sponsorship in a few cases

the end of 1973. Since an additional 11 questionnaires described projects with open-ended or uncertain completion dates, only 94 (about 63 percent) definitely can be identified as efforts in progress.

Fourth, there was a related question about the actual funding level or manpower commitment entailed by these projects. Either man-months or dollars could be used to specify level of effort; about 55 percent of the questionnaires contained an answer of one sort or the other.

Only a small number provided the dollar amount. Since some of these projects had a duration in excess of one year, and sometimes considerably more, we normalized the dollar amounts to a 12-month basis, in order to get an impression of the cost of a project year for work of this type. The resulting average funding level was about $104,000 per project year a figure that looks much too high on the face of it, and should in any case be viewed with caution, given the sample size. On the other hand, the median value which emerged from this analysis - approximately $50.000 per project year—would seem at least to be in line with recent experience in the realm of the independent nonprofit and profitmaking research centers. The data distribution also happened to be bimodal, with modes of $10,000 and $80,000, which again *seems* to reflect actual levels when all of the different types of research organizations are taken into account.

A substantially larger number of questionnaires reported level of effort in terms of man-months or man-years. The results indicate an average of 3.1 man-years per project; the median and the mode turned out to be essentially the same—about 2.0 man-years. Either of these values might be reasonable if they include research aids and junior staff personnel. They are almost certainly wrong, however, if they refer only to senior researchers.

Answers to the question asking for a description of the substance of the project itself sometimes conveyed the idea that no one, including the principal investigator, knew what the project was about. Other answers involved a kind of shorthand, though readily translated by those familiar with work of the same sort. But the majority speak clearly to a general audience. The variety in lucidity is paralleled by a variety in subject matter. Nevertheless, when taken together, *the data provide an indication that professionals concerned with forecasting have, in the aggregate, implicitly elected to work on an unevenly balanced set of issues.* A classification of the projects into rough categories (somewhat like those used in Chap. 18, though not exactly the same) makes the point:

I. Research on Philosophical or Conceptual Issues. About 3 percent of the projects can be assigned to this category.

II. Research on Methods. Approximately 25 percent can be included here.

III. Research on Issues of Professionalism. A little over 10 percent of the projects seemed to fall here.

IV. Research on Substantive Issues in Business and Industry. The largest percentage of responses (33 percent) could be included here.

V. Research on Substantive Social Issues. Some 29 percent of the responses can be assigned to this group.

If this categorization is reasonably meaningful, another general conclusion of some significance follows. It is *not* that less than half of the work reported to us (i.e., the items in I, II, and III) can be considered relevant to the aims of the survey, but rather that *a sizable number of the respondents apparently believe that the only sensible way of trying to improve forecasting is by trying to forecast.* Perhaps this is the message of the items in IV and V.

In our opinion, the most discouraging, and yet probably the most important, finding from the survey came in reviewing the answers to the question that asked about hypotheses being tested. Nearly 42 percent of the questionnaires left this question unanswered. Equally striking, a large fraction of those that did have a response merely extended the description of the project or simply offered pieties. In short, only a minority of the respondents seemed to have a hypothesis for their work, or seemed to be aware of what a hypothesis is, or were able to articulate it, if they knew. Even allowing for the fact that some projects may not require a hypothesis (e.g., the compilation of an anthology), the absence of such a statement for most projects suggests that the work itself may amount to no more than sheer rummaging among available methodologies or sources of data. The point is made more than once in this book that such rummaging provides a principal reason why the development of futures research has been slow and irregular, and has often involved waste and failure. It is, therefore, disconcerting to observe in the questionnaire results that the tendency apparently still prevails of undertaking studies with no clear, explicit notion of what one expects to discover or verify, much less any clear, explicit notion of the possible value of the results. if actually obtained. For a policy-oriented activity like futures research, this augurs a continued slow and irregular growth.

The next question, on the general approach being used in the research, was accompanied by several examples of the kind of answer being sought: "historical case study, computer modeling, laboratory experimentation, essay, poll, and so on." As a result, the number of specific techniques named was quite limited and it was possible to group the answers into the small set of categories given in Table 4. Delphi, trend extrapolation, and system dynamics are shown separately because of the frequency with which they were mentioned, and the meaning of two of the given categories changed somewhat in view of the responses: "essay" came to include scenario writing, and "experimentation" came to include psychological tests and social experiments. The "other" category served to collect references to techniques like cross-impact analysis and content analysis.

An interesting point not made by the table is that among contacts who completed a number of questionnaires, a single method tended to be cited throughout, despite differences in the research subject and in the hypotheses. This may give further credence to the general belief that the choice of methods is determined as much by a researcher's earlier enthusiasm for a particular technique as by the appropriateness of the technique to the project in question. In sum, organizations and individuals ride methodological hobbyhorses. To put it differently, they specialize.

Answers to the final three questions—regarding antecedent research, current related research, and promising lines of new research—were often alike in being cliquish. The important *earlier* work was done by the respondent, others in his organization, or his sponsor; the important *current* work was being done by the last two; the important *new* work would be to extend the present work. These are fair answers, of course, and the instructions in the questionnaire may even have encouraged them. Moreover, in some cases it might be pedantic to refer to major sources (e.g., a familiarity with Forrester if one is working in system dynamics). But the in-group character of the responses can make one wonder. After all, it is one thing if a Rand Corporation cites only its own work or if a Herman Kahn cites no one, since either is apt to be doing something unprecedented. But Rands and Kahns are rare, and even they do unprecedented things very rarely. Thus, what the questionnaires may show in these instances is the presence of the infamous not-invented-here factor, or at least the possibility of a genuinely serious problem in professional communications. In this regard, it is worth noting that a large number of the respondents neglected to answer any of these three questions.

For whatever it may signify, it is also worth noting that out of the 6500 mailed questionnaires, just one was

Table 4
METHODS BEING USED IN THE RESEARCH*

AREA SURVEYED	Delphi	Polling or Other Surveys	Modeling (except as included elsewhere)	Trend Extrapolation	System Dynamics	Scenario Writing and Essays	Historical Case Studies	Experimentation	Other	Unspecified
United States	15	10	18	8	5	7	15	10	8	16
Other North and Southern America	0	1	1	0	0	0	1	1	0	1
Western Europe	7	4	18	4	4	7	9	4	0	8
Eastern Europe and the Soviet Union	1	1	3	1	0	3	1	1	0	1
Pacific, Far East, and Other	3	0	6	0	0	0	0	0	2	11
TOTAL NUMBER OF APPLICATIONS	26	16	46	13	9	17	26	16	9	28
% OF TOTAL (n=205)	12.7%	7.8%	22.4%	6.3%	4.4%	8.3%	12.7%	7.8%	3.9%	13.7%

* Includes only the methods given in the questionnaires; the total exceeds 148 because many projects involve the use of two or more methods.

returned by a respondent who accepted our invitation to suggest a possible new line of research.

Because the research suggestions, in particular, are commonly tied to the projects described, we have not attempted to list them here separately, though a number of them were singled out for evaluation in the analysis reported in Chap. 18.

FOOTNOTES

[1] L. D. Wilcox, R. M. Brooks, G. M. Beal, and G. E. Klonglan, *Social Indicators and Societal Monitoring: An Annotated Bibliography* (San Francisco, Calif.: Jossey-Bass, Inc., 1972), pp. 433-464.

A Bibliography of Research on Futures Research

Wayne I. Boucher

This bibliography references many of the technical and general-interest publications that were reviewed during the course of this project. It is, therefore, current only to the date of the formal end of research on the project—early 1974. Hundreds of papers and dozens of books have appeared since then, a few of the more important of which are cited here, though without any claim to being truly representative in the selection. Because the rate of advance in research on basic issues in futures research has been maddeningly low, this bibliography can still be considered to be current, even though it does not include many of the most recent titles.

The bibliography has several other limitations that should also be mentioned. Like the project itself, the bibliography is concerned not with actual forecasts, but with ideas *about* forecasting—its current strengths, its limitations, and its future. Moreover, while writers of many nationalities are represented, the bibliography is further limited in that it omits materials in languages other than English. Thus, it is far from exhaustive, even within its own domain. Nevertheless, it should be of value to many readers in that it brings together an important sample of publications from across the spectrum of interests that must be considered if forecasting is to become more useful, and if futures research is to become more sophisticated and professional.

Aristotle says somewhere that the worst of books is not wholly bad, because it can make us think of what is better. The same is true, of course, for the best of books. Quality varies throughout this list, as inevitably it must, but most of the items can be recommended without qualification. The few patently bad publications which have been included are present by design, since they are so obviously instructive, and since they sometimes represent the best discussion so far of the problems they address.

Forecasters and futures researchers tend to have global concerns, as indeed they should, and their works are not easily categorized. Hence, to avoid what would be an arbitrary and probably meaningless classification, the entries in this bibliography have been listed alphabetically by author. One important advantage of this arrangement is that it compels browsing.

It should be emphasized that futures research still lacks a basic, comprehensive, critically annotated bibliography—itself an important candidate for research, since the problems of preparing such a work are complex, even if attention is limited only to English-language publications. Several helpful general bibliographies have been issued, however. Among those that have appeared since 1970 and are still accessible, the following early compilations can be recommended:

Charles N. Ehler, *Integrative Forecasting: Literature Survey*, Exchange Bibliography 252 (Monticello, Ill.: Council of Planning Librarians, January 1972).

Annette Harrison, *Bibliography on Automation and Technological Change and Studies of the Future*, Paper P-3365-4 (Santa Monica, Calif.: The Rand Corporation, March 1971).

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Weldell Bell and James A. Mau (eds.), *The Sociology of the Future: Theory, Cases, and Annotated Bibliography* (New York: Russell Sage Foundation, 1971).

L. D. Wilcox, R. M. Brooks, G. M. Beal, and G. E.

Klonglan, *Social Indicators and Societal Monitoring: An Annotated Bibliography* (San Francisco, Calif.: Jossey-Bass, Inc., 1972).

Among the most recent compilations are two guides available through the World Future Society:

Michael Marien, *Societal Directions and Alternatives: A Guide to the Literature* (Lafayette, N.Y.: Information for Policy Design, 1976).

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