LONG RANGE FORECASTING METHODOLOGY

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PREFACE

These proceedings of the Second Symposium on Long Range Forecasting and Planning contain the papers which were presented and the discussion of those papers. The discussion was recorded during the symposium, and has been edited to clarify the discussants' meaning. However, during the editing process, a conscious attempt was made to retain the flavor of free discussion which marked the symposium. We feel that the interchange of information and opinion during the discussion adds significantly to the formal presentations, and we hope that both the papers and the discussion will prove to be of value to all those concerned with methods of long-range forecasting and planning. The reader should remember that the opinions presented by the speakers, particularly during the discussion, are their own, and do not necessarily represent the official position of their organizations.

Special thanks are due to Mrs. F. E. Carter, from the Office of the Staff Judge Advocate, Holloman AFB, for recording the discussion of the formal papers; and to Colonel Weldon, the Staff Judge Advocate, for making Mrs. Carter available for this task. Also we wish to acknowledge the valuable assistance of Majors Claude Stephenson and Eugene Erb in editing the transcript of the discussions.

We wish to acknowledge the efforts of Mrs. Ann C. Masi, Mrs. Wanda Climenhaga, and Mrs. Patricia Mergler in preparing the manuscript for publication.

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DELPHI

Norman C. Dalkey, Rand Corp.

INTRODUCTION

Delphi is the name of a set of procedures for eliciting and refining the opinions of a group of people. In practice, the procedures would be used with a group of experts or especially knowledgeable individuals.

The significance of the Delphi technique should be examined in the context of what I call the advice community. Both industry and government are served by a large group of consultants who purvey information, predictions, and analyses to aid the formation of policy and making decisions. The community is a highly miscellaneous assortment of in-house advisors, and external consultants from academia, other industries, non-profit corporations, and, of course, any other walk of life that appears relevant to the problem facing the decisionmaker. Some of this advice is based on solid generalizations from observation, either of the crude empirical variety of somewhat more prestigious deductions from established scientific principles. A great deal of it is opinion.

The notion of opinion is extremely fuzzy, but with your indulgence, I would prefer not to try to make it precise. With respect to the interests of this conference, I believe you will agree that in the area of long range forecasting of technological and social developments there is an especially large admixture of opinion. For this area, the creation of techniques for refining opinion is of particular interest.

Pragmatically, a basic characteristic of opinion as opposed to more solid knowledge is the fact that if you interrogate several equally competent individuals, you are likely to get a divergence of answers. This is obviously not a defining characteristic, since uniformity of response does not guarantee the solidity of that response. From the standpoint of the decisionmaker, a divergence of estimate creates a problem of how to use the estimates in fashioning his policies. There are several heuristic devices that are traditional in the advice community. One is to select a single advisor on some grounds (ranging all the way from personal friendship to lustre within the community). This usually guarantees a certain uniformity. Another is to involve several knowledgeable individuals and employ some method of group interaction to arrive at a common opinion. The most popular of such methods is that of the committee, or commission, with a variety of informal ways to arrive at the sense of the committee.

Selection of a single advisor in soft areas is clearly fraught with danger; on the other hand, committees have certain drawbacks which have been dramatized by a large number of investigations by psychologists and small-group sociologists over the past two decades (*1*). One major drawback is the influence of the dominant individual. A quite convincing series of studies have shown that the group opinion is likely to be highly influenced, if not determined, by the views of the member of the group who does the most talking, and that there is no significant correlation between success in influencing the group and competence in the problem being discussed. Another difficulty which has not received as much attention in the literature is noise—irrelevant or redundant material that obscures the directly relevant material offered by participants. A third difficulty is the group pressure that overvalues reaching a consensus ("A committee is something that designs a camel when you want a horse.")

DELPHI PROCEDURES

The Delphi procedures have been designed to reduce the effects of these undesirable aspects of group interaction. The procedure has three distinctive characteristics:

1. Anonymity.
2. Controlled feedback.
3. Statistical group response.

Anonymity is a device to reduce the effect of the socially dominant individual. It is maintained by eliciting separate and private answers to prepared questions. Ordinarily, the procedure is carried out by written questionnaire; on-line computers have been used for some exercises. All other interactions between respondents is through formal communication channels controlled by the experimenters.

Controlled feedback is a device to reduce noise (among other things). A Delphi exercise will usually consist of several iterations where the results of the previous iteration are fed back to the respondents, normally in summarized form.

As a representative of the group opinion, some form of statistical index is reported. For cases where the group task is to estimate a numerical quantity, the median of individual estimates has turned out to be the most useful index tried to date. Thus, there is no particular attempt to arrive at unanimity among the respondents, and a spread of opinions on the final round is the normal outcome. This is a further device to reduce group pressure toward conformity.

A typical exercise is initiated by a questionnaire which requests estimates of a set of numerical quantities, e.g., dates at which technological possibilities will be realized, or probabilities of realization by given dates, levels of performance, and the like. The results of the first round will be summarized, e.g., as the median and interquartile range of the responses, and fed back with a request to revise the first estimates where appropriate. On succeeding rounds, those individuals whose answers deviate markedly from the median (e.g., outside the interquartile range) are requested to justify their estimates. These justifications are summarized, fed back, and counterarguments elicited. The counterarguments are in turn fed back and additional reappraisals collected. This basic pattern has, of course, many possible variants, only a few of which have been tried.

The procedure has been exercised with material where there is no immediate way to evaluate the results—e.g., long-range technological and social developments—and also with material where there is the possibility of checking, such as short-range economic predictions and estimates of quantities where the actual figures are obtainable, typically almanac-type material. For material where confirmation is possible, typical outcomes are that opinions tend to converge during the experiment, and more frequently than not, the median response moves in the direction of the true answer. In the case of material where confirmation is not possible, all we can say is that opinions do converge during the exercise (*2*), (*3*).

One additional feature of present Delphi procedures should be mentioned. Respondents are requested to make some form of self-rating with respect to the questions. Several different kinds of self-ratings have been tried—ranking the questions in the order of the respondents judgment as to his competence to answer them; furnishing an absolute estime of the respondent's confidence in his answer; estimating a relative self-confidence with respect to some reference group. In general there has been no significant correlation discovered between such self-ratings and individual performance for confirmable estimates. However, it has usually been possible to use the self-ratings to select a subgroup of relatively more confident individuals where the performance of the subgroup has been slightly, but consistently better than the group as a whole. In one very thorough study, the improvement was obtained only by combining two self-rating indices—ranking of questions, and absolute estimates of confidence (*4*).

RESULTS OF EXPERIMENTS

There are many things we do not understand as yet about the information processing going on during a Delphi exercise. Thus, we cannot as yet determine how much of the convergence is due to three different factors which are clearly at work: (1) Social pressure, (2) rethinking the problem, (3) transfer of information during feedback. Several exercises have been conducted that throw some light on this. In one (*5*), a set of 20 almanac-type questions were posed to a group of 23 respondents. A control group of 11 respondents were given the same questions, but on the second round were simply asked to reassess their answers, with no feedback whatsoever. They were not even told what their previous responses had been. In general, except for two questions, the amount of convergence was comparable for the two groups, and the accuracy of responses for the control group was as good as for the second-round responses of the experimental group. This would appear to indicate that a major factor in this exercise was rethinking. However, the effect of social pressure and/or information transfer is also indicated by the fact that for the experimental group the interquartile ranges of the second round responses were uniformly contained in the interquartile ranges of the initial responses, whereas for the control group the second-round ranges were contained in the initial ranges for only 13 out of the 20 questions.

To try to pin down a little more the factors involved, we conducted an experiment this summer comparing the performance of structured face-to-face discussion groups and the anonymous questionnaire technique. The experiment was guided by two presumptions. (Hypotheses is too pretentious a notion in this rather unstructured subject.) The first presumption was that in a face-to-face situation, information transfer is likely to be much greater than in the anonymous controlled communication situation. This would presumably tend toward greater accuracy on the part of the conference estimates. The second presumption was that the effect of undesirable social interactions could be ameliorated by imposing a specific format for the discussions. The format employed was: for each question a new discussion leader was selected by chance; the leader listed on a blackboard all relevant information (including opinions) suggested by members of the group; he then listed as many different approaches (little models) for answering the question as the group could devise; estimates were made by each approach; and finally a group consensus was arrived at by informal agreement.

The presumption to be tested was that a structured conference of this sort would produce more accurate estimates than the questionnaire technique. The experiment was performed using a group of graduate students engaged in summer consultant activities at RAND. There were 10 participants, divided into two groups of five. There were 20 questions, of the almanac sort, divided into four groups of five. Each participant group answered 10 of the questions by questionnaire, and 10 by structured discussion. The only innovation in the Delphi procedure was to interpose a pure information round between the first and second estimation round. Each respondent was allowed to ask the group two questions and the group replies were fed back before the second estimate was made.

The major outcome of the experiment was that the presumption that the structured discussion would turn in a better performance was not borne out; in fact, the questionnaire responses were, if anything, somewhat more accurate than the structured conference responses. The difference was not significant except for one measure, namely the sums of ranks of standard scores,[1] in which the questionnaire technique showed up as better.

For the discussion groups, no adequate measure was obtained for the role of dominant members, noise, and pressure for consensus; but it was clear from observation of the discussions that the structure imposed was inadequate to eliminate these effects.

An interesting anomaly appeared in the performance of the questionnaire groups; namely, the responses on the second round were more accurate than the responses on the fourth (and final) round. Whether this was due to fatigue—for each set of five questions, the entire set of responses was obtained in one afternoon session—or due to a saturation effect (all of the relevant information elicited by the second round, and simply wandering estimates from then on) cannot be determined from the data.

Perhaps the significance of the experiment can be most sharply summed up by the following conclusion: If the conference groups had been requested to open their session with anonymous individual guestimates of the answer to each question, the median of these off-the-cuff guesses would have been more accurate than the group consensus obtained after a more or less thorough discussion of the subject.

[1] *Standard scores were computed by dividing the group estimate by the true answer. The 40 responses were ranked in order of accuracy, and the sums of these ranks taken for each configuration (group, method, question set). The analysis of variance for the sums of ranks indicated a difference between the two methods significant at the .05 level.*

DISCUSSION

Delphi procedures are still in an experimental stage with regard to applications to the advice process. The evidence is mounting that systematic processing of expert opinion can produce significant improvements both in accuracy and reliability (using the notion of reliability to refer to the range of estimates). However, the role of Delphi procedures within the corpus of forecasting techniques—extrapolation, simulation, demand analysis, gaming, etc.—has not been established. In particular, there are no cases that I know of where Delphi procedures have been explicitly employed to support specific policy decisions. Hence, there are no direct comparisons of the relative effectiveness of the procedures versus other more traditional forms of advice. Perhaps Mr. North will have some examples to fill this void. The studies that I am familiar with in areas relevant to policy have been more like exploratory exercises to test the feasibility and manageability of the procedures with extensive subject matters and geographically scattered experts. In this respect, the procedures have turned out to be manageable, but often rather cumbersome.

A common reaction is to imagine Delphi as a method of obtaining inputs for some kind of formal estimating structure—e.g., inputs for a simulation model. I must confess that at times I find this is an appealing notion, but it cannot be the full story. Most often, for those areas where data is lacking, a formal model is lacking as well. As a matter of fact, the Delphi procedure is one of the most efficient ways I know for uncovering the implicit models that lie behind opinions in the soft areas. One of the most valuable side products of a Delphi exercise concerned with strategic bombing was the skeleton of a model which was later fleshed out in great detail (2).

There are several tautologies which are directly relevant to the group estimation process: (*a*) The total amount of information available to a group is at least as great as that available to any member, (*b*) the median response to a numerical estimate is at least as good as that of one half of the respondents, (*c*) the amount of misinformation available to the group is at least as great as that available to any member (this one is usually overlooked in discussions of the advantages of groups versus individuals), (*d*) the number of approaches (or informal models) for arriving at an estimate is at least as great for the group as for any member, (*e*) corresponding item for approaches as (*c*) for information. For simplicity I have included noise in misinformation and poor approaches.

These tautologies do not add up to anything like a theory of the group estimation process, but they are suggestive. For example, (*c*) and (*e*) hint that there may be an optimal size of group for a given kind of estimation. This would be in accordance with some experimental results with small discussion groups. They also suggest that part of the group estimation process should be concerned with active suppression of misinformation as well as filling voids in information.

We have no way at present of determining whether the questionnaire-feedback procedure is anything like an optimal use of the information available to a group, or whether it includes a mechanism for reducing the effect of misinformation. Nor can we say that it is most effectively used in isolation, or within the context of other methodologies. In short, there is a very large field waiting for the plow.

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Mr. Irwin. You touched rather briefly on one topic that I wondered if you would expand on. You mentioned the variance to the questions, in your twenty questions, was greater than the variance between the control groups. Since the control feedback would obviously be a very important factor in Delphi technique, the person who controls the controls and his attitude would be important. Specifically, I was wondering in some or all of the experiments you conducted, do the people who control the controls know the answers?

Dr. Dalkey. The answer is they, at least, have the answer available and probably know something about it. If you are asking the question whether we made a study of the influence of that, we haven't. I would like to say that is an important factor certainly which has not been looked at. You are quite right insofar as the feedback is controlled, is, in fact, manipulated by the experimenter. The experimenter is part of the experimental situation in this case. And he should be controlled. He has not been controlled in the experiments we have conducted.

Mr. Irwin. It seemed to me particularly important that if he knows the answer, he is going to filter out some noise which he knows is noise but nobody else does.

Dr. Dalkey. That's correct.

Dr. Taylor. We were just visited by Richard Suitman and we were having a discussion on what is called criterial learning versus noncriterial learning. This means in one case we are playing with the unknowns—no one knows the answer yet—e· .he other case where the experimenter knows the answer and these may be radically different areas of performance and different areas of experimentation. This is a very young field in which you are working, as you indicate, and I think we should make this distinction and at times if we take truly long range forecasting and planning, I think we are playing with this noncriterial type of thing. This is just an observation.

Dr. DALKEY. I couldn't agree with you more, and we have made some attempts to distinguish between these two types of material in the studies we have made. Unfortunately, we are too impatient to wait 20 years to check some of the results. It is clear for a number of very important issues, you simply have to think in terms of experiments that will stretch over 20 or 30 years, there is no way around it.

Colonel DAVIS. Perhaps this would be a good time to mention briefly a small amount of work we, at OAR, have been doing in the Directorate of Plans which seems to indicate there might be some connection between the Delphi technique, for example, and use of the growth curve or logistic function in order to describe the evolution of a technology or series of technologies. I have a couple of viewgraphs and it might be appropriate to show them at this time. First of all, you re-

FIGURE I–1

a. *The Cumulative Logistic Distribution*

(Hypothesis for Helmer Forecast)

$$G(t)=\frac{1}{1+\exp\left(\frac{-\Pi}{\sqrt{3}}t\right)}$$

where $t=\frac{x-\mu}{\sigma}$

and μ=mean of the x values (15.75 yr)

σ=standard deviation of the x values (8.43 yr)

$$\frac{dG}{dt}=\frac{\Pi}{\sqrt{3}}G(1-G)$$

member the report by Gordon and Helmer from the Rand Corp. which was entitled "Report on Long-Range Forecasting Study." We would like to have some way of getting an analytical description of the evolution of technology using one of the Delphi group's predictions. In this case, we chose the predicted progress in space, a set of predictions. In the report there was a graphical display of the predicted progress in space. One of the axes was time in years and the other was the events which we tool to be arranged in some degree of technological difficulty. An example of some of the events predicted, for those who don't happen to remember them in detail is: Soviet Union orbital and U.S. orbital rendezvous. They were 1964 and 1967 respectively. 1970, for example, U.S. manned lunar flight by manned scientific orbital station, 10 men; 1975, something like ionic propulsion, man to Mars, Venus flyby, and so on. In trying, at least, to get some kind of exponential growth related to this, we tried to plot it and found that a plain exponential growth didn't work but a cumulative logistic distribution of events did, in fact, fit within some reasonable statistical sense. First, let me show you the equation (see fig. I–1). (1–G) indicates the availability of knowledge to be exploited which is occurring in a random fashion whereas G is the actual growth of the technology. The abscissa on the second chart (fig. I–2) is made up of years, 0 up to 40 and is keyed to the year 1960 which is the year 0, going to the year 2000; the ordinate represents a measure of the technological difficulty and was obtained from Gordon and Helmer's results. This is simply an assumption from the results they had obtained in an arbitrary fashion up to 1984. If you get the arithmetic mean of these events for the different years and plot a continuous line connecting them, you have a line that is reminiscent of a cumulative distribution of some quantity. Take the expression just shown and make the appropriate substitutions then you get the dashed line (fig. I–2) which, when you apply the chi square test to indicate statistically the difference between one and the other, is not unreasonable. If you wanted to reevaluate when the events might have occurred, which are above and below the arithmetic mean, you might shift one this way and push one that way and get a reevaluation of the actual times. In conclusion, the result gives an indication there might be some connection between the Delphi technique results and a previously used method of technological forecasting. The cumulative logistic distribution has been used to describe the growth of populations and growth or development of other events, the evolution of other quantities; and is indicated by Dr. de Sola Price and other writers as having some validity; that is, a cumulative logistic distribution describes the evolution even of science as a whole and presumably this is associated technology.

Dr. DALKEY. There are two possible hypotheses we can make, one is that this is an estimate on the part of experts as to the way in which technology will go and the other, that this is a statement about the psychology of the experts.

Colonel DAVIS. Not only that but you could elucidate a theory of growth of technology as described by the product of a couple of terms, (1–G) being interpretable as the cumulative distribution of availability of knowledge when it is being exploited against time. It is being exploited by technology. In other words, it starts out at value 1 and drops on down. In turn, the technology goes to its maximum capability according to this hypo-

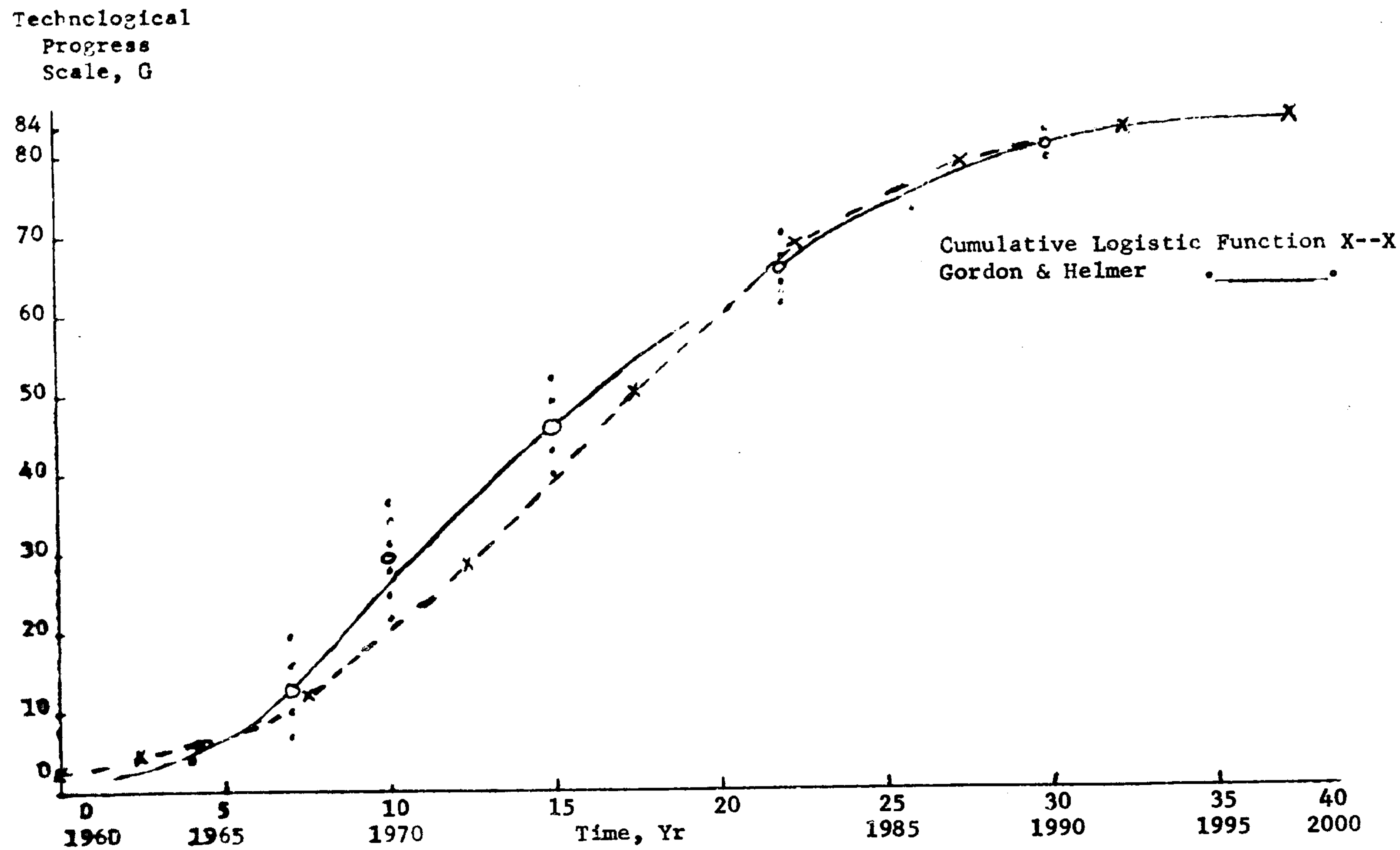

FIGURE I-2.—*Comparison of Logistic Probability Function and "Predicted Progress in Space" (Gordon and Helmer—Report on a Long-Range Forecasting Study)* thesis and levels off. Also $1-G$ is recognizable in the distribution of physics. The distributing of quantum states, some type of order situation, where only one particular level can be occupied by one particular quantum state. There is something which leads you little further which we have not done up to this point to see if there is further correspondence that might be interesting.

Dr. JOHNSTON. With respect to this logistic curve, we have had, before my time, a very unfortunate experience in using logistics for a reason that may be relevant in technological control—that is, in demography you don't really have a closed space except in some ultimate sense that has to be defined out of the physical, perhaps the psychological, so where you are on the curve today cannot be specified nor can you fix the indexes. There may be an ultimate maximum level. In the field of technology, if you restricted the argument to a particular area of development, it makes sense to say that as you approach the ultimate which you agree exists, you are going to have a harder time, like saying to get 1 percent down is a lot tougher than when it is already fairly high. You start introducing soap and you get good results. Now you are dealing with cancer and getting very tiny results. I don't know if you are talking about technology without an upper limit or whether it is appropriate.

Colonel DAVIS. The upper limit appears to be provided, at this time, by the estimates of the Delphi group involved in this particular exercise and some of the results came out "never," which puts a level on it; however, the upper limit is somewhat in doubt and will, of course, influence the whole shape of the curve.

Dr. LINSTONE. I would like to ask a question of Dr. Dalkey. I had some trouble with the Delphi method going back to this question of the first two sociological statements. Information was missing which gets down to the problem in some areas: Is the majority or consensus more correct than the maverick opinion? It seems to me some areas can apply—I'll just mention two: The scientific breakthroughs beyond the year 2000 on the scale; another area: military long-range plan. Consider as an example the military area. If you tried to get a consensus in 1956 on army mobility in 1966, most of it would be consensus based on 1956 which would be misleading and not valid. The same, for example, in an historical instance, in the area of science when the gas turbine began in 1940; where we began, the maverick opinion was collecting most of the records. I don't know whether Delphi would have helped; when you get such problems as the inbreeding of experts you get the fact that breakthrough is unpredictable by definition and it very often comes in another disciplinary area. The estimates are in one area and something else comes from the outside. I wonder if the almanac or Lincoln or the questions you mentioned are valid tests for these kinds of applications because it is one thing to test known data, obvious to the expert; if he is interrogated properly you would expect a group of this type would be better in determining it than an open meeting. By putting the first two tautologies together, what do you get? I would like a comment.

Dr. DALKEY. Yes, I am hoping Dr. Taylor will raise and discuss more thoroughly the area of evaluating experts. We haven't done a great deal of work on this at Rand and it is clearly a very difficult subject. We haven't done anything about this problem of inbreeding you mention which I think is very serious. There are a large number of aspects of the advice community which has some drawbacks similar to those of smaller committees. The community itself pertains to some of these drawbacks which raises difficulties—difficulties that I shall not comment on at the moment. The conservatism of the established member of a community is very undesirable. With regard to the long-range forecasting type of material, obviously, we haven't been able to do anything experimental in that area. The question whether the results we derive from experiments with confirmable material is applicable to the nonconfirmable material; I think to some extent it is applicable. That is, the difference between postdiction and prediction is not as great as we sometimes would like to think. A question like the popular vote for Lincoln, in a sense that was trying to make a prediction backwards. I don't believe that's enormously different from making a prediction, but there are differences. As far as using a group consensus, the use of a statistical measure like the median rather than a more direct way of getting group agreement, I believe the results we have obtained from the experiments are directly applicable in that case. I think no matter what material you are dealing with, the use of a more objective technique for arriving at the group response will increase the accuracy of what you get from the group. I feel that what it does is to get rid of, or at least mollify, ameliorate, the willingness of members of a group to throw away information they know in order to arrive at a group response by allowing differences of opinions to remain and simply reporting, Here's the range of opinions that we got, and the median seems to be about the best way of putting it together. By doing that, we allow a maximum of information to remain in the decision.

Commander HAMILTON. Several of us are a little unfamiliar with the Delphi technique. We are wondering about the background and development. Do you, at Rand, have a special group involved in this technique or is this a sideline? You may have touched on this at your symposium last year.

Dr. TAYLOR. Can we get the reports from last year describing the Delphi technique?

Dr. DALKEY. In my paper I have some references with which you can obtain some background on Delphi. As far as historical questions, there are studies on the subject that have been going on at Rand since the beginning of Rand in a rather low key. By low key, I mean, rarely have there been more than two or three senior people involved. It has been intermittent. We started in 1948—1949—looking just at this aspect of anonymity. We asked whether it was possible to take a set of estimates from a group and improve them by statistical advice. This was applied to all kinds of questions such as forecasting the outcome of horseracing among other things. We found that it was possible to use statistical averaging techniques to get results that were better than most responses of the respondents. What do I mean by that? This tautology says the median is better than half of the estimates. That's tautology. You can always be sure that's true; however, in most of the experimental results you will find the following: This is a set of estimates; that is, the quantity we are trying to estimate and you receive some type of distribution in answers, the true answer will be somewhere on this scale. In most of the experiments the median is closer to the true answer than more than half of the respondents. It would be closer to the true answer in, say, three-quarters, five-eights of the cases. It's not something you can derive from logic but it is something that turns out, to come from experiment, and we have found that we were just looking for statistical group responses. That is a rather crude way of going at the problem and we gradually introduced the feedback way of refining the opinions around 1953–54. There was a little hiatus around that time. The reason for the hiatus was, we were exploiting some of the models I mentioned earlier which we discovered by the Delphi technique. I think it is fair to say that this is the reason for an increased interest in Rand and elsewhere in this technique has been the emergence of a greater interest in long-range forecasting. The reason for that is, this is an area where we don't have a great deal of information. We don't have a large number of theories. If you are going to make decisions based on long-range forecasts you are going to use opinions and, therefore, a way of refining those opinions seems to be particularly important. I don't know whether that answers your question.

Mr. CETRON. There is a book which you may find useful by Gordon and Helmer, "Social Technology" published by Basic Books (1966), that would answer your question. I am one of the believers that hindsight is better than foresight by a damned sight and I believe the real key question is control. As you mentioned, you can control results if you know what the results were when you check your information from past performances. If you know something happens and have the information you can also control the future pretty well. One of our admirals said he can make these forecast self-fulfilling by controlling the expenditures of the money. I think we find that some of these things are feasible but may never be picked up. Your forecasting is strictly what is possible, and what will happen depends on what resources are put in. Is there any information or background work on this particular area?

Dr. DALKEY. Not a great deal. Let me make this comment on the role of Delphi with regard to decision processing itself. Normally, the decision process is laid out like this: You have a series of strategies, policies, courses of action, that you can carry out; a set of possible states of nature over the future, which will influence the outcome of those strategies and the general decision process then consists in finding a figure of merit for strategy versus the outcome. One way is to find some estimate of the likelihood or probability of the state of nature, and then use some expected value as a way of scoring the strategies and deciding on them. There are a couple of features we have not gotten and have not appeared a great deal in the literature so far, but are obviously on the periphery and very important. First of all, the kinds of discount rates you are going to use in scoring policies for events that are far in the future. If you look at the normal social process, you would swear that for many social developments, it is enormously high. For example, the standard one I use for this purpose is: Probably one of the elements of the long-range picture that is most sharply etched in the minds of long-range forecasters is the population explosion. It is also one of the things that in some sense has the greatest social value of any of the kinds of events that are talked about. It is a disaster of the first order that we are looking forward to with a great deal of certainty. The question is how much is being done about it at the present time. If you want to look at this question as a social discount rate, how much of our resources are we actually spending, at the present time, with regard to that problem and the answer is practically none. Most of the investment we are making at the present time with regard to that problem has to do with birth control and you can see it is possible to discount most of what is being done regarding birth control in terms of present demand. If women in the United States weren't interested in birth control devices, we probably wouldn't be doing anything about it. If we didn't think the population of India was already too big, we probably wouldn't be doing anything about it. In short, here is this great looming disaster facing us with a very high probability and we are investing practically nothing. In that case, the social discount rate is enormous. That's the kind of problem where we can use Delphi techniques and I don't know any other technique that would give us a handle. I don't know whether that answers your question.

Dr. CHACKO. You raised the question about the relationship between Delphi techniques with other means of forecasting and as I listened to you, I thought there was one large area where Delphi could be used and has not been, at least you didn't tell us, namely, your examples of almanac situtations, such as Lincoln's popular vote in 1860. I believe you are probably throwing away a considerable amount of information which relates not to the outcome or the ultimate results, but to the logical process which this group has. The role of the maverick is denied when you use the median of the numbers. Have you worked on the logical process of the participants in your experiments?

Dr. DALKEY. The answer is no. We think it is important but we haven't dug into it in an experimental fashion.

Dr. CHACKO. I have done a limited experiment in this connection. In your speech you mentioned the application of Delphi to business and government, however, I will leave the business part to Dr. North. I will report on what I attempted with respect to the basis of forecasting for national policy making. The features I wanted were: (1) Situations should refer to the highest policy matters; (2) the process underlying the policy recommendation should be highlighted; and (3) the maverick responses should be highlighted and not ignored.

What I developed is a bluff-threat matrix. Every event in the cold war was identified in terms of one or more of 110 bluffs. The bluffs are simply any characterizations of situations. When someone says, "There is a bomb in this building," it is a bluff irrespective of the truth of the matter. It becomes a threat the instant you look for an exit even though you may break your leg trying to escape.

What I did was identify 110 categories which all conceivable events of control could again be categorized. Please feel free to add to the 110 bluffs. Corresponding to the characterizations, I identified 90 responses, and I am glad it came out as a 110 times 90 matrix rather than 100 times 100.

I presented these characterizations to the team at Bendix, at the University of Michigan, arms control group. It was a group of scientists whose reaction was quite favorable. Two weeks later, I gave it to the Joint War Games Agency people. There we had, of course, representatives from all agencies and after explaining the rules, I had them play the Vietnam game using only unclassified information as the bluff: how do you see the war situation and what are the objectives. The amazing thing was that in spite of the most outlandish hypotheses such as Cuba having a hand in Vietnam, and the resolutions they wanted to make, I was able to show that each man had a large matrix. When it was put together, there was a reasonable area of consensus. First, if there was anyone who suggested tactical nuclear weapons, that would show up as a different "X" and would not be discarded. Secondly, we are forcing each man to say why he indicated such action, which I think you are losing if you merely look at the quantity results. So he says, "because of these, therefore I say such and such."

The merit of this approach is that instead of 15 volumes of 500 or 600 pages, the President of the United States can look at one matrix representing not only the policy recommendations but also the logic leading to the recommendation.

With the 500- or 600-page volume, the President is damned if he doesn't follow the advice on the last page but he is also damned if he does, because the 499 pages preceding contain caveats. But, with the bluff-threat matrix, both the reasoning and the recommendation are shown and any maverick is not ignored but recognized. The orientation is toward substantial policy matters of national concern.

Major MARTINO. I'd like to say something regarding the control exerted by the experimenter. From the standpoint of the use of the Delphi procedure as an advice giving or advice gaining instrument for the policymaker, his role is different from that of an experimenter in an experiment determining the behavior of people in the Delphi situation. If he wants to alter the feedback or control the information in a way which gives predetermined results, if he wants to kid himself, he doesn't need to go through the elaboration of the Delphi procedure. The real danger, as I see it, is a man may be kidding himself unconsciously.

When I ran a Delphi experiment, I made a deliberate decision at the outset not to include any responses of my own or to sneak in any predictions of my own because I believed I ran the danger of kidding myself consciously or unconsciously. In short, I hired a team of experts and if I had been an expert I wouldn't have hired them. So I stayed out of it. But I would like to inquire of Dr. Dalkey if any other attempts have been made, or if you have looked to the problem of unconsciously fooling yourself when you are the policymaker who has hired experts to run through a Delphi sequence and give you advice.

Dr. DALKEY. Let me first distinguish between experiments and technical applications of the procedure. There is a big difference between using the Delphi procedure as a tool of investigation for methodology and using it as a tool for putting information in a form which is useful to a decisionmaker. These are two different types of operation. One is an application of the techniques and the other is a study of the techniques and the information processing situation. In the past this distinction hasn't been as sharp as we would like it to have been, but a number of exercises which have been run using the Delphi techniques have really not been experiments. They have been applications of the procedure. As far as I am concerned, when you are applying the techniques there are no holds barred. If the experimenter wants to think of himself as being part of the expert community, it seems to me that is a perfectly reasonable thing to do. In some of the exercises we have run, of the applied sort, our feeling was that after the exercise was finished, the group had in some sense a kind of expertise which none of the respondents had. They had a better grasp of the entire field, and of the interrelationship of the factors than any single respondent. It is a way of helping a very small group of experts to become more expert. It is a way for them to become acquainted with all the richness of the field; to become as clear as possible as to the structure of that field. As a device for a small group of experts who want to, in a sense, become as rapidly acquainted with all the richness of the field as they can, it seems to me it would be an extremely valuable device. That's irrespective of the other qualities of the technique. In that case, it is not only reasonable for the experimenter to manipulate the material before he feeds it back, it is reasonable for him to impose some structure on the feedback material. In short, if you think in terms of several rounds, by the end of the second round of the experiment, you can say, "A pattern is emerging. I begin to see what this field looks like. Now let me check my intuitions against the whole panel of experts." However, as I say, that application is not an experiment. I think you have to be very careful in interpreting the results of that kind of an exercise. In the material which I presented this morning, I tried to be careful to refer to exercises where we felt the amount of interference of the experimenter had been sufficiently small so we could say we were dealing with an experiment and not with a sure exercise.

Dr. OBERBECK. My question is aimed at focusing attention on what is an expert and what do you mean by the accuracy which you defined with respect to the almanac event. My question is: In what respect do the Gallup polls meet your criteria with the exception of the last point and if the Gallup poll is a Delphi technique? However, if we know exactly how the Gallup polls are run, then what does expert mean there; and what does accuracy mean there; and what do we know about accuracy in respect to the Gallup polls?

Dr. DALKEY. I am not an expert on experts. We haven't done any studies which really illuminate this enormously. First of all, presumably an expert is someone who just has a fair amount of miscellaneous background information in the field; and if you take the standard type of definition in the selection of experts, you will find that's usually the kind of criteria used. It usually comes down to a simpler one which is the number of publications a man has put out. It is a standard academic type of criterion, and that is a measure of the amount of background information people have. But there is a point which is extremely important. No work has been done, but there is some informal analyses of what we have done in this area and there is a big distinction between having a wide range of background information and being able to use that information to estimate the answers to new questions. In short, there seem to be two characteristics involved in what you would like to think of as an expert: A lot of background information and then some kind of skill in putting that information together. Let me give the result of one study we made when we were working with the statistical response itself with the almanac type of question. There it was possible to ask a very large number of questions in order to make statistical analysis of the results. In one study, we asked a rather intricate question. The kind of response we were getting from individuals was in terms of the probability of a statement being true. These were statements where the answer could be obtained. We deliberately mixed true and false statements and we simply asked the respondents to estimate the probability that the statement was true. We then introduced a notion of precision, the idea of precision was the following: Suppose you take all the responses of a given individual in which he said the likelihood of this statement being true is 75 percent. We took all of the responses in groups. For example, take 75–80, count the number of questions where he responded 75–80, then ask what was the proportion of true statements in that group. The idea was that a good predictor was one where, roughly speaking, if you said 75–80, then 75–80 percent of the statements in that group would be true. We discovered that almost all of the respondents were good in this respect. If you used just this measure, they all did very well. This was rather startling; however, it turned out that there were a few who almost never used 50 or 60 percent. They always stayed quite well up in the 75's 80's, and 90's. Others were very fond of 50, 60, or 40. There was one individual who was very fond of 95 and gave a very high proportion. Well, 95 and 5, 5–0, 95–100. It turned out that he also was very good in the sense that when he said something had a probability of 95, it was very likely true. If he said it had a probability of 5, it was very likely false. He is the kind of chap we like to call an expert. We didn't find very many.

One frequently used method of generating forecasts is to ask an expert in the field. This leaves another problem, however. How does one go about identifying an expert? Professor Taylor describes some of his research on means for identifying various kinds of talents in individuals. This can be extremely important, as his work has shown that everyone is good at something, and no one is good at everything. An expert in the field to be forecast may well turn out to have no talent at all for forecasting. He would thus be an unfortunate choice as a source of forecasts.

—Editor

UNSOLVED PROBLEMS IN SELECTING EXPERTS IN PLANNING AND LONG-RANGE FORECASTING

Calvin W. Taylor, University of Utah

It has been both an opportunity and a challenge to prepare a presentation on this topic. Dr. Dalkey opened the field in an excellent manner with his clear communications. In fact, he provoked enough thought in me for a second entirely different speech from the one I will give. I particularly liked his analogy that the field is ready for the plow, with which I strongly agree.

First let me mention briefly some other fields of talent which we have already plowed and show how these enable us to be more ready now to plow the fields of planning and forecasting. My approach will be a more broad theoretical one. However, the many specifics that he has been citing would provide many research opportunities to learn more about those who are more expert from those less expert. His last comment indicated clearly that people differ quite widely in their planning and forecasting talents, which is the heart of the selection problem. Let us extrapolate from what we know in some other high-level talent areas and from what little we know in planning and forecasting talents as a way to anticipate what likely will happen as we do more R. & D. work on these newer talent areas.

First, let me make an apology for our field for having made a contribution some 60 years ago and then having oversold the public, or perhaps used inappropriate words to have misled the public. If we should sketch a large circle to represent all the various things that this tremendously complex brain, or man's mind, can do, we put together some 60 years ago a test and we called it by the big word "intelligence." Now that we have hindsight, which is far better than our foresight, we can see that intelligence definitely does not measure that entire large circle but measures only one small circle within the much larger circle. With this little circle being intelligence but with people thinking we are talking about the big circle instead of the little circle, then you realize the confusion that we have created. The total research to date indicates that something like two-thirds of that large circle has been explored. We have found at least six or seven other circles (or talents) in the areas of the known, which, on the average, might be equal in size and importance to that first talent which we called intelligence. These other talents as yet have been largely ignored in programs for identifying the talented. They have also been neglected in educational programs, too, because we have oversold educators on the word "intelligence."

The plan for the intelligence test was to measure what was already occurring in schools. So we produced a closed system of intelligence intercorrelated with grades and grades intercorrelated with intelligence—but unfortunately grades do not correlate very well with what one does in the work world. This is the dilemma that exists and our profession has probably been as tardy as anyone in finding out that this is a closed system. The academic world is one world and the world of the professions and other high-level talents is another world and there is comparatively little relationships between these two worlds. In one case, we have 800 correlations between performances in the academic world and the professional world and the average of these correlations is .00, with a random error curve around zero describing very well the histogram display of all these correlations.

We have also looked at educational practice in terms of the basic research on human characteristics. We took several views of it, one of which states that in this third perspective, education is viewed in relation to the actual world-of-work requirements. Then we talk about the Ph. D. on-the-job being a higher degree than the Ph. D. in the academic world and how certain effects of the academic world have to wear off before a person can attain the real degree which is only earned by becoming truly productive scientists.

My doctoral professor was L. L. Thurstone, a mechanical engineer who worked for Edison before becoming a psychologist. He did most of the initial basic work on what were called primary mental abilities, stretching the number of these talents or abilities beyond the seven or so in intelligence tests to about 20 before his death. Guilford and his students have since picked this up and developed it much further. If we want to consider Guilford's periodic table of the mind (The Structure of Intellect, see Taylor, 1964), approximately 80 out of a possible 120 cells in his periodic table can be identified to date. The intelligence test includes about six to eight or if we are very generous, we could say almost 10 out of the 80. That gives you a feeling that we have gone a long ways in the talent area beyond what we knew 60 years ago, except in communicating this to the public and in widening the practices.

One of these other new talent areas on which we have been working very hard is creativity and another one is communication abilities. A further talent area which we are now trying to open up concerns planning abilities or talents. Frankly, we have not yet paid much attention to the equally challenging talent area of long-range forecasting.

Incidentally, my most recent experience in planning was in planning this speech. At times I couldn't help but wonder if I had a good enough plan and pondered if I should abandon it and start over. As indicated earlier, I was sparked to a new set of notes from the first presentation and maybe I will wish afterward that I had changed the plan in order to speak from these newest and freshest notes.

Let me tell about some of these later approaches our profession has generally used in these newer talent areas. Creativity, very appropriately by its nature, represents our breakaway talent. Through our findings on creativity we are trying to break away from this strongly held but erroneous notion of one and only one kind of talent, one and only one kind of giftedness. Contrary to this widely held notion, the overwhelming evidence is for multiple types of talent and multiple types of giftedness.

Let me open up the work on creativity with my story about a plan for getting rich quickly, though obviously I haven't put the plan into effect yet. I will tell you about how it all starts, in terms of life and creative talent on this earth for a given individual. I have a plan to open a national chain of schools, so although I am here officially to make this presentation, on the side I am laying the groundwork to open another one of my new schools. That is my real agenda here. Of course, when we think about schools, we ask at what level should we start applying our creativity work. We always come up with the answer of starting at preschool levels. I will use the best psychological advertising. All at once I will open up a nationwide chain of these nursery schools and turn on flashing neon signs across the Nation. The signs will advertise: "A Nursery School for Creative Children Only." Then someone says, "You still have a problem in selecting the children. How are you going to select them?" I have a very simple answer: "I'm just going to let the parents decide." That's how it all starts as far as creative talent in one's life is concerned.

There has been a lot of research on what happens to potential creative talent as people move through their academic world and into the world of work. Every time we review these findings, we always want to go back as early as possible to start at preschool ages and shoot the works to implement all we have learned about creativity so far. Tomorrow I will work with the Colorado State program for gifted and creative children. Colorado is a leader in being aware of and recognizing officially at least two kinds of giftedness. We are going to try and open further tomorrow this awareness of their 450 workers throughout their State to include multiple types of giftedness.

Researchers in creativity have been asked how to start having our findings implemented into classrooms. One way I responded was by writing a series on some clues to creative teaching, on some hints about teaching for creating activities in the students. This is a pattern I would like to see followed in planning, too, by writing a series and by developing demonstrations on teaching for planning activities in the students. Through these steps eventually the students can have the opportunity to experience creative processes and planning processes and can develop their creative talents and also their planning talents during their regular classroom programs.

Recently we have been doing many biographical studies, life history studies of creative people, especially scientists and engineers (as published Mar. 3, 1967 in "Science"). From studying their life programs, i.e., how they have been programed to date, one can forecast whether they are likely to be creative or productive or not. This is probably as good a single approach as available for forecasting whether a person will be productive and even creatively productive in science. It is also our best single approach for forecasting performances beyond the academic world in any field. Inside the academic world, the usual study has been to predict school grades. Most studies have done that and most studies aren't worth very much because grades don't forecast other things well, except other academic grades. This is one of the real dilemmas in the field, so I am pleased to work with you to start breaking further away from forecasting performances only within the academic world.

Recently we have finished some work for the Peace Corps in which we were trying to identify those with talents and characteristics that are needed overseas in the Peace Corps, new talents like sensing problems, resourcefulness, initiating action, innovativeness, and creativity. We have been studying what characteristics are needed to be top performing physicians and nurses, too, so we are not studying low level and applied fields after one leaves a so-called high-level academic world, but instead we are focusing upon very high level fields and professional performances and accomplishments.

With AFOSR support we have done several studies on communication abilities and creative abilities just published as a GPO monograph (Taylor, Ghiselin, and Yagi, 1967). In these studies of communication abilities needed in large organizations we found several high-level talents, such as revision abilities and talking abilities, to be needed in several important supervisory and executive activities; we also found that many of these important communication talents are often being ignored in usual educational programs. Such programs tend to emphasize reading and writing much more than revision or oral communication, including talking and listening abilities. Even though students do take English and language arts courses from 12 to 16 years in school, we have evidence that the kind of communication abilities which are important in the world of work are not ones in which they get much training. Revision abilities is our best single example of a high-level talent in the total complex set of communication abilities which is generally neglected in educational programs.

As shown in figure 1, our theory for education entails a multiple-talent approach. We are learning how to develop creative and other new talents in classrooms. We have strong evidence from many demonstration

DIFFERENT TALENT PROCESSES IN STUDENTS

CONTENT ACQUIRED BY STUDENTS	Academic Talents	Creative Talents	Communication Talents	Planning Talents	Forecasting Talents	Decision Making Talents	Other Types of Talents
Language Arts	X						
Social Studies	X						
Humanities	X						
Arts	X						
Biological Sciences	X						
Physical Sciences	X						
Mathematics	X						
Other Subjects	X						

FIGURE II 1. *The Two Student Centered Dimensions of Content and Processes.*

studies that two things are happening simultaneously in the students. One is that they are acquiring subject matter and the other is they are using some internal processes to deal with and acquire that subject matter. There are many different internal processes that students could use while they are acquiring subject matter and these different processes could be and are rightfully called different talents. So students can acquire subject matter by the academic talents (sometimes facetiously called the tape recorder talents) or they can acquire subject matter by creative talents or they can acquire knowledge by decisionmaking talents or by their planning talents or by their forecasting talents, and so forth.

Some early evidence even suggests that if students are given opportunities to practice these talents or processes, these talent processes are likely to show more transfer of training and be more valuable in later life than will the sheer knowledge that they acquired, partly because of the exploding and changing nature of man's knowledge. We suspect that schools have focused almost so highly on knowledge acquisition that much of the meaning of knowledge has been lost to students; too often we teach them the dead corpse of knowledge after all the "life" is out of it. We now have some fairly strong hints that if we focus more on talent development in students than we do in knowledge acquisition, then interestingly enough, the students acquire more knowledge as a by-product than when we have them focus so highly and directly on its acquisition.

The X's in the first column in figure 1 shows that generally students have developed primarily their academic talents and have stayed within this narrow band of talents while they are acquiring subject matter. As a breakaway we are doing all we can to learn how to add another column of X's by having them develop their creative talents while they are acquiring subject matter. We have made similar progress with communication talents. We are now starting to get ready so we can give students experience in developing their planning talents, their forecasting talents, and their judgmental or evaluative (decisionmaking) talents, as referred to earlier.

All research and the classroom demonstrations are leading to the conclusion that what is needed in education is not just more of the same, but instead to try to develop creative and planning and other talents. One of our demonstration studies by Hutchinson is quite devastating to some educators. He had two comparison groups. One was the typical classroom where students are thought of as learners. The second type of classroom which was found to be quite a different type with different people being the star students; it was one where students are thought of as thinkers rather than merely learners, or absorbers. So, we are ready for another type where the students are to be thought of as planners, and still another where they are to be forecasters. Those gifted in the learners classroom are not those who are gifted in the thinkers classroom and they in turn are not those who will be gifted in the planners classroom or in the forecasters classroom. So much for my introduction to the matter of talent identification and cultivation in education. Let us now move toward the world of work.

As a consultant with the Stanford Research Institute I have had many challenges in dealing with several groups of vice presidents in charge of planning. This has been a delightful experience. After working with people of all ages, I must honestly say that I have rarely run into a group of people more hungry for information and insight, who are more searching for capabilities and techniques on how to do things, than this group of vice presidents in charge of planning.

Let me explain with a brief story which will illuminate, through perhaps exaggerate their predicament. It goes like this: Whenever a person holds jealously unto himself any activity as his own sole prerogative, all others around him will be deprived of experiencing whatever that activity is. In other words, whatever talents are called for in those activities will tend to remain dormant in other persons who are deprived of participating in those activities.

When a person is born into this world, he has potential planning talents that could become active and be used. But he soon finds that as an infant and as a child, he does not get too much of a chance in his own home to do much planning because someone else considers planning to be their function and duty and holds this planning function jealously unto themselves. But he soon grows up and finally he gets a chance to leave his home during the day and go to school. When he gets in school, he may think, "Now I will be able to use my planning talents and they will become active instead of dormant talents." But he soon discovers that someone else does all the planning around there. In fact the evidence is that he is probably working under extremely close supervision in school and is highly dominated by the teacher. In about 80 percent or so of the time, the teacher is talking or otherwise dominating the class. So apparently he doesn't get too much of a chance to do any planning there.

After he finishes school, he gets a job and hires in at the bottom of the ladder and thinks, "Now I'll get a chance to do some planning." But suddenly he discovers that someone else does all the planning there. (One of the audience suggested the next possibility to me.) So the person decides to get married and does so, but he soon discovers that someone else does the planning there, too. (Incidentally, it has been said that two people get married and they become one. The problems start when they try to decide which one.)

If he is a good worker, he eventually is promoted to be a first-level supervisor. Now he feels he is going to do the planning, except he soon finds that the first-level supervisor doesn't do the planning. It is apparently done higher in the company. So he continues to be a good worker without doing any planning and he again is promoted upwards. After 30 or 40 or more years of his life in programs where he is deprived of all experiences in planning and by having done so well in nonplanning activities, he suddenly is rewarded by being appointed vice president in charge of planning. And believe me, he and others like him are hungry for information and skills in planning.

In our educational chart (fig. II-1) let me give you an example of how we might fill out one of those X's for planning talents in students while they are acquiring subject matter. I will deal with a known rather than unknown area of subject matter, since teachers, by and large, are not yet prepared to deal in areas either at the fringe of knowledge or out beyond where no one yet knows the answer. One possibility is to give students a map of the physical and biological resources of our Nation without any markings about the population distribution. Show them where the mountains, forests, rivers, the fertile ground, and all the natural resources are. Then ask them to plan a nation, saying: "Where would the people start cities and where would they be doing different kinds of things." Let the students plan where the people would be and what kind of activities would occur in which places in the nation. The teacher would keep secret from them where these things have already occurred. After they map out the whole thing, chances are that they will be eager to check up to see to what degree the nation had really developed according to their plan. We suspect that they will be much more interested in acquiring this information than they would be if we had just dished out all the answers to them as is often customary, on a silver platter to learn (memorize). Then, after you did that, you could lead them into the unknowns which will emerge from population explosions by having them make long-range forecasts through these instructions: Now, where is the rest of the expanding population going to go? What are we going to do now in light of man's latest knowledge—in terms of what he can now do in the way of utilization of resources and new means of production of energy and so on? Where will the populations of the future be located and distributed?

This gives you an example of how they could both plan and check up on their plans from the past and in the second case, how they could map out a long-range forecast or plan for the future. We could also check and see if any of their future plans were any better than the plan of the moment of our experts. Maybe they can think more freely. There is some evidence that when students get into school, they learn how not to be inquiring as they go further through school. Maybe the earlier that we get them started, the better they can use their natural planning and forecasting talents without built-in restrictions from their negative life experiences and training. No doubt, all of you could suggest many more planning tasks that could be used in testing and training the planning and the forecasting talents of students. We should soon need such suggestions.

As we look at these multiple types of talent, there is a possibility that the really top persons, the "true experts," are those who have a high profile not only in planning talents, but also in one or more of these other pertinent talent areas, too, such as creativity (including flexibility) and communicating and decision making, and in effective integration of these various types of talent activities. So particular talent combinations could produce a very interesting approach for studying the selection of experts.

I think we should realize that whether we have used good experts or poor experts, in any event we do have a plan and this plan can and usually does shape the future to some large or small extent. It does affect people and most of them tend to work within the plan, not outside the plan, and the plan is imposed almost as a restriction upon them. So the future is partly shaped by a plan, whether the plan was produced by people with a high degree of planning talent or not.

In the same vein, I am told that the President of the United States can always guarantee that he can get advice. What he cannot guarantee is that he can get wise advice. So you see some of the talents that the psychologists of the past have ignored and postponed for the future have been some of these other high-level talents, like exist in the wise men or in those that have great foresight and vision and prophecy ability and judgmental ability, creative ability, and so on. We are starting to move more vigorously into plowing these fertile talent areas that were largely ignored until a decade ago.

Man has a tendency to narrow a problem down so he can cope with it, so some people often work in a very narrow frame. Certain persons may have fewer ideas and can only juggle fewer ideas and less information and less variables in their minds at one time. This is one of the potential bases for differences in people's creativity and it may be true about differences in ability to plan and also to make long-range forecasts. If a person deals with fewer variables at one time, his resulting plan or forecast may be equally restricted by his lack of dealing with all pertinent variables. Maybe those who are best are those who can juggle and manipulate many more variables at one time, and eventually work with them all at once and then suddenly pull them together into a plan or a forecast.

One incident in the history of the behavioral sciences in this Nation should be recalled in which they made some forecasts. After the forecasts, they got together and took stock nationally and professionally about this business of forecasting. This was the time when the newspapers erroneously printed that Thomas Dewey had been elected President of the United States. One bit of advice that the forecasters from the behavioral science gave themselves was that it would be quite possible to retreat into only those areas of forecasting where there is no severe checkup on their work and where they still would be able to make a good living. Or they could carefully avoid making anything but the longest range forecasts which would be so far ahead that they would be safe since few of their generation would still be around alive to check up on them. Instead, they argued to stay in the election and other tough forecasting arenas where they would continue to encounter severe checkups on their forecasts.

A great deal of our research effort is spent on the criterion problem of measuring the total complexity of career performances and accomplishments of professional and other people. For example, we obtained 50 measures of what research center scientists had contributed in their career. We have 80 measures of what physicians accomplish in their career and we are told we didn't cover enough, so we have a new project to move further into measuring their performances and achievements. We have a somewhat different set of 80 measures to date for medical faculty than we do for specialists in medicine, and also a particular set of 80 specially for general practitioners. Thus, we are struggling with a great complexity of human performances and human characteristics in any given profession.

Another result that we always find when we study human beings is that there are always individual differences and often they can be great differences. So I am persuaded that we will find great complexities in the performances plus large individual differences in the various planning and forecasting activities. I felt that Dr. Dalkey presented some evidence on this point of great individual differences. In a lot of his group studies, it might be quite possible to tease out studies of individuals as well as of groups. Perhaps ways should be found for some of my research staff to team up with Dr. Dalkey's group so that findings on individual differences can also be extracted from his group studies.

Another feature in which we are interested is what would be called variability within a person from one time to another. This variability is in contrast to what we are most often seeking, namely consistency and reliability within a person. In some cases, variability within a person might alternately be described as flexibility instead of inconsistency or unreliability within a person from one time to another time. Some of Thurstone's last projects before his death dealt with psychophysical measures of what might prove to be important temperamental traits such as flexibility in individuals. I noted in listening to Dalkey's comments that we have a lot of psychophysical techniques which could be applied to the kinds of things that are now occurring in his on-going planning and forecasting activities.

When groups of people are making judgments and we see that they differ individually, one of the things we might ask is Should we weigh their opinions equally or should we give them some differential weight? Some might facetiously say, "One way we can give them differential weight is to give greater weight to those who had higher school grades or who have higher IQ scores." But, as the previous speaker indicated, it is amazing how their talents in one area are not particularly related to their talents and performances in another. This is our finding, too. Good things go together slightly, and not by any means synonymously. This is indicated by a little bit of overlap between circles sketched to measure talents, with only 10 percent overlap between two talent circles—and you know 10 or less percent is not much overlap. With good things going together only slightly, we must avoid or be very cautious about weighting according to what may be an almost irrelevant characteristic. For example, we should try to avoid letting the greatest weight be given to the person who talks the most on the topic in the group. Just the fact that he talks the most does not mean he is the best planner or the best forecaster or the best judge or the one with the most creative ideas or with the highest quality of creativeness of ideas.

I honestly have the impression that there are very, very few psychologists who are working in these important live world-of-work planning and forecasting situations. Many others would defend themselves by saying such things as, "You are playing with problems that are too applied or too uncontrolled," and so on. We almost walked alone when we initially started to study the world-of-work performances of scientists, of physicians, of medical faculty, and of nurses. Almost everyone told us that it couldn't be done, but we tried anyway. One of the deficiencies of our profession is this overstrong tendency to continue to work only back in the old areas and not being very willing or able to move into new untouched areas. The notion that the field is ready to be plowed is a good notion. There are lots of old fields now being plowed, but some of the potentially most fertile ones are oftimes the ones that are not being touched at all.

We do have, I think, a lot of techniques that might be effective in helping us delve into these new fields. I sense in all of Dalkey's cases, that we could obtain multiple scores on the performances of each of these people on an individual basis. Most of the few tests in the field of planning so far are largely restricted to paper-and-pencil (schoollike) tests. From our research experiences with situational and other tests, we could also set up situations of various kinds of planning activities from which we could obtain multiple scores as an effective means to analyze the behaviors of experts in practice. In our recent study of Peace Corps abilities we built some live situational tests and found they called for a lot of abilities (or talents) that paper-and-pencil tests could not or at least did not get at. These abilities tend to be the live complex, world-of-work situations and activities because they do not overlap much with the academic world where most of the psychological research work on talents has been done so far.

In our work on scientists, self-ratings on creativity were very good, compared to other scores, and we could get them almost instantaneously. Our hunch from this is that self-ratings are very promising, but with one most important condition: people have to be given many opportunities to experience their talents on which we eventually want their self ratings. For example, if the students who started in my hypothetical nursery school were to practice their planning talents and to continue such practicing throughout their school and work careers, by the time they got to where they were contenders for vice presidents in charge of planning or vice presidents in any other kind of activity, these people would be able to give you quite valid self-ratings on their planning talents.

I also sensed, in Dalkey's studies, that irrelevancy and noise in the discussions were coming out from the participants. I would bet on tremendous individual differences in how much people contribute to the irrelevancy and noise because some have a strong screening process before they speak, while others are almost nonstop talkers and at times have no barriers on what flows out. (There are even suspicions that some people talk maybe even faster than they think, although the reverse is most usual.) To the degree that this irrelevancy is going on, the ability to select relevant from irrelevant might be one of the important things in the group planning activity and therefore one of the things in the profile of the high-level talented people upon which we should focus.

Rethinking is also interesting when it occurs because revision ability in our communication studies was the best single device we found for predicting performance in world-of-work communication situations. You can nearly always rework your communications because they are usually somewhat ambiguous the first time. If you don't believe it, start expressing yourself by writing a long program to a computer and see if you can ever get it completely clear the first time without needing any debugging. So this rethinking is one instance in which we are asking for variability within a person. Variability is likewise needed in flexibilities, including the ability to get new information and adjust oneself and have a change in reactions and plans and forecasts in light of the new information. Just plain scrambling is another form of variability in performance that may appear to be unplanned and disorganized, but as in the case of quarterbacks it can at times eventually prove to be quite effective.

A potential complication is that some people have pretty well convinced themselves that they don't want to play the revision or rethinking approach because when they take a test their first overall impression is often better than their rethinking impression. When they change their answers, they say they run into trouble. We should recognize this possibility, too, in planning and forecasting. If a person already had a first overall impression before he got into the group discussions, sometimes his initial impression had more validity than one he might change to after he looked in detail at all the relevancies and irrelevancies and then mixed them all up together as the basis for his change.

Pressures in a group may be worth studying to see who produçes the group pressures, who gives in to the group pressures, and how these pressures are related to the final actions of the group. I do not know what would happen in the area of planning but in the area of creativity, the people who do not give into the group pressures, and who are willing to tell the truth and report it as they see it, independently if necessary of the pressures, are the most creative. So I suspect that two or three creatives in the group might move farther away and diverge from the consensus rather than converge when group pressures emerge. I am wondering whether maybe some of our most expert planners, those highly gifted in planning, might also show some of these rugged individualistic tendencies when working with groups in the area of their own greatest talent. I doubt if anyone knows; it would be interesting to find out.

In our communication ability studies, we felt that much of group activity could be accounted for on an individual basis—not all but much and probably the majority of the group phenomena. If we knew each person's individual profile of communication talents, we felt we could account for a lot of what would happen when groups of such people got together because of the individual characteristics of each in terms of their communication talents. I would suspect that we might also find some of that in the area of planning: That much of the group behavior could be accounted for if we knew each individual's profile of the planning and other relevant or irrelevant talents elicited in the groups. Then there would be some other distinctly group factors above and beyond the individual (non-group) ones that would remain to be explained, but I suspect these remaining aspects would usually entail only a minor part of the total phenomenon.

Since all of you are pioneering this new area, may I comment about problems you may encounter based upon experiences I have had in trying to help pioneer in the field of creativity. One idea came to me when I was working in the National Research Council, whose full-time business is research counciling. I sensed that there was an in-group for each specific science area who wielded an influence as well as some control on the progress in their area. I wondered what the chances were of a new person ever getting invited into the in-group council meeting and if he did get invited, what chance would he have to contribute to the short report of their meeting that went to the outer circle. Furthermore, if he were invited but proved to be a young upstart who made some comment that didn't fit the ideas of the leaders, those who currently owned the field, so to speak, what chance would he have of getting in his idea and having it received by the inner circle and also included in the writeup sent to the outer circle. On these points I think you have some protections in this series of two conferences: First, you are rotating members and speakers to some degree; secondly, you are recording the discussions, so that they are published instead of screened out by the inner circle; and, thirdly, you are using cross fields persons, such as those present, who will provide a further protection against the type of errors and distortions described above. At least, this has been my experience.

In turning to the planning processes within a person, including the input processes, the central processes, and the output processes, the cycle can be repeated in terms of checking up on your plans and your forecasts and revising them. In other words, you can repeat the input, central, and output processes in doing planning work. I suspect that the input can be mighty important. The input could include the whole past life history and experiences of a person and all the immediate things that are coming in that do or could bear on what he is now doing. I suspect this total input, both past and present, might be much more important than we have given it credit for. The total reservoir of inner resources of a person would, at least potentially, be available to draw upon if a person knew how to do so and had the central processes working well to use his large inner library of resources. The central processes, I suspect, can be very complex and the studies that have been done on planning indicate that the more complex the planning activities, the more complex the set of talents that are needed to work in the activity. Since some of our tests are very simplified tests, we would have to use a collection of these tests to get at all aspects of these central processes. The output (expressional) processes, too, can be full of interesting problems in each area of human activity, including planning and forecasting.

Let us examine, for a moment, the input processes together with the flexibility of the person engaged in planning. Can a person continue to listen and receive new information as well as he could when he first started to plan after he has progressed some distance toward making a plan or even after he has crystallized a plan?

When a person and/or a group has completed a plan, to what degree have they fully and appropriately used the available inputs. Are the facts, ideas, suggestions, and even something so far out as suggested plans of others fully listened to and utilized? One of the human tendencies, I fear, is that whenever someone comes up and makes a suggestion to a person in a crucial position in the organization who is responsible for planning, that person in the status position may make a human reaction such as: "That's my business, not yours, to make the plan. It's none of your business." If the suggester goes too far by using his own talents to form his suggestions into a suggested plan and communicates only his suggested plan, the official planner, of course, may feel threatened or even feel the suggester is trying to sharpshoot him out of his position. So this kind of complexity can occur in the input processes.

Apparently one of the most difficult things for any supervisor to listen to—and it is hard for a supervisor to listen—is to listen to a subordinate, especially if this subordinate has a highly creative new idea. Now, maybe it is equally difficult for a supervisor to listen to an extremely high quality plan from below. Maybe one of the problems is that some of the best ideas, best information, best suggestions, and maybe the best plans may occur in the organization but in the wrong minds in the organization—in the wrong places in the organization chart, perhaps at the grassroots. How to get the suggested information, ideas, and plans from there through a set of human communicators to the right place in the organization and have them be transmitted without erroneous distortion and be accepted effectively at all stages, including the last one where the responsibility resides, will be found to be one of our major problems, I believe, in this planning area.

Other questions are: Who is included in the plan and what input is included in the plan? And who is excluded from the plan and what is excluded from the plan? The organization may have a screening process here in which it is screening out those with the higher level planning talents or screening out or partially distorting the better ideas and better facts. Thus, some of the best potential information and talent resources in the organization may be organized out during the planning processes.

Let me illustrate these points in terms of our new field called architectural psychology, in which the architects design environments and the psychologists study the reaction of people to these environments. As the initial psychologist on this team, I have realized that a lot of the activities of architects fall into the field of designing and planning. Our architectural psychologists, including our graduate students, have made some plans about hospitals and we have run into the above problems. We go to multiple sources for information that might be pertinent to the future plan. As we worked in hospitals, we have found at times that it was difficult to get much usable information from physicians. Our work with psychiatrists uncovered that they did not easily become very explicit in terms of the architectural problems and secondly, they showed us almost anything but agreement among themselves. The further down in the organization that we went for information, almost the better the information we were getting from these usually least expected sources. Some of our best information quite frequently came from the lowest people in the hospital organization, from nurses' aides and from maintenance people. They were the ones who gave us considerable information, but we suspect that some architects won't listen to these people or perhaps really the architects do not get a chance to listen to them. Usually they only work with their client and their client is up at the top and perhaps the client does not prove to be a good representative for all the other persons in the organization and does not listen to them or even realize he should listen to them so he can represent them well and make their contributions available to the architect.

Some of these problems may stem from the fact that architects often do not have a "live" client in their training, but only their teacher. So their tendency, when they get into practice, is to practice like they were trained to do, except to do without their teacher. Therefore, an occasional client may, at times, almost have to fight his way into the architect's creative and planning processes to make his points (and I am exaggerating a little again to make my point).

The planner also has the problem of understanding the information he is getting, so there is a communication process here and also one of integrating it after he gets all of it. In architecture again, one of the things that comes out of the communication process is a set of drawings or blueprints, which represent a plan. We have just realized it is not only a plan—it is a communication device, too, between a second and third person in the chain. The architect produces this plan for the sake of the third person in the chain, namely, the production or construction people. After the client has communicated in terms of words with the architect, a problem arises if the client cannot understand the set of drawings which are the plan. The architect has produced a visual plan for the production people, but the client may often have a problem of comprehending the complex spatial drawings.

To handle some of these communication problems in design, a project on our campus is planning how to use a computer in graphic outputs in architectural design. The computer can potentially handle a lot of input information, too, and can double check things that are happening so that it can do a lot of the structural engineering and other technical work. Then the architect will be free to spend a higher percentage of his time in creative planning and designing and communicating at all stages with the client. Potentially he can plan and design a higher percentage of buildings and the man-made space that men will occupy in the future. Thus, the computer can also be a communication device to clarify the plan to the client and can give feedback to the client, to include him in more throughout all stages in the planning so he can ask for modifications where necessary before the plan comes out in its final form of solid cement walls.

In terms of city planning, someone recently said that "cities are people, and not just buildings." Again this means that someone in the past has not given enough of their thoughts to the people, or has not thought enough of the people, so something has been neglected or overlooked in the planning as far as the people are concerned. Yesterday one of my students said, "When we plan for the heart of our cities, which might become the slum and crime delinquency areas, we must consider what the place will be like at night and how many dark alleys we have produced both inside and around apartment buildings as well as outside in other places." This aspect of planning could have easily been overlooked in the past, leading to plans which have actually facilitated crimes.

There is a plan for cities of the future called the Model Cities Plan. The designers have been very proud and pleased with this plan, except that during the riots last summer, a statement was made by some of the riot leaders that they dismissed this plan as worthless. In effect, they said that it was a white man's plan instead of a black man's plan (even though the planners argue that Negroes have had some representation). They want to own their own communities, namely, much of the heart of the cities. When these slum places are renovated, they also want to be involved in the renovation process by being crucial parts of the construction team, and part of those who are making a profit, rather than having a white man's construction team come in and largely exclude them from the work force. They may also have the feeling that they have been included out in the planning, in spite of the fact that they are the ones who are going to be the tenants. People who move into buildings as tenants are often probably not even selected as tenants when the buildings were being planned. So we have a real planning problem there.

Another kind of problem which is happening in the city, is a grassroots movement, which can have a lot of healthiness to it. The people in the slum area want to renovate their own area for themselves instead of being wiped out and having to relocate elsewhere while some new buildings go up and some other groups of people move into this newly built area. One aspect is that the city fathers who have been successful in handling all these previous community problems may now be planning what they are going to do next with the area. But at the same time other plans are emerging at the grassroots level and the question arises: What is going to happen to these two competing plans and are they ever going to become synchronized together somehow to become only one plan?

In our work for the Peace Corps, we have tried to learn how to select Peace Corps volunteers who, when they will go overseas, will not be the highly talented and display their great talents overseas to become the great performers themselves. Instead we wanted to select and train them to be subtle catalysts so that the host country people develop their own talents and thereby become the great performers. We wanted our Peace Corps volunteers to spark the people overseas to become creative themselves, to learn how to sense problems, how to create plans themselves, and to follow through and implement those plans. This may be an interesting challenge in city planning and broad planning of the future: how can we go in and be the sparkplugs to help the other people make their own plans instead of doing it ourselves and then imposing the plans on them?

In the magazine industry, the cardinal sin is to follow the plan right out the window. I heard this when I was on a 14th floor where it had real meaning to me. So sometimes there has to be some continuous checkup with potential revisions in the plan. In research there are times when it is wise to have a good tight research plan and other times when it is not wise to be so locked in. Sometimes in power struggles a way that some people get control over other people is to build a plan with the ulterior motive of control and then try to hold the other people to it. But the others may not want to slavishly follow their plan.

The issue in question is the tentativeness in planning and the capability of people to be tentative in their planning or in their following of a plan. This may be highly important for leaders to be capable of producing only a tentative plan and really function so it is a tentative instead of a firmly crystallized plan. Quite obviously, any needed revision that may arise is more likely to be welcomed if the plan is widely considered to be tentative. In contrast, it may be a very difficult thing to uncrystallize a plan that is highly crystallized.

Recent research on human talents mentions divergent and convergent talents. The divergent type involves various boiling up and expanding processes whereas convergent talent entails boiling down and reduction processes, focusing and zeroing in and converging toward a solution. A strong argument can be made that the best planning may be divergent rather than convergent. Or maybe the best planning alternates as follows: converges, diverges, converges, diverges, converges, diverges. The need is to find persons who are flexible in their planning, who can alternately converge and diverge, and open up for further information by uncrystallizing the plan to some degree and then converge again. In this way, the planners are always alert and can modify the plan, and thus have some degree of tentativeness, but will also have a plan ready that they can follow, if necessary, at any given time.

Human beings are often not very good at this kind of flexibility. I used this technique on persons in an organization that was highly crystallized in its plans, as the best approach to use to deal with them. I would say, "OK, we have completed a plan today that we can use and can have ready for use, but now let's reopen it and rethink it again." It was difficult for these people, the powers that be, to allow us to do this. Yet it was the best chance I had to get in other information that had not yet been included in their planning. After we did this several times, opened it and then crystallized it again and again, we then put on the show. But we changed our plan only 30 minutes before we started. It just about scared the wits out of those in power because they said that we still hadn't figured out two or three more details of the plan. They asked how did we dare to open it up and change it at this late stage when we hadn't even quite finished making the plan. But we never did give the participants an agenda; they never asked for one; and we were keyed up creating the new plan as the show kept rolling. We didn't schedule our coffeebreaks or anything else but let them happen when it was timely to do so, we just kept everything rolling and the people enjoyed it very much. Admittedly, a couple of people who were helping us run it were under some strain; they came up to us, quite upset, because we weren't following specific enough plans. Later one friendly critic said, "Gee, you look so unplanned and relaxed. Tell us how long did you have to plan to get that way?"

Let me go a little into the problems of selection. As indicated earlier, we would probably use a biographical inventory as our best single basis for selecting those high on either planning or long-range forecasting. By learning to read appropriate parts of the life program of a person, we already know how to forecast the future creativity of a person. If in the past a person has largely kept his creating processes dormant and has not used them, then he is not a good prospect next year to be a creative person, whereas another person who has had his creative processes working and has been in climates and situational experiences where year after year these processes are functioning, then we can forecast that during the next 5 years he is going to be a good prospect to be creative. We should likewise work very hard to build an inventory for measuring planning talents (and another one for forecasting talents) along this same kind of line. First, we would generate a thousand multiple choice items and try to find 150 of the best ones that we could find that display a life history leading to a high degree of effective planning talents. We could also select among those experienced in planning to try to get some measure of the batting average of those experienced in each of these activities. Then we would find the biographical items that select the better planners from the rest and then repeat the procedure to find the better forecasters from the rest.

Let me point out some highlights in the work of Guilford and his staff (1964–65) at the University of Southern California in a study of planning abilities. They did a first general study of basic planning talents, so let me give you some titles of the sets of characteristics they were studying. One was orientation, including sensitivity to order, discovery of conceptual relations, and recognition of variables. A second was elaboration including specifying details, producing ideas and alternate methods, and symbolizing ideas. The third was ordering, arranging, and integrating including both temporal and hierarchial ordering. The fourth was originality and ingenuity in new methods and applications and adaptive flexibility. The fifth included an ability to put all this together into a prediction where you have to use such talents as visualizing things you can't actually see, or having foresight through ideas and through things you can perceive, and extrapolating from the knowns to the knowns. And finally, things like judging and evaluating (a high degree of which would be wisdom), including seeing shortcomings in plans, and judging the importance of variables. In summary, there are a lot of talents and processes and activities in planning, as best understood so far, and the beginnings of measurement exist in these above areas.

One consulting group is working vigorously in the field of planning. They feel that if they would put together everything now known about planning, it would make a major book. Through such a book they feel they could get planning training into all management departments in schools of business across the Nation in about 3 years elapsed time. But is someone doesn't actively take such steps, they feel it will take about 30 years to get what is now known into the system because of the slowness of the bridging process now going on. We have an organization with a group of students and former students who are ready not only to work in creativity and to handle the snowballing of activity there, but also to move into these other areas of high level talents as they really occur in the world of work.

Let us think briefly about the combination of planning, creativity, and flexibility in planning. I have indicated certain problems of communication which I think are very important: The ability to listen to new information and new ideas from others, and also to listen to yourself, to find value and use for that which you already have within yourself, so it is not only listening and receiving from outside but also receiving from inside. Some of the great creative people are the ones who so fully use the stuff, the inner resources already within themselves. Perhaps the same thing is true of great planners and of great forecasters. They must know how to use their own full background of experience as well as the immediate surrounding information that is available to them.

Let me tell you a sad finding in creativity, which may also be true in planning. If you try in turn to stand in the shoes of the two types of people who are involved, the person with the idea and the other person, often with the higher status, you may get a better feel of the phenomenon. The finding goes like this: the more highly creative an idea a person has at any time in his life, the more likely he is to be in trouble. Both he and his creative idea will probably be in trouble with other people and with the system or establishment and its features.

A new creative idea is likely to run into an old plan and usually someone else's plan who probably is higher in the organization. Likewise, a new idea will run into an old system built by someone else who usually is in greater power than the suggester of the new idea.

Maybe along the same vein, the more unexpected though high-quality plan a person has at any time in his life (if he isn't sitting as vice president in charge of planning) and the more unexpected it is and the less it resembles the present scheme of things, and the more likely he and his plan are to be in trouble with other people and with the established organization. The people and the system will not separate him from his plan but will tend to clobber them both together.

Results and observations like the above ones strongly suggest that organizations are often at their best on matters that are quite routine and of minor importance. In sharp contrast, however, organizations are often at their absolute worst on matters of highest importance, such as (1) when a quick-reaction capability is sorely needed in responding to unexpected but marvelous opportunities; (2) when the organizations are asked to respond properly to creative individuals and to the creative processes and creative ideas from within themselves; and (3) when they should become flexible and adjust themselves away from their old plans toward new plans and in directions indicated by brilliant new ideas.

I have given you a little feel of flexibility in planning. Some organizations have had an opportunity knock on the door and their individuals could hear that opportunity and knew it was there, but the organization as a whole officially would not listen because, for example, they were already planned and programed for the next 2 years. Instead, they could start thinking about putting that opportunity in the program 3 or 4 years from now, but they could not think of adjusting the present program right now in terms of that opportunity. They had what I call a slow reaction capability of at least 2 or 3 years lag time. I am sure some of you have encountered organizations that say, "Let the present plan run its course before we consider new ideas."

There is at least one instance when for 450 years an organization had shown an incapability of listening before it finally became capable of listening—but that lag time is better than infinity. Those currently in the organization would not live long enough to see their ideas or plans accepted with a 450-year lag time for the organization to listen to what might be valuable for it. You will find individual differences in persons and in organizations on listening ability and readiness and receptiveness to new ideas. Two of the slogans you may have heard are: "Don't confuse me with further information. My plan is already made up." The other one is, "If you don't understand it, oppose it." The new information or new ideas may be difficult to understand because of fixed ideas about the old plans and they may mean a new plan or replanning would be required which might be resisted as not being worth the trouble.

We have wondered how to get versatility and flexibility and a variety of plans and quick-reaction capability into organizations so that instead of having only one plan, the organization may have multiple plans. In the military, the cardinal sin is to have one and only one plan, without an alternate plan. How can you develop flexible plans so that they will still work in terms of the whole organization? This is a very challenging problem.

And how do organizations deal with new ideas and new plans that emerge from below in terms of uncrystallizing plans or of having flexible or tentative plans? How can one get those in positions of control to be more flexible concerning plans? They may all need a high-level profile across talents, but we often have to work with the people who are now in the positions, no matter what their profile of talents looks like.

Let me give an example, which is both seasonal and appropriate. In professional football they really have many, many plans for their entire organization (or team) to follow. They have many, many plans and every player is well versed in each and every one of these plans and knows how to carry each one out to form a well-coordinated team. Instead of a single plan, they have many, many plans, and they also have quarterbacks who have a sequence or strategy of plans. They have a game plan which is a whole series or pattern of plays, including things to carry out which are supposed to open up the other team. Then the other thing I like about the smooth functioning professional football team is that they have quick reaction capability. They aren't too tightly planned, though, and if the play is broken up, they still somehow scramble and try to make a good play out of it. Also, when they are in a huddle, the quarterback is open for input from other players. If one of the players says, "I see something unusual," they may even create a new play in the huddle. And at the last moment when the quarterback comes up to the line of scrimmage, he is still open for input from the other team's defensive arrangement. And the quarterback can and does often change a play at the line of scrimmage. So you see they are well planned, but they also have great flexibility in their planning so that they can try to avoid getting into trouble. Those who don't have this flexibility in either offense or defense are vulnerable to being put into trouble by a more flexible and creative though also well organized and planned opponent.

This brings up the point that the more we can learn how to release the creative potentials in people so that there is more creativity in action, the more exciting and unpredictable the future can be. Creativity when effectively funtioning can change capabilities and feasibilities. So creativity can thereby upset the long-range forecasts and may also call for a change in the plans, if only to keep in adjustment with the creative strides.

I would like to close with a somewhat subtle story, which I hope you will enjoy. A young fellow had done well in the academic world in meteorology and felt he had a promising future in weather forecasting. So he got his first assignment in a place like New Mexico where one can nearly always forecast that the sun is going to shine. He did quite well in his first job and was really very happy in his work and was moving along successfully. The Weather Bureau also liked his work, so when they had an opening in New England, they decided to select him to fill that spot. They believed that if they could get the right man in the right place, their problems would be largely over—but if they got the wrong man in the right place, their problems would just begin. So they selected him and sent him to New England. When he got up there, he found that he had a different world in which to work. The weather was much more complex and changeable and unpredictable, so that "if you didn't like the weather, you should just wait a minute." He worked hard in this new setting and made his forecasts, but after a while he didn't have anywhere near the batting average he had previously had in this New Mexico sunshine State.

He struggled with this problem and tried to perfect his techniques, but could only improve them a very slight amount under this new and difficult, many-variabled situation. After a while, some of his neighbors started kidding him and other people stopped him on the street and would tease him, and then the newspaper picked it up and started making alternate forecasts from someone's bunion, and so on. Gradually, all of this started to trouble him greatly. Finally, he wasn't thick skinned enough to handle it all, so it started to really get to him. He wrote to the Weather Bureau in Washington, D.C., and said, "I would like to transfer elsewhere." The Weather Bureau, after having felt that they had the right man in the right place and could stop worrying for a long time, were caught by surprise. They immediately sent a telegram to him stating, "We don't understand. Please explain." He replied with the simple answer that "The weather doesn't agree with me."

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Mr. Cetron. There is an old saying, "in the country of the blind, the one-eyed man is king." I don't really believe it. I think they'll poke his eye out. This is a problem with any man who is ahead of his time, especially in large organizations. If we take one of these creative people, that we trained from kindergarten, he'll get to the manager, who will say "Fine," pat him on the head and forget it. We try to force everybody into the same mold and it's particularly true in the military.

Dr. Taylor. I recall a statement which says, "Divide and conquer." As long as we keep our research going where we deal with one position at a time, we can get cooperation. If we have to deal with the whole organization, we will have problems. You might call it informal versus formal organizations, and we are trying to work through the informal. This is very central in my thinking. Our next book is "Climate for Creativity," and my chapter is: "Can organizations be creative, too?" If individuals in organizations can, can organizations as a whole? There is a whole series of characteristics you seek in organizations. Can we get organizations functioning at this high level? That's what I'm talking about: The whole team having quick reaction capability, being able to adjust as a team to the latest information. In the competitive game of pro football, we have reached that and in the competitive game of organization, it must be functioning this way. One organization with which I met said, "The things we are doing this year, we were not doing last year." So they changed almost a hundred percent. Some organizations do have very fast reaction capabilities in planning and creating. They are creating the world of the future. It is others that aren't adjusting who will be on high and dry land.

As far as the, "can't doers" and "it's impossible," in our Peace Corps approach we built situational tests where this could be a dodge and if a person took the dodge out of the situational test, the sooner he flunked out of the Peace Corps, as far as our proposed testing was concerned. So we've played this game. There are many people sitting in key positions who know the reason why it can't be done and are making no contribution except to kill things. Other people who have the reputation as can-doers—no matter what you take to them—will find a way to help you. Like the mayor of Fairbanks said, "We are a can-do community and we're going to find ways to plan and carry this out."

Dr. Linstone. I agree with Mr. Cetron only there's one thing you mentioned, tentative planning. It is very useful and I think it helps in a great number of situations. The thought of the planner is that he doesn't consider or propose to the decisionmaker how to get from here to there in small steps. He goes there in one step. This means a great risk on the part of the decisionmaker. It helps very often, and I found more receptivity, when you indicate not just a plan but also you might say the off plans, where, if you are wrong, he hasn't committed himself too much. In other words, there are additional points on the way. You don't give one single best way to get from here to there but a good way, not as good as the best way, which permits alternatives. This is nothing more than dynamic programing in which the decision is made on the information available then. You will find he is much less opposed and is willing to take this risk because he sees he doesn't have to commit himself for 15 years, and a tremendous amount of money, which is obviously important. This tentative planning is very good.

Dr. Taylor. I have two comments. First, I have been working with a composer and he says, "A good composer composes just the right amount; not too much, not too little. Too little, he doesn't give enough of a pattern and, too much, he doesn't give the person who is going to carry out the plan a chance for his own individual imagination to make it really and truly artistic, to make a creative performance."

The second, we had a dissertation in the field of mathematics, in creativity. It's a fascinating one. The person himself chose to be two different kind of teachers: the typical kind of teacher in the morning, the learners' teacher and select story problems in the area of work. In the afternoon, he was going to be a teacher where the students were thinkers. What happened? In the morning the students learned the typical way. The teacher told them the solution to the problem, the one and only way to get to the solution. There was one and only one route and that was all they were supposed to work—the teacher's way, the textbook way, and so on.

In the afternoon the teacher had to play a different role. The students generated all the routes they could go to Rome and learned each other's routes and so on. In the end what happened was: the students in the afternoon didn't work on nearly as many problems as the students in the morning. But when tested on an exam guess who could work the story problems better? The students in the afternoon, who enjoyed working story problems, who had to listen and learn about these different solutions. The teacher had to listen to the students to see if their solutions were feasible and to think with them, where before he had pulled out the textbook answer and said, "Yours doesn't fit this so it's wrong," where it might have been an alternate route. When the students were asked: where did we spend too much time per problem, which group of students answered this? The ones in the morning said they spent too much time on a problem. The afternoon students actually spent more time on a problem than the morning students but did not make this complaint because they enjoyed studying the routes to Rome and were interested in finding a solution more than the morning students. The afternoon students didn't care how they got the solution, just so they got one; where the morning students were more interested in the routes to Rome than the final solution. We are products of this kind of training. If we were trained in that afternoon class, we would all probably have multiple routes in mind, if we got this far, maybe we could go over to another route and so on. I think we'd have a much greater flexibility in ourselves if we would have been trained this way.

Dr. DALKEY. Let me elaborate. Where an individual is most likely to have highly creative ideas, he is also most likely to be in trouble. What kind of trouble were you referring to and were you suggesting that being in trouble is part of the environment that is favorable to creativity or the reverse?

Dr. TAYLOR. In my latest writing, it says it is hard for this poor guy to read the symbols—the signals coming back to him. Being highly "clobbered" may be the highest form of recognition some people can get. There are about eight or 10 research studies that add up to this picture and one says, if you get youngsters together, quite small youngsters comparatively, and have them think up new ideas in a group, and one is starting to outstrip the others too far, the group will tend to say—these are group pressures—we've got to get organized; we aren't organized. One of the kinds of thing they will do, will say: we've got to write some rules. When they write the rules, they don't write them to facilitate progress but to constrain people and the one they are trying to constrain is this fellow who is thinking too fast and far and, I think, unexpectedly for them. Many times rules are written into organizations by people who are more interested in controlling progress, maybe trying to control creative people. The first thing plan suggestors do is say, "We've got to have various members of the group," so they try to organize again and get a chairman, secretary and a recorder and things of this type. Guess what they do with the fellow who is outstripping them? Make him the recorder so this takes care of him since he is busy writing their ideas down it gets him out of the ball game. They've organized him in, unless he writes a minority report and doesn't record it. The other tendency is if they can't organize him in, they organize him out, and use group pressure which I hinted at earlier. The highly creative people who want to be leaders also want to be surrounded by a group. The whole world may follow them later when the world finally makes enough progress to get up that way, so, if you bring in a new idea that barely inches ahead and no one has to do too much to adjust to it, it is easily communicated because there is just so much expected and it's just a wee bit new. But, if you are bringing a new idea that is highly unexpected and is going to cause trouble and work, many people in key positions won't go that route. You go to all this work because you have an idea and they are in a lower status and these are the kind of problems one runs into. He is all alone, in the first place, he and his idea, against the world. Whether he can get other people to join this idea is one of the problems and it is sad but the higher the percentage of people against you may be an indication of the degree of unexpectedness and potentially the degree of creativeness of new ideas.

Dr. DALKEY. There is a big difference between having a talent and employing it. I was wondering if you had reflected at all on the long series of studies which are gradually seeping into public view with regard to the relationship between child order and eminence? The studies, for example, demonstrated in "Who's Who", the probability that the first child is successful much higher than it is for total population, and that this is a way of increasing the function of the order of the childbirth pattern.

Dr. TAYLOR. Incidentally, my latest paper is called "Leadership toward Creative Organization and Functioning." It deals with trying to get a leader to function so it is the creative function of a whole organization. We have done more biographical studies of scientists and creative people by far, and have gone through the computer about a hundred times on a hundred different criteria of performance with 20 or more samples, 15 or more samples. I don't think we have found this as strong as you indicate about order and eminence. One of our problems is we cannot publish this or we spoil the scoring key. We have people write themselves up in the form of an application blank. Then we go back in life history to see what kind of experiences lead to being a creative person, but I don't think we found it as strong. In fact, I can say with confidence, no single item by itself will give you very strong predictions. We have to use 150 or more collectively to give a good solid report.

Mr. IRWIN. I was interested in the first point Dr. Dalkey made—the question of whether the creative person is beaten down by the group. All of Dr. Taylor's responses were very much predicated, it seems to me, on our own society. What happens in American schools today or yesterday or even 20 years ago, when a person or a child shows creativity? I personally want to speak vigorously against the idea that it is a good thing to have this creative person jumped out of the mass in response to the pressure. This talent is so great that he achieves his creativity anyway. It's true; it happens. But I want to refer, as my authority, to Eric Hoffer whose ideas affect me a great deal and I offer it for discussion: Hoffer's thesis is that everybody is creative and if you and society encourages the creativity, the mass creativity that comes from this is enormously greater and he cites the type of painting that came from Florence, out of a very small numerical group of people, who produced a tremendous amount. He cites the Elizabethan period with their tremendous flowering in poetry and music and I am very much opposed to the idea that it is good for creativity to make him jump out as a group.

Dr. TAYLOR. You are saying that some people say creativity will rear its head no matter what and will fight its way through. This past summer we had Dr. Toynbee on our campus, to give two lectures. We are now in the process of getting out 19,000 copies of the two lectures in creativity. He defined creativity as that talent which when turned into activity in enough people in society, will create history in any field of human activity they are engaged in, and asks that more Americans engage in this because they are at present neglecting its future history. He says creative talent is the ultimate capital asset of mankind of any society and it is a matter of life or death for any society. He said if America is to have a manifest interest to spark the entire world, then we must use all the creative potential that we have. All the studies of creativity indicate wide individual differences in the amount of potential or actual creative talent that we would have. It doesn't say anyone is zero. Most of the contributions and studies say the responsibility is on other people's shoulders, a whole flock of other people's shoulders. So if you can get the whole shoulders raised, you don't have as far a hike to go, to go higher. So our attempts to get creativity in the classroom are consistent with what you are saying. Some will rear their head no matter what and others won't. We want this to rise to higher levels so we want other talents to go with it. In American schools evidence shows there is a fourth grade slump in thinking abilities but there isn't a slump in height and weight and so on. When this was tested in other cultures, a slump was not indicated, however, Americans go around the world and try to be of help in other cultures. One place in Samoa, they tried to help the fourth grade. What did the Americans help the fourth grade to get? A slump in their thinking abilities.

Mr. IRWIN. This is one proof of the point I was making. I have a boy, 5, in kindergarten. Everybody draws a picture and they are all enormously creative, very original. There are no restrictions and all their paintings are exciting. Then they run into the attitude of society. He is just emerging and hardly knows he is a person. As soon as he does then he finds there is this restriction. It has got to be removed. There will be an enormous increase.

Dr. TAYLOR. Again this is my point. Norman said there is a strange thing in history, a fraction of society becomes effective in terms of creativeness and rise to help make that part of the world rise to a place in history. The interesting thing is, in the course of history, that same society starts turning its back on the very talents that had it rise to its place in history. They even try to neutralize these talents when they start to emerge. That's the alkali on the acid when it starts to sparkle, so he is saying this is what the natural tendency of history is: Why some people have their names in history—and you are saying that America is showing too much of this and cannot reverse itself. We have had our day in history.

Dr. MANZ. I think I can reconcile between the two points if you modify your statement a little, by saying: I know many great mathematicians and one in particular, Felix Linewall, has said, on several occasions that mathematics could be understandable and could be an intellectual pleasure to everyone but it isn't, because so many teachers are failures. They do not have the ability to bring this pleasure to the student. I realize this in my own son. He hates mathematics and physics and I cannot help it—he is appalled by it—because of the miserable way mathematics and physics are taught in high school. An even worse situation is facing us today. There have been several articles written that the present teaching of mathematics at universities indulges in formalism, ugly emptiness, and therefore does not really appeal to any creative mind. We have an interesting phenomeon, many of our most intelligent people turn their backs on science and go into other areas. I do not mean to degrade, they are important, but we do have a degradation of our scientific education. Creativity—when you make the statement "everybody has creativity," this I would modify. I would say creativity is something you either have or you do not have. Therefore, I am not sure about that statement, but if you say "talent," I absolutely agree. Everybody has talent unless he is a complete idiot and if it so happens that a student dislikes a certain field at school or university then it is the fault of the teacher. As I said before, mathematics is something that has so much beauty and intellectual appeal that it must appeal to everybody and if it isn't so, then something is wrong with the system of education.

Dr. TAYLOR. My previous statement was a search for a creative climate. Every study we made indicates what we have in the way of a climate for creativity is not ideal. It is not anywhere near. Changes are necessary. In our proposal of education, we will be developing the talents of the various students as they go through acquiring subject matters, we are trying to have these students experience different talent processes while they are working with the subject matter. They are using this infiltration test during the subject matter, trying to get the youth to use these new talent processes. The students are finding the schools much more interesting since they get a chance to use this variety of talents. The students are ready—it's the system that isn's ready. The students are ready for the opportunity.

Dr. SLAFKOSKY. I think in a sense you were misunderstood. I didn't think you were saying that it's a matter of fact creativity had to spring from a time or a period of held-down influence. It often has in the past and it certainly is frequently the case now. An answer to the question about the function of American culture is, If you look at the scientific areas or philosophic areas throughout the centuries, Socrates wasn't recognized—he had to take the poison. Galileo wasn't. Neither was Copernicus. Their contemporaries ridiculed them. This is the function not only of our society but the whole western tradition. Granted we want to eliminate or ameliorate it, but I do think the idea you were propounding was mainly: Let's make it easy for the creativity, first, to be recognized and for it to flourish. You didn't necessarily say it had to be done in adversity. Do I understand you properly?

Mr. IRWIN. I didn't take this as being directed at me. I thought he was talking to people who had expressed the idea in the field of creativity because I did not express the idea he was commenting on. I dedicate my career to the opposite. I wasn't aiming at anybody. Dr. Dalkey's question struck me as being fundamental. I would add, however, that many of these outstanding people who ran into adversity, arose from a culture which encouraged the kind of activity they were doing. In Athens everybody discussed philosophy.

Dr. TAYLOR. We almost used as one of the possible clues to creativity: how much trouble the idea cost; and the other is that we are finding we do not have, at least on a broad scale, highly favorable comment on creativity in many of our organizations, including schools. However, we are trying to do all we can in our studies and enlighten more people. I will be speaking in Colorado tomorrow and Oregon the next day to illustrate my attempts to awaken and open the thing so more people who have talent can come from the grassroots when they are ready. The youngsters are ready and are delighted when you try something new.

Colonel VAUGHN. I am interested, Professor Taylor, in whether you have any information or knowledge of what other nations and cultures are doing or have done rather recently, keeping in mind, of course, have the Russians a program for selection of experts and what is it and how well is it working?

Dr. TAYLOR. Let's go into the area of creativity not into planning. We undoubtedly have by far the most research and most research knowledge of insight of creativity on the globe. I am writing a paper to go to Holland next summer as a position paper on creativity. I was chosen as the one in the world to write it and have had correspondence with many of these places and find that we are way ahead. That doesn't mean we are way ahead in practice, the kind of things he is talking about necessarily, the culture that is most favorable, but at least in the research area, we are way ahead. I don't know of anyone that is doing much in other high level talent areas other than us. I think we are probably way ahead there, too. I have pondered a little on creativity when I received correspondence from the right places: Warsaw, Moscow, and such, and way before some of our nations awakened back in 1955, 1957, and 1959 whether we send everything to them for fear they might implement it faster than we did. Our thoughts in creativity now is to shoot the works—send them anything we have. It will be one of the hottest things they ever had in their hands.

Mr. FRIEDMAN. If I can attempt to translate what you were talking about in ancient Greece when people were talking, philosophizing, from this some people emerged who were not primarily accepted. Let me turn that to today in modern times and point to the beatniks, the hippies, who, I think represent perhaps a throwback in modern times to those ancient Greeks who sat around philosophizing. How does our society prepare to accept and look upon those hippies today? I think there is a lot of talent and creativity in that group. How are they accepted?

Dr. TAYLOR. If you think of two circles, conformity and creativity, they overlap 10 percent. Nonconformity and creativity overlap about 10 percent and among the nonconformists you might find some creatives but you might find many others who are different. We spent some time this summer with Toynbee— he writes history in terms of creating and emerging and talks about the two stages of creativity. Maybe this pattern isn't always followed but one is called withdrawal and the other return. He talks about this establishment as tending to be conserving, conserving what we have. John Gardner has said:" Too many people in leadership positions spend all their energies attending the old operations and doing nothing to improve operations." Then people who are disenchanted or can't quite take what we have in the establishment withdraw sensing something is not perfect, but can be improved, and they react sufficiently in disenchantment that they break away and withdraw. You raised the question about the hippies. If the second stage is going to return or create a whole new society—he usually thinks of history as their returning and modifying the establishment sufficiently to make history—he is raising the question: Will full scale creativity come out of this? They are showing the first sign—they are a product of this, a by-product, and are not satisfied with the rat race. What is it they say? Get out of your mind and come to your senses. Whether they will be able to come back and tackle the establishment successfully to make some change in the establishment is the question. The point I was trying to make, in response to Dr. Manz' comment, is not necessarily the university, faculty or the student; but perhaps the outside, the fringe society around the university, political, social, whatever it may be, that is resisting change. Perhaps of teaching methods or what have you, more than the students, or the faculty. The constraint here is not with the university and teaching methods necessarily but that society which surrounds the university and resists change within the university.

Mr. IRWIN. I have a feeling from listening to the responses that the solution is not in changing the curriculum but the other factors surrounding it. If we go ahead and devise a better curriculum we are not necessarily solving the problem.

Dr. MANZ. I agree as far as curriculum goes. A few days ago my son came home and had to memorize symbols of the periodic system of the elements. I remember when I was in school, I learned it because I loved it. I learned it without knowing it but I never had to sit down and memorize it. When a teacher doesn't know what he is doing—he is doing severe damage to talented people having them memorize completely dead information.

Mr. CETRON. You mentioned the nonconformists are only partly overlapping. How many individuals are way ahead?

Dr. TAYLOR. You want creativity to emerge. Evidence from the studies of highly creative people is that we almost have to go back to energy use. If we want to do something with minimum expenditure of energy, we get a habit working for us so we can tie our shoes without thinking—do you understand what I am saying?—or we don't pay attention to certain things. We don't expend any energy. The highly creative people, according to studies today, are rather typical people in many areas using habitual responses and so on and conserving their energies, a high percentage are available for areas of battle, so to speak. Does that make sense? They would appear to be conforming according to many things and quite a few are trying to do all they can to be nonconforming or conforming to a nonconformist pattern.

I said there are killers of creativity. I'm not sure I agreed with that last comment. I think lower down in the school system, the greater hope—the university can be our hardest nut to crack. I also think the excuse of the faculty and principals and so on is the public because of their lack of leadership. The public wants the best it can get for their kids. Take the public—they are with you if you can give them the best—this is from the research we have on the youngsters.

I made a speech at the PTA that almost all children are above average, and almost a third of them are highly gifted in at least one area of talent. The public and the children will be with us when the system is better adjusted. It is the system that had adjustments to make. Let me close with this story: It is an anti-killer, a killer like an antimissile missile. There are many killers of creativity around. It happens so naturally and humanly that it just kills you to see it happen especially to you. Someone said: what if we decide this is a highly important talent in creativity and we should do all we can to identify and develop it in our children for the next 15 years, and after we did, would there be too many creative people? The reply was made: If, at that time, there are too many creative people and it is a problem, there will be enough creative people around to solve it.

In principle, the demographer deals with a simple item, the individual human being, who in principle should be easily countable. However, Mr. Irwin's paper shows that this simplicity is misleading. The actual counting of those humans who are now alive is a serious enough problem, let alone predicting where they will move, when they will have children, and when they will die. This discussion of the problems of demographers should be comforting to those forecasters who have to deal with even more intractable data.

—Editor

DEMOGRAPHIC PROJECTION TECHNIQUES

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INTRODUCTION

Those who make projections of population and of the demographic characteristics of population have made little use of the methods developed by economists and by others engaged in forecasting. Rather, they have developed their own techniques, in response to the character of vital statistics and population census data. This paper discusses some of those techniques.

The study of population dynamics has a long history of which the development of the life table in the 19th century and of stable population theory later, in the 1920's, are notable examples. It has some similarities to the study of depreciation of physical assets, but in general it has unique features not common to other disciplines. A population is composed of a finite number of units (human beings) each progressing from age to age through time, beginning with birth and ending with death. Thus age is central to most demographic projections.

Although once it was common to develop population projections by fitting curves to the total population. now it is usual to analyze growth by components of change, by age, or by age and components of change combined. Population change is the net sum of births, deaths, and in- and out-migration. Projections may be made of each component separately and total change then obtained by summation. The presumption is that the determinants of the number of births, deaths, and migrants are independent of each other. This is not entirely true, but true enough for statistical purposes.

The projections are further defined by making projections for each age group in the population separately and obtaining total population by summing all ages. Each age group is at a different point in the life cycle. To keep this concept of a life cycle straight, we define an age cohort. An age cohort is all the population born in the same year or group of years. The cohort assumes different ages in different years as it passes through life. For example, the cohort born in the years 1895 through 1899 was age 60–64 in 1960 and will be 70–74 in 1970.

In order to provide for an orderly progression of cohorts from age to age, the age detail and the time detail must have the same interval. Thus, we must make projections of 5-year-age groups by 5-year intervals of time and single years of age by single year intervals of time.

Let us review, at this point, how population projections are made by what may be called the cohort-component method. One starts with a population at some base date distributed by age and sex. Then one multiplies the number of women in the childbearing ages by age-specific birth rates to determine the number of births in the subsequent period. One multiplies the population and births by survival factors to determine the number of survivors at the end of the period. Finally one adds in the number of net migrants to the survivors. The result is an estimate of population by age and sex advanced one interval of time and one interval of age.

In organizing a project calling for population projections of one or more subgroups of a given population, a common procedure is first to develop a population projection for the largest unit under consideration, called here the parent population. This parent population is commonly the population of the Nation as a whole but may be the population of a State or local area or some subgroup such as American Indians or the population living in households. The population may then be distributed by area, such as by State, or by characteristics such as school attendance, marital status, or labor force participation. The projections for the parent population should be more accurate than for the subgroup, for the smaller unit is subject to all the factors of change that the parent population is plus the factors that lead to redistribution of population among the subgroups. The procedure of estimating the parent population first permits the separation of the analysis of population change into those factors common to all subgroups and those factors causing redistribution between them.

This paper will first discuss the problem of making national population projections, next, of making projections of demographic characteristics, and then of making State and local area projections.

NATIONAL PROJECTIONS

In making projections of the national population, projections are first made of birth rates, death rates, and net immigration distributed by age and sex. It is believed that mortality and immigration may be estimated with fair precision.

Internal migration may be the most important component for many subareas of the population, but immigration is of only secondary importance to the growth of the United States population. Over the past 10 years it has amounted to about 13 percent of total growth. Furthermore, it is a fairly stable component. The quota is the most important determinant of annual immigration. There are many other categories of migrants to be considered such as arrivals from Puerto Rico, illegal immigration from Mexico, refugees from Cuba and elsewhere, citizens who choose expatriation, and the arrival of children born abroad of American parents. None of these are important enough to change the general picture of limited immigration. The most important source of error for the projections is the age and sex distribution assigned to the immigrants.

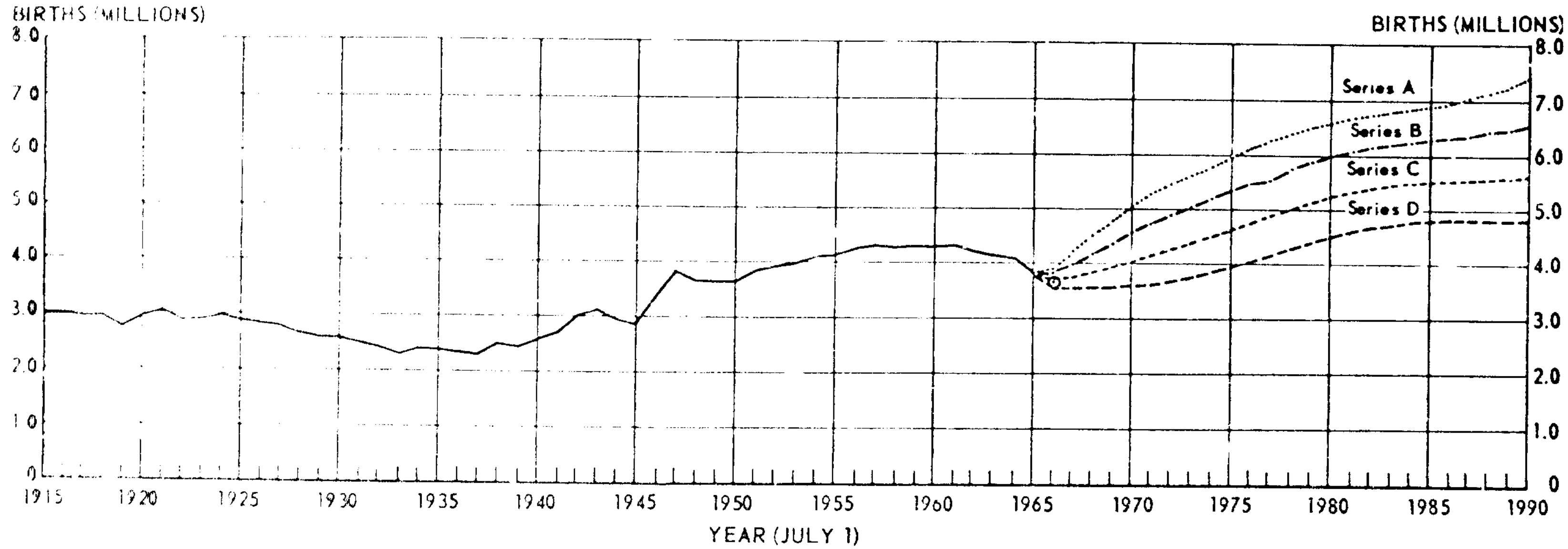
Mortality is now low, and except for the possibility of war or national disaster should not vary much. It cannot be expected to drop much more, for it is now close to some kind of biological limit dictated by human longevity. Nor is there any reason to believe that mortality will increase. There are two schemes for projecting mortality rates. One is to extrapolate past trends and the other is to set some kind of ultimate level for mortality based on an analysis of specific cause of death. In the second scheme, period rates are obtained by interpolation between the present and ultimate levels.

It is the births that make population projections for the United States so uncertain. In the last 50 years, births have fluctuated widely and proven quite unpredictable. This may be seen in figures III–1 and III–2 which show the total number of births and the total fertility rate (a measure of births per woman) from 1925 to 1990. The charts show wide fluctuation in past births and a wide band in expected births. They suggest a definite cyclic pattern of about 35 years duration with peaks in 1922, 1957, and 1979–84 and troughs in 1932 and 1967–70. These fluctuations have been analyzed by Whelpton, Ryder, and others, but so far, their analysis has not added greatly to the precision of the projections.

It should be noted that the swings in births do not follow the swings in economic activity. There have been marked declines in fertility in the 1920's and the 1960's despite economic prosperity in those years. Births do follow the 4-year business cycle, but these short term swings add little to the total variance in fertility. There is some evidence that young adults have not fully shared the prosperity of the 1960's. However, it is difficult to convert this observation into a forecast of future fertility. It would not seem that economic forecasts are useful for making national population projections.

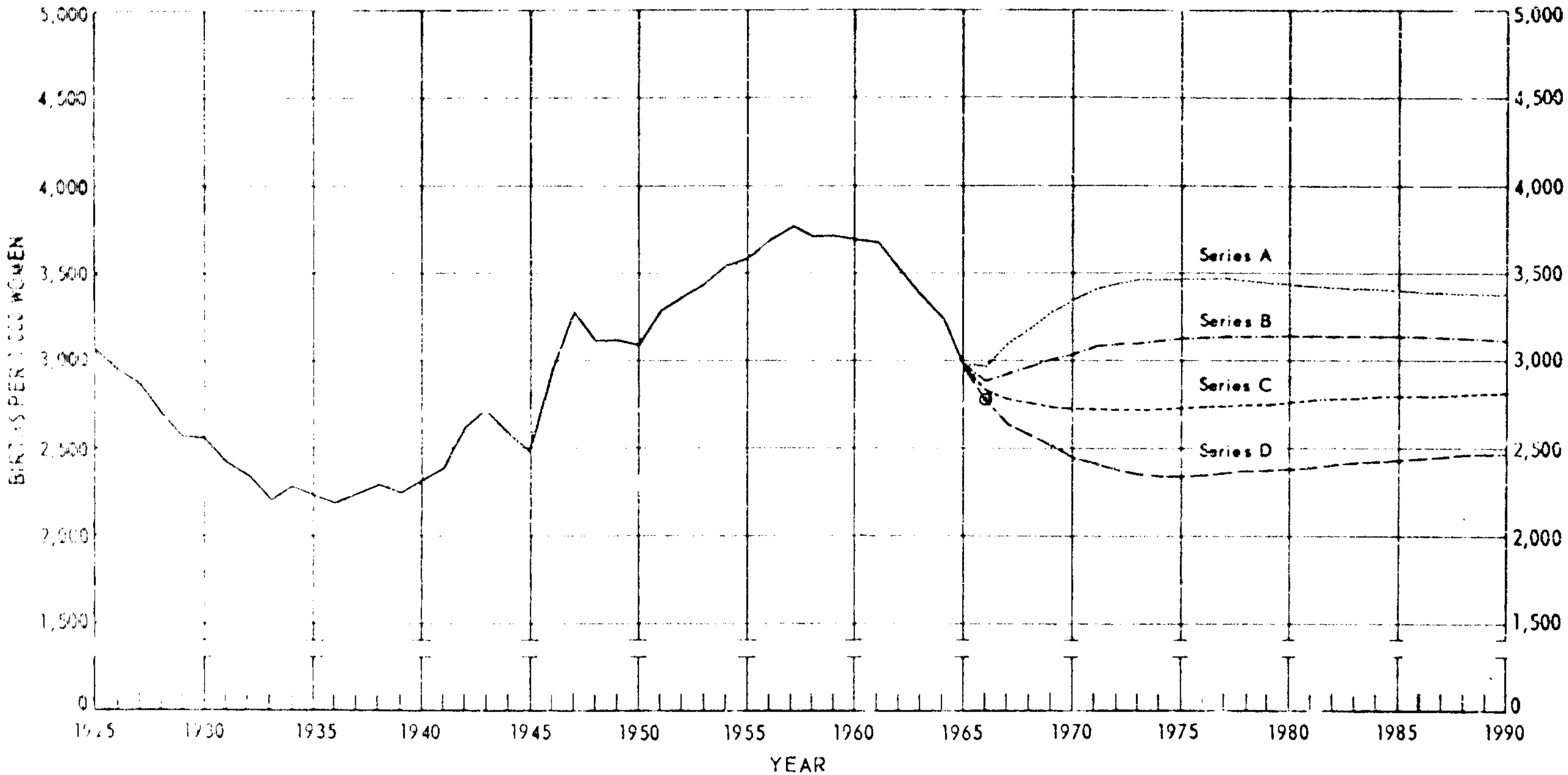
Fertility may be disaggregated analytically into average completed size of family and the age at which women bear their children. (The age at which women bear children is referred to as the timing of births.) Fluctuations in completed size of family are measured by the completed fertility rate shown in figure III–3. The chart shows that size of family, shown by the completed fertility rate, has fluctuated less than annual fertility shown by the total fertility rate. The rest of the change must be explained by changes in age of mother at childbearing. Four distributions of age of mother are shown in figure III–4. Series A is an approximation of the experience of the cohort of women born in 1930–40, who reached the peak of childbearing in 1960; Series D is an approximation of the experience of the cohort of women born in 1910–20 who reached the peak of childbearing in 1940. It is evident that historically high fertility has been associated with young average age of mother and low fertility with an older average age of mother. These changes in average age of childbearing may be measured by the median age of childbearing shown in figure III–5. It shows that the median for the 1910–20 cohort (used for Series D) was about 27.2 years and the median for the 1930–40 cohort (used for Series A) was about 25.3 years.

Age of mother at childbearing affects fertility in two ways. In the long run, it determines the length of generation. The lower the median age of mother, the shorter is the length of generation, and consequently, the higher is the projected number of births. In the shortrun, changes in age of mother cause births to bunch up or fan out.



NOTE: POINTS RELATE TO CALENDAR YEARS.
O ESTIMATED FIGURE FOR 1966.

FIGURE III-1.—*Estimates and Projections of the Number of Births: 1915-90.*



NOTES: TOTAL FERTILITY RATES REPRESENT SUMS OF AGE-SPECIFIC BIRTH RATES FOR A GIVEN CALENDAR YEAR.
SEE TEXT FOR EXPLANATION OF SERIES A, B, C, AND D.
⊙ ESTIMATED FIGURE FOR 1966.

FIGURE III-2.—*Total Fertility Rates for the Total Population, by Calendar Years: 1925–90.*

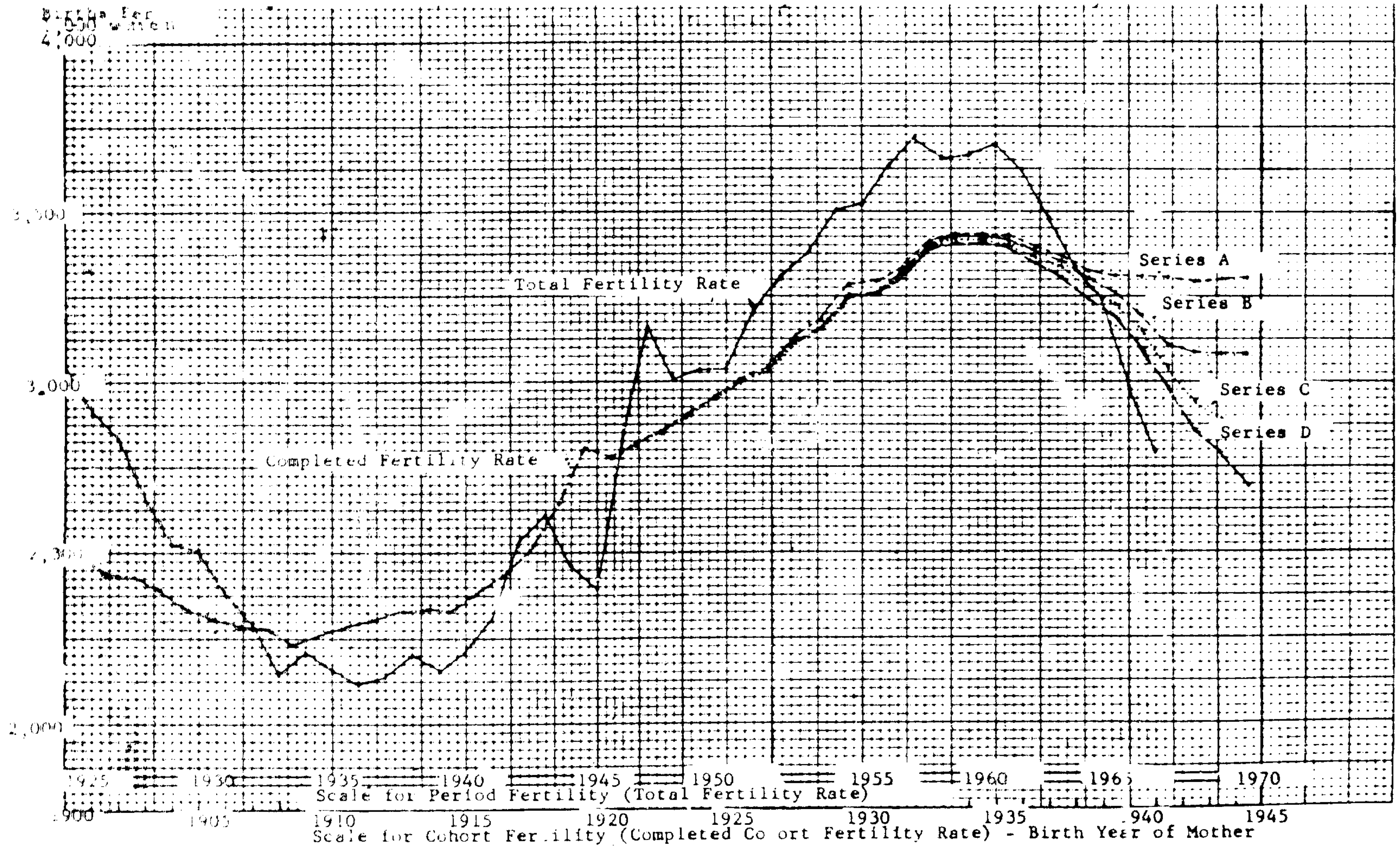
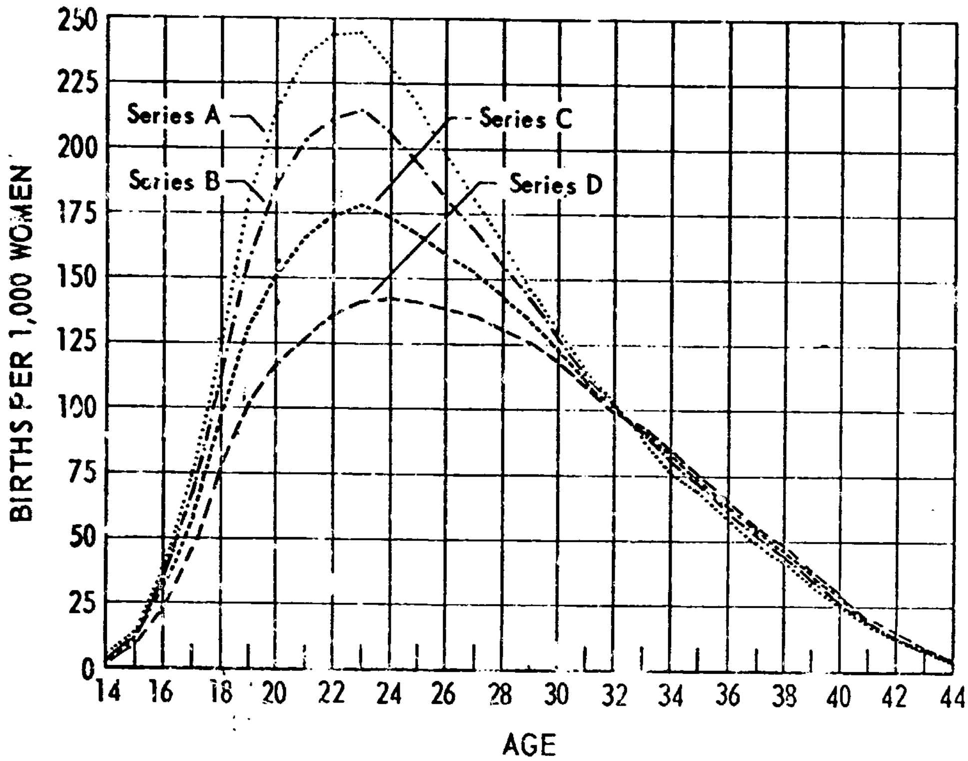


FIGURE III-3. —*Comparison of Cohort Fertility With Period Fertility.*



NOTE: DISTRIBUTIONS RELATE TO COHORTS BORN AFTER JULY 1951.

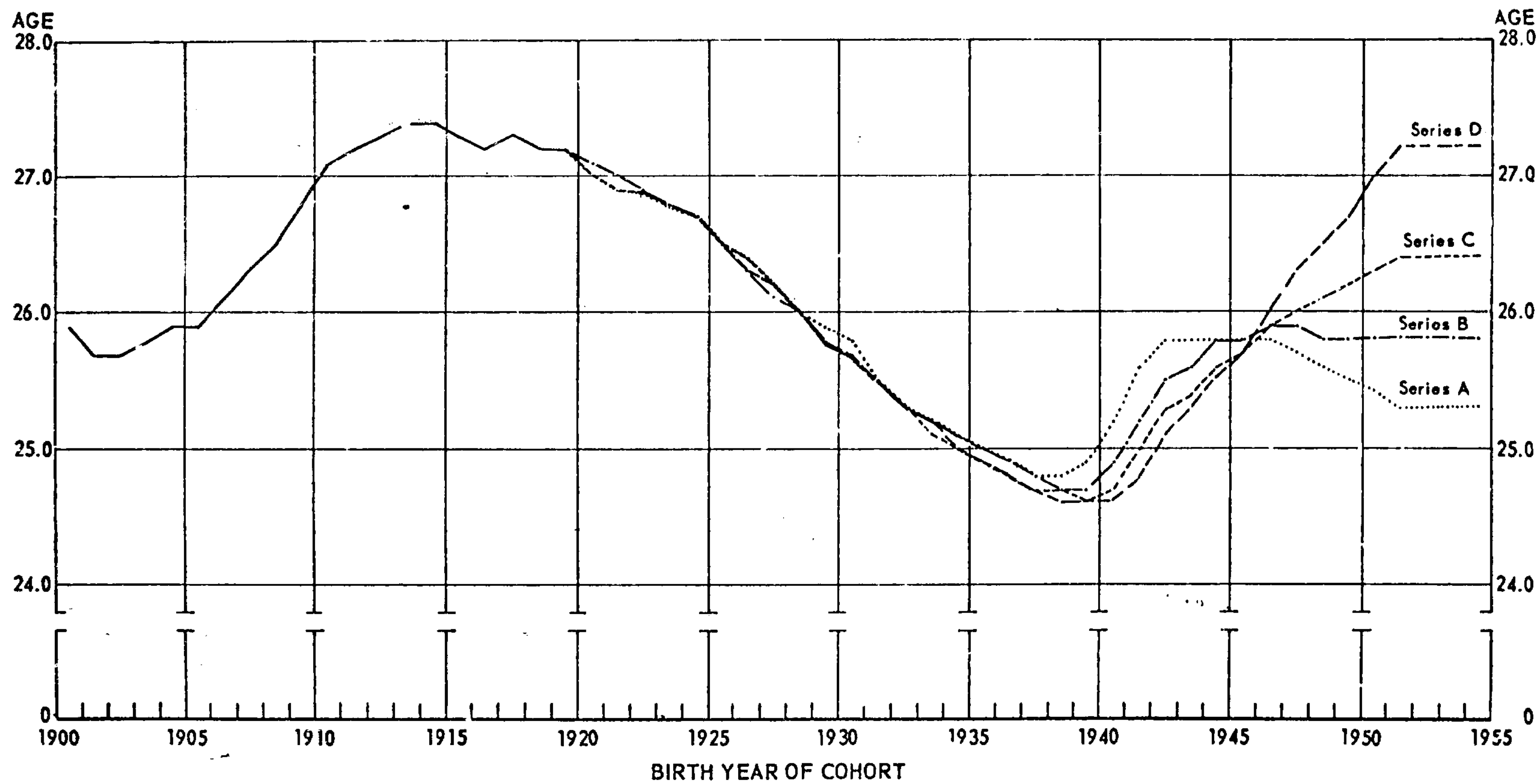
FIGURE III-4.—*Distribution of Age-specific Birth Rates Associated With Terminal Completed Fertility Rates.*

In the 1950's women who had delayed having children during the depression and war years were having children at a relatively high rate in their later childbearing years. Thus, the 1920 cohort had a high median age at childbearing. At the same time the younger women were having their children at a young age. Thus, the 1930–35 cohorts had a lower median age. The high fertility of the 1950's is to be explained in part by this overlapping of cohort fertility, a situation that could not persist. The downward slope from the 1920 to the 1940 cohort in figure III–5 is symptomatic of this overlap. Today, the situation is reversed. Older women are having fewer children and younger women are delaying having their families. This is illustrated once more by the upward slope after the 19[illegible]0 cohort.

It may seem from this analysis that changes in age of mother causing peaks and troughs is fertility exceed the range of variability expected for completed size of family. Disentangling these fluctuations from the historical data and deciding whether to build the cycles back into the projected series are two problems to be resolved in projecting births.

Two methods of making fertility projections that take account of this complex picture are being tested—the cohort-fertility method and the parity-progression method. The cohort-fertility method, as the name implies, analyzes fertility in terms of cohort. It assigns an average size of completed family to each cohort. It then distributes by age of mother the differences between the completed size and the cumulative fertility already experienced by the cohort. The age-specific fertility rates thus generated are regrouped into an annual series and then fed into population projections of the cohort-component variety described above. The cohort-fertility method is the one that was used for preparing the latest set of population projections by the Bureau of the Census.

The parity-progression technique starts with women of childbearing age at some base date distributed by age, marital status, parity (that is, the number of children ever born to the woman), and interval since last child (or since marriage for childless women). The women are multiplied by a corresponding set of birth probabilities. This operation determines both the number of births and the progress of women from one



NOTE: POINTS FOR FISCAL YEARS ARE PLOTTED MIDWAY BETWEEN JULY 1 DATES.

FIGURE III-5.—*Median Age of Mother, by Birth Cohort of Woman: Birth Years, 1900–01—1954–55.*

parity to the next. If mean parity is high at the base period, a peak has been passed and fertility should drop; if mean parity is low a trough has been passed and fertility should rise. Interval between births is a measure of timing of births and determines age of mother. There has been less testing of the parity-progression method than of the cohort-fertility method.

One distinction between these two methods is similar to one already made in discussion mortality projections. The parity-progression method is an extrapolation of rates of the recent past whereas the cohort fertility method starts with an assumption about the ultimate level of fertility and works backward.

The success of the cohort-fertility method depends in part on the successful prediction of completed size of family. Ryder and Lawrence have suggested methods of doing this by mathematical extrapolation. However, more reliance has been placed on the results of national surveys designed to measure expected size of family by interviewing women in the childbearing ages. Whelpton and Campbell believe that the expectation data are highly predictive. Critics say that women cannot really assess all the future developments that may affect fertility. They point out that within 5 years half the births will occur to women who were young and unmarried at the time of the survey and thus fall outside its scope. Finally, they object to a philosophy that says average expectations are predictive even when it appears that expectations for individual women are not.

The average size of family expected by the women surveyed has remained very stable in the last few years despite the sharp drop in fertility. It may be argued that the drop is due entirely to a change in timing, and not to a tendency for women to have smaller families. Experimental calculations show that this is possible. If this is true, a rebound of fertility is likely. However, it is hard at this point to give full credence to the surveys.

The latest projections made by the Bureau of the Census use the results of the surveys only in a general way. Three different surveys all place average completed fertility at about 3 children per woman. The projections assume a range of 2.45 to 3.35 children per woman. They also vary the timing of births. As a result the projections have a greater range in the short than in the longrun. We are hopeful that the parity-progression method will give more reliable shortrun projections, although probably less reliable longrun projections, than does the cohort fertility method. Perhaps eventually we will find some way to splice the two methods so as to obtain the advantages of both.

There is great uncertainty as to level of future fertility. Despite the considerable current research that has gone into the subject, much needs to be done. There is a need for a great deal of mechanical experimentation with the proposed methods for making the projections. There is a need for more detailed data on birth interval for the parity-progression method. There is a need to enlarge and refine the surveys on birth expectations, and there is a special need to consider the philosophical underpinnings for using expectation data for predicting the future.

DEMOGRAPHIC CHARACTERISTICS

So far, we have been discussing projections of population only. We may now consider methods of projecting demographic characteristics. These include school enrollment, educational attainment, labor force participation, marital status, and the number of households and families.

There are two general methods of projecting demographic characteristics. One is the cohort-component method, already discussed in explaining the manner of projecting the total population. The other is the participation rate method. In this method, projections are made of the proportion of the population at each age in some subgroups of the population. These proportions are then applied to the total population by age to obtain the number in that subgroup. For example, projections may be made of labor force participation rates by age, and these rates are then applied to the total population to obtain the number in the labor force.

Let us give two examples; projections may be made of school enrollment rates by age, and these rates applied to projections of the total population by age to obtain the number enrolled. This is the participation-rate method. Or, grade progression rates may be applied to the school population distributed by grade at some base date to project the number left in school. This is the cohort-component method.

The second example is of marital status. Projections may be made of the percent single by age, and these percentages are then applied to projections of the total population to determine the distribution between single and ever-married. This is the participation rate method. Or, first-marriage rates may be applied to the single population at some base date to determine the number of marriages. Then first marriages are subtracted from the single population and added to the ever-married population to determine the marital status of the population at the end of the year.

Each method has advantages and drawbacks. One consideration is whether there is likely to be a cohort effect, that is whether the previous experience of a cohort exerts a significant influence on its future development. For example, a cohort that has unusually high enrollment part way through the education cycle is likely to continue having high enrollment thereafter and to achieve high attainment in the future. This information will be lost if the projections are made by the participation-rate method. On the other hand, the cohort-component method is affected unduly by errors in the data for the population at the base date and will accumulate errors through the repeated application of the transition rates to the population. Where projections are made by a linked process, as in the cohort-component method, errors in the data tend to cumulate until unreasonable results may be obtained. Cumulative errors may be reduced if a target can be imposed on each cohort. For example, the proportion ultimately marrying might be assigned to each cohort in the marital projections.

A suitable method for projecting households depends on the state of the housing market. If housing is in short supply as is still true in many European countries, the number of households will depend on the enterprise of the construction industry. If they are in plentiful supply, then the number will depend on the number of adults who decide to establish their own households. Since the number of households equals the number of household heads, the latter may be projected like any other demographic characteristic. Theoretically, either the cohort-component or the participation-rate technique may be employed, but the data needed for input to the cohort component technique are not readily available. Hence in practice, as with most other demographic characteristics, the participation-rate method is more frequently used.

LOCAL PROJECTIONS

Projections for States and local areas may be made by extrapolating past trends in population growth, by estimating the response to some future development, such as the location of new industries, or a change in the relative attractiveness of different geographic areas as places of residence. Often there is little really known about the determinants of future growth, so that some kind of extrapolation of past trends by component is the most commonly used method. The procedure is strengthened by reference to projections for a parent population for the birth and death components.

In developing projections of population or of population characteristics for a single area such as a State or county, a choice can be made between developing a projection independent of outside factors, i.e., developing a parent population, or treating the area as a subgroup of a larger unit for which projections already exist.

If the State or local area is treated as a parent population the problems faced are similar to those for national projections, except that the component of migration is relatively much more important, and often insufficient data are available to determine historical trends in migration accurately. In projecting fertility, only period fertility analysis can be used, because migration obliterates cohort fertility analysis, and the data needed for parity progression techniques are not available.

If the area is treated as a subgroup of a parent population, a common procedure is to restate basic data in terms of their proportion to the parent population. A historical series of these proportions may be extrapolated into the future. This proportion is then multiplied by the projected figure for the parent population to determine the desired figure for the area. This technique, usually referred to as the ratio method, is, of course, related to the participation-rate method described above. The ratio method can be developed in widely varying degrees of detail. A single projected total population for a single area can be obtained in this manner, or a more detailed projection for all areas can be obtained. It may also be applied to each age group, with the total population obtained by summation. The procedure is simple and assures a degree of consistency with the parent population.

Even if the cohort-component method is used, effective reference to a parent population can be retained, especially if projections for all subgroups of the parent population can be developed simultaneously. This is the procedure used by the Bureau of the Census in developing population projections for States. To project deaths, one national set of age-sex-color specific survival rates has been used, and the sum of deaths for all States derived in this manner is forced into agreement with the number of deaths developed by the national population projections.

Fertility rates are also linked to national values, although rates specific to each State are employed. The ratio of each State's general fertility rate to the national rate at the base date is computed, and projected values are obtained by assuming convergence to national rates at some future date. Values for intermediate projection dates are obtained by interpolation. The births are generated by multiplying the projected fertility rates by the appropriate population at any future date. Births are then summed for all States and forced prorata into agreement with births projected for the Nation.

The component offering the highest degree of uncertainty in State and local projections is migration. Although an error in the projection of the birth component over a 10-year period could cause a deviation of up to 10 percent from the total population that actually develops, the error would tend to be of the same sign for all areas, thus not changing their relative position a great deal. The migration component could cause a deviation of from 10 to 20 percent of the total population or even more, and the differences in the actual growth rates from those projected would be accentuated from area to area, since the gain of one State through migration is a loss to the others. Furthermore, net migration rates are subject to much greater fluctuation in the shortrun than is the case with birth rates. For these reasons, considerable attention has been given to the analysis of internal migration by demographers, and by economists and members of the business community. The economists and businessmen are drawn into this field by the desire to forecast economic variables, many of which are heavily dependent on population size and composition.

In making population projections migration between areas within a parent population can be calculated either as net migration, that is the excess or deficiency of inmigration as opposed to outmigration, or as gross migration, where separate computations are made for out- and inmigration.

Under the gross migration procedure the sum of the projected outmigrants must equal the sum of all inmigrants, and when net migration is used the sum of the net values for all areas must be zero. Thus migration for one area cannot logically be calculated in isolation, for it must affect the migration for some other area.

Projections of net migration have been more common than projections of gross migration, because the data for estimating net migration have been more readily available. Since the mobility of persons in the United States is free of political restrictions, event reporting of migration has been meager. Net migration, however, can be obtained by noting population change from one census to another, explaining some of the change by reference to registered births and deaths, and inferring that the remaining change is due to net migration. Net migration determined in this fashion is sometimes called residual net migration. Unfortunately migration values obtained in this manner include errors of census underenumeration, age misreporting, or errors in the registration of vital statistics. None the less, net interstate migration is often so heavy as to dominate the picture.

Although net migration statistics are readily available, their use in population projections is not entirely satisfactory. There are certain technical difficulties (discussed below) in balancing the sum of net migration for all areas to zero. More fundamentally, net-migration represents only the surface effect of two powerful streams of out- and inmigration. When one probes for causes of migration, the search leads not to one question, "Why is the net effect of migration thus or thus?" but to two questions, "Why do certain people move out, and others move in?" For example, even in areas of heavy net outmigration, there is considerable gross inmigration, and in areas of heavy net inmigration the rate of gross outmigration may be as high as for areas of net outmigration. Thus gross migration has recently come under increasing scrutiny. If the necessary basic statistics are available, gross migration as greatly to be preferred over net migration as a basic projection technique.

Projections of gross migration can be handled either by calculating the migration streams between each pair of areas, or by calculating outmigration regardless of destination, and inmigration regardless of source. By this latter technique a "migration pool" is developed by first computing outmigration for all areas and then distributing this pool back to the areas as inmigrants.

To our mind, the pool method of projecting migration is to be preferred at this time over the place to place stream method, taking into consideration the character of the basic statistics and the imprecise nature of present knowledge about the determinants of migration. It is believed that outmigration is a function of specific population characteristics, especially age, and is relatively constant from area to area, while inmigration is more sensitive to the different levels of attractiveness of alternative destinations. But the precise functional relationships involved are not known, and it may be impossible to determine them without more precise data. If place to place migration streams by age are projected a complex model with a very large number of cells is the inevitable result. On the other hand, the migration pool method separates migration into the two chief dynamic factors of out- and inmigration, yet remains simple enough to permit the analysis of the relationships involved and to correct for shortcomings of the data with relative ease.

A number of problems exist in connection with the basic gross migration data obtained in the 1960 Census. Only 92 percent of those persons found in a reinterview study to have been interstate migrants were so indicated by the regular census results. This represents misclassification of those enumerated by the census. The overall 3 percent net undercount for the 1960 census may be presumed to be considerably heavier for migrants. Of those enumerated, another 3.3 percent of those who moved did not indicate their place of former residence, hence are not included in the outmigrant statistics. Only partial information was obtained about mobility of members of the Armed Force, college students, and institutional inmates, yet movements of these special groups often obscure migration more directly related to basic economic and social factors.

When migration is projected for all areas of a parent population by the application of rates, the sum of the inmigrants projected for the areas tends to exceed total outmigrants and the values for the various areas must then be adjusted to zero (or to a control total representing net immigration to the parent population). As the projection proceeds, the imbalance becomes progressively larger, requiring larger and larger adjustments. This difficulty is brought about by a paradox implicit in the use of migration rates. Specifically, for areas of net inmigration, which grow faster than the national average, the computations produce larger and larger numbers of net inmigrants. For the outmigrant States, however, the computed number of net outmigranst increases less rapidly, since their population base is growing more slowly than the national average. Since the sum of net interstate migration for all areas must equal zero, it is necessary to adjust the computed in- and outmigration in some way.

Various techniques have been employed to bring the computed number of migrants into balance. When working with net migration rates none of the techniques available for this adjustment is entirely satisfactory, but we have found one that seems quite satisfactory for the gross migration pool method. In this method, outmigration is computed from gross outmigration rates applied to the population of each area to form a pool of internal migrants. This pool is distributed as inmigrants among the areas in proportion to the distribution of inmigrants during the base period. Thus each outmigrant becomes an inmigrant, and the sum of net migrants for all areas is zero. Net immigration to the parent population can be added to the migrant pool for distribution among the areas, or handled separately. Due to the distinctive character of net immigration into the United States, the Bureau of the Census makes a separate allocation of this component in its projections of State population.

The same set of proportions can be used without change throughout the projection period. This procedure has the automatic result that net migration for each of the areas tends toward a level common to all, as the projection proceeds through time. An area growing more rapidly than the average because of net inmigration tends to have its rate of net inmigration decreased as its population base grows. By contrast the slower growing areas fare better as time passes since they retain a constant proportion of the inmigrant pool while their relative share of outmigration is declining.

The tendency toward equilibrium obtained by treating out- and inmigration in this manner is in accord with the general assumption that net migration streams will tend to eliminate differentials existing among the areas at the beginning of the projection period. If this assumption is not desired, rates of inmigration can be developed for the base period, and inmigration computed for each projection period, with the resulting values converted to proportions of overall inmigration for the purpose of distributing the out-migrant pool. The sum of net migrants for all the areas will still balance with the overall total, but the migration assumption here would be of continuation of net migration differentials among the areas.

We have said that internal migration is best projected by the application of rates to a population base. This is true of the majority of migrants who move as individuals or families in response to economic opportunity or social factors. It is not true for certain large groups of migrants who move in response to administrative decision, and are often concentrated in large group quarters such as military barracks, college dormitories, or institutions. Regardless of whether the projection is in terms of net migration, gross migration, gross migration, or place-to-place streams, account should be taken of movements involving the Armed Forces, and if possible, college students and inmates of institutions. It is particularly important to remove these groups from the base data from which the rates are calculated. The need to take account of these special populations becomes more acute as the geographic detail and age detail becomes more precise. Thus at the county level, the presence of a major university can dominate migration statistics for the age groups involved. To leave these statistics unadjusted, thus allowing them to be converted to rates of the population of the area, can seriously distort the results of a population projection. Interstate movement of college students is not such a serious problem, although projections of State population could probably be improved by adjusting for this factor.

In the case of the Armed Forces, however, an adjustment is almost mandatory at either the State or county level. About one-sixth of all males aged 20–24 in 1960 were in the Armed Forces. Since a standard tour of duty is about 3 years, practically every member of the Armed Forces can be assumed to move to a different State in 5 years, or at least to a different county. If movements involving the Armed Forces are allowed to remain in the base period statistics used for computing rates and/or proportions of migration, the implicit assumption is that the military developments during the base period, both for the area and for the parent population, will be duplicated during each projection period. Thus, if a new military base is established during the base period, an unadjusted projection will build it anew during each projection period, like a sorcerer's apprentice.

A complete adjustment for the impact of military movements ideally should allow not only for changes in station strength of military bases, but should also adjust for movement between the civilian and military population. Adjustment consists of removing these streams from the statistics for the base period, then making some assumption about future size of the Armed Forces and adding or subtracting an adjustment for each area consistent with this assumption during the projection period.

The basic data required to make these adjustments are difficult or impossible to obtain in full detail, and in practice it is usually necessary to apply a partial correction. It is felt that such corrections improve the projections, even though falling short of a perfect solution. In developing an adjustment for any special population it should be remembered that census migration statistics are subject to errors of census undercount and misreporting. The 1970 census will provide important new detail as to the migration of Armed Forces personnel and their dependents. This detail will contribute to the understanding of census misreporting as well as providing material for developing a more detailed adjustment.

To derive alternative projections of the population by States, the different fertility assumptions of the national projections can conveniently be used, since changes in the national level of fertility can be expected to affect all the States to some degree. Alternative migration assumptions have been developed in projecting both gross and net migration for States. For gross migration it has been possible to adjust projected out- and inmigration in such a way as to gradually approach an equilibrium of net migration among the States at a selected future point in time. This provides an alternative value to that obtained under the assumption of continued differentials among the States. For both gross and net migration projections the length of the base period can be varied, or two base periods can be used, averaging the migration rates for each period, and using different patterns of weights to achieve an alternative migration assumption.

The customary assumption that migration will continue in the future according to some pattern of behavior in the past has been felt by many to be a weakness of demographic projections for State and local areas. It is widely believed that the future population development of a community is heavily dependent on the economic potential of the community, and that once this potential is determined, the approximate population level which can be maintained is determined. However, projections of economic activity for an area are often an extrapolation of past trends. There is little reason to believe that this kind of projection should be more reliable than projecting population from past trends in population growth. It may well introduce an additional source of error; much migration is not in response to economic opportunity.

More meaningful projections of economic activity may be made from a careful inventory of prospects for the growth of primary industry within an area, especially of those that produce for export to other areas. This information is used to estimate prospects for secondary industry, employment in secondary industry and construction, employment in services, excess unemployment, and finally the population dependent on the labor force. This is known as Hoyt's economic base method. It is very time consuming to apply, and the increase in accuracy may only be moderate.

A projection of economic activity for regions can also be developed by working from a parent projection of the economy and allocating industrial activity to the various areas according to locational factors, principally the location of resources and markets. One application of this method allocates secondary industry in the same manner as primary, thus abandoning this dichotomy as the explicit basis of the regional projections.

It has not yet been demonstrated that population projections dependent upon an analysis of the economic factors are more reliable than those based on purely demographic factors, and for the present demographic projections are coexisting with those based on an analysis of economic factors. This coexistence has resulted in some cases in the inclusion of both techniques in one projection model. An estimate of future employment based on a demographic projection assuming no migration is compared with one based on an analysis of economic activity. The observed difference is assumed to result in a net migration of employed persons, from which the net migration of all persons may be estimated.

Finally, with respect to population projections for small local areas, expected land use has provided estimates of future population. Here the analysis is in terms of areas available for residential development and expected density of population. Such projections are often referred to as saturation populations and are sometimes presented without indicating the date at which this population might be expected to materialize.

RELIABILITY OF PROJECTIONS

Statements as to reliability of projections are much in demand by users. There seems to be no way to assign a probability measure to a set of projections, however. It is common to avoid the issue by labeling a set of projections as illustrative or as true, given the assumptions. This practice ignores the fact that some types of projections would seem to be more soundly based and, hence, worthy of greater confidence than others.

One way of indicating reliability is to calculate a range. It is defined by a high and low series of projections. They are generated by choosing assumptions that are considered extreme, but nevertheless, thoroughly reasonable. Thus, the projections are something less than confidence limits. Yet the narrowness of the range does give some indication of the confidence placed in them.

The Bureau of the Census publishes four sets of national population projections. Each set has a different assumption about fertility but all sets use the same assumptions about mortality and immigration. Hence, the projections differ for the cohorts yet to be born but not for the cohorts already alive at the base date. There is an implication here that the projections for these cohorts are so reliable that there is no need to indicate the degree of reliability by a range. Certainly, they are more reliable than the projections for the cohorts yet to be born. Nevertheless, a check of the projections against subsequent census counts shows important errors in the older cohorts, too. For example, projections of the population 65 years old and over in 1960 prepared in 1957 were short of the 1960 census count by 6 percent. The principal source of error seems to be in the counts both in the 1950 census, upon which the projections were based, and in the 1960 count, which served as the standard to which the projections were compared. The explanation of these errors has not been determined.

It seems then that errors of census count are an important source of error in demographic projections and also complicate the measurement of the success of the projections. It is hoped that techniques of estimating net census undercount will become sufficiently reliable to provide acceptable population figures as a base for population projections. A corrected population base for the projections and corrected population distributions against which to evaluate them should make a considerable contribution to their improvement.

Projections of demographic characteristics may often be assigned a narrower range than projections of the total population. An analysis of the participation rates or transition rates for various characteristics often indicates that they may be afforded considerable confidence. Hence, the uncertainty about births is the principal source of uncertainty of such characteristics as school enrollment. Where birth cohorts are not involved, such as projections of marriages and labor force for the next 15 years, the projections are highly reliable.

Alternative sets of projections are often calculated for distributions of parent population among States or local areas, but there are methodological problems in calculating the migration component in such a way as to provide reasonable ranges for every member of the distribution. To provide maximums and minimums of net migration for each area would require computing as many sets of projections as there are areas. Methods of obtaining alternative series of projections by varying the migration component were discussed above. Projections of births and deaths are not a problem in this respect, since the alternative assumptions for the parent population can be used without changing the basic procedure outlined earlier.

DEVELOPMENT OF ALTERNATIVE METHODS

Attempts are made from time to time to find more mathematical methods for making population projections. The most notable achievement was the development of stable population theory by Lotka in the 1920's. He demonstrated that a schedule of age-specific birth and death rates applied to any population will eventually generate a predictable age distribution and rate of population growth that are independent of the initial age distribution. Lately, Coale and Demeny have applied stable population theory to making projections for high fertility countries.

More recently, attempts have been made to apply matrix algebra and Markov chains to population projections. The cohort-component method, which defines population in terms of a population distribution and matrixes of transition probabilities, suggests that matrix algebra would be a useful tool. However, the work so far suggests that it can add little to what is already known from the cohort-component method and from stable population theory.

The methods employed by economists and researchers in other fields seem to have little application to demography. We find demographers making little use of regression analysis, simultaneous equations and the like, so common in economic projections. Whetner the uniqueness of technique is truly a reflection of the nature of demographic material or whether it ony reflects the parochial view of demographers might well be a topic to discuss at this conference.

Mr. FRIEDMAN. There is an old adage that I always felt to be true: The rich get richer and the poor get children—is there any validity to that?

Mr. IRWIN. The lower income groups do have larger families. In the rural South, for example, the nonwhite family, both white and nonwhite families the per capita income is very low with high birth rates. I don't think the situation has gotten far enough to see whether the same conditions that prevailed in the thirties, when the people who could afford to raise children did not have them, is coming true now. During the high fertility of the fifties, it wasn't so true. Both rich and poor were having children, about comparable-sized families.

Mr. CETRON. I've got a question. It seems that you're controlling and compensating. If your figures don't come out the way you want, you bring in more migrants to make them come out right. You are actually taking in migrants to compensate for the lack of validity of your sample.

Mr. IRWIN. No, we are not that chicanerous. We'd probably have done something when we made the basic assumptions. Just before the immigration started—that's happened in the last year, and in that blue book we make it very simple. I must admit we make a very simple assumption about immigration, the national projection. The blue book you have, 286, was about 300,000 a year—it had been running around that—and now in the new ones, which are going to come out and are referenced on page 1—we are assuming 400,000 a year. The latest computation is over 400,000. But we didn't do it in order to come out right, however, we would like it to come out between B and C.

Mr. CETRON. Can you modify that figure with figures going out—the number of human beings on the continent? There are so many Americans on passport who are out of the country all the time.

Mr. IRWIN. We do take this into consideration as one of the components. We have about five categories of net immigration. One is citizen arrival and departure, so we try to take this into account, and we take into account dependents of American service men coming back from overseas, for example. There are a number of places where statistics are not perfect, especially by age and sex. We have serious problems. Furthermore, the Bureau of Immigration in 1957 stopped giving us a count of alien emigrants—not alien immigrants, but emigrants leaving the United States with the announced and stated intention of staying away permanently. We do try to take these into account. The Armed Forces abroad are also taken into consideration—we have a million men overseas.

Dr. FULLER. On the curves, is there any showing of that final net?

Mr. IRWIN. No. Once a year we have a report on the components of change which shows some of the variations from year to year. This is net immigration as a rate down at the bottom. This is 1951 and that's the absolute figure of about 300,000 a year. This would be my basis for the statement that it is relatively constant. But it has gone up as a result of changes in the law.

Colonel VAUGHN. Not too long ago in the Washington Post and other papers, there were stories about the 3 million to 5 million Americans that weren't counted in the last census. What is that going to do to your chart?

Mr. IRWIN. When you read my paper there is something pertaining to having a paper reviewed so that every little point gets in. I made the statement that these projections of the population that are alive on today's date are fairly accurate. We found that projections made during the fifties when compared with the 1960 census, for the older age groups, were not good. Looking further, you find the trouble is the population is undercounted and furthermore, is differentially undercounted by age. There are some very striking things in the table—the base population is phony. One is 5 percent low, another is only 2 percent low. These kids are really good. They are in school, mama is home taking care of the family. We do good with those kids, 5–14. Things really start going to pot when we get in the 15–19. For white males, 20–24, the undercount rate is about 7 percent and for nonwhite males, 20–24, is 20 percent. We missed one out of five of every nonwhite made 20–24. Obviously that's going to throw things off if you use as your criterion that your population projection should agree with the next census. I think the next major step is to build in, and work with the corrected population. In other words, first we correct the population for undercount and we now have an estimate of the true population, just an estimate. I won't use the words "true population." It is corrected. Then carry it forward and either state our projection in terms of the corrected population, which would require a resolution on the part of everybody who used census statistics, so it is a major step. Furthermore, we are not that sure about the exact undercount by age, however, we know there is a big differential in undercount. We know some of the percentages are big but we don't know exactly how big.

Dr. LINSTONE. Can't you use birth and death data to correct population projections and keep them more current?

Mr. IRWIN. Registered births with the native born population enumerated could not be done until about 1940, because in the United States birth registration was not even nominally complete until 1933 when the State of Texas entered the birth registration area, so until that time there were no printed statistics on registration of births in Texas and a few other States. Then in 1940 a test was conducted to find the percentage of under-registration of births. From this we take the births that were registered when the area was finally complete, correct them for underregistration: Apply mortality rates; compare them with the population enumerated in 1960 by age; by cohort, then comparing it with births during a stated period of years: They don't come out.

Dr. JOHNSTON. I wanted to make comment relating to undercount, too. We are in the process of writing an analysis of the implication of the undercount for labor statistics—that is, manpower and labor force statistics—in which we will do something to the review and approval of the Census Bureau. The idea I want to get across is you can be worried about the effect on the levels when you have missed 5 million people, or as one man put it, "You missed the population of Denmark," or something like that. The rates may not be affected significantly because we don't have too much evidence to indicate that the people missed are that different from the people who are caught, at least in the same area—people missed are not likely to be all different from the people caught, we are interested in rates of unemployment on rates of school attendance, they may not be that far off even though the levels aren't there. This is always subject to some doubt. As long as you have missed these people, you really don't know what they are like.

Mr. IRWIN I really didn't answer your question, Colonel Vaughn. First of all, there is some evidence to show with respect to fertility and, for example, the difference between nonwhite fertility rates as printed and white fertility rates. There is a differential undercount which tends to bring them together. But it is not too much after you do it with the best figures available, the rates and the trends are not seriously contradicted and the point Denis made is you are not quite sure everybody you missed had a different proportion of the characteristics you are talking about. Everybody assumes that all the missed people are unemployed; that's not necessarily true. They may be people who work, have no children and are away from home. Then I might defend the Census Bureau when the field division is trying to count all these people. We have a lot of ideas about how to do it, which are usually expensive. Between 50–60 we had this same constant amount of undercount, almost exactly. We missed 5 million people in 1950 and we missed 5 million in 1960. We would have had about 30 million more people, so the rate went down.

Dr. DALKEY. If you look at the distribution of ages in the almanac, it has some suspicious lumps. For example, a lump at the age of 59 and another lump in what looks like it might be preferred ages. I don't know whether this is due to misreporting.

Mr. IRWIN. It is due to misreporting of 1900 as the year of birth.

Dr. DALKEY. Did you try to take this into account?

Mr. IRWIN. In 1960 the Census Bureau had a dedicated staff so they said, "We are not going to ask just how old were you at your last birthday," which is what I will answer if you ask me how old I am, but they are going to ask what year was he born in. This was done in 1960 because when you ask everybody how old were you at your last birthday, there is a distinct preference for ages, like 45, 50, 60, 65, and so on. This is called age-peaking. In 1960, we asked, "What year were you born," and this caused an awful lot of finger counting at the door. People would say, "Let's see. I'm 18," and so forth. However, it did have the effect of eliminating the age preference for 10, 20, 30, 40 years and the only ones marked were people born in 1900. They came out as 59, but there wasn't very much preference for 1910, there was a change to the year of birth which added a distinct improvement on agency tables from the point of view of peaking. I just came from a pretest of the 1970 census in Philadelphia. We are very concerned about the undercount in the Negro ghetto. There's no question about it; it is high. That's where we are missing males. I should mention that the computations of undercount give no indication of the geographical location. The whole thing is predicated on a closed population. You've got births in the United States; you've got native born people and you take account of the people abroad, citizens abroad. Dr. Fuller, and you've got a closed population but that's all you know. You don't know whether it was in Harlem, in Mississippi, but it is believed to be in the ghetto.

The point of year of births, I observed several interviews with people with low education and our schedule was just too complicated. It is going to be tough. The year of birth requires a set of figures. If we said, "how old are you," that part of the question would be simpler and we would get the resu[illegible] the rest of the information.

Dr. FULLER. Since 1960 haven't there actually been less babies each year in the United States?

Mr. IRWIN. Wasn't 1963 the highest year for births?

Dr. FULLER. We actually had less babies and I think this is interesting in view of the fact that we would have more fertile females each of those years, would we not?

Mr. IRWIN. That's right.

Dr. FULLER. So we would have higher potential.

Mr. IRWIN. We are maintaining that the present drop in the volume of births could be caused by a change of timing and age of marriage and does not necessarily reflect a decision on the part of women to have smaller families.

Dr. FULLER. The customs of people in America, in the last decade during this period since World War II, has changed—the conduct of young people. The older people not exercising their will over the young peoples' lives. The young people have had much more opportunity, and they've lived around together and had more opportunities to make babies than they ever had before. I think this is quite a significant figure and trend.

Mr. MONAHAN. Why the drop in the birth rate between 1925 and 1929? You say there is a drop of the fertility rate.

Mr. IRWIN. The fertility rate dropped all through the twenties, I believe. The best discussion I ever heard was by a fellow named Fred Chino in the State College. He gave the best analysis and I have never heard anybody refute it. He said, "During the twenties, first of all, the women having babies came from large families and their mothers were generally overworked, but more than that, women had just achieved equality. They had gotten the vote in 1919, so this was combined with the feeling on the part of women that having babies was not the only aim of life and it was very popular to think about having a career. So a woman was a woman; she had a baby but that was the end of her responsibility. She was going to go out and make her way in the world. As this period continued, this attitude was the dominant one. Then in the thirties was an artificial financial restraint, so this deepened and then the war came along with the physical restrictions on having children and all of these things let loose in 1946. That's my answer.

Major MARTINO. I would like to address what I think was your original question—the methods you use and so on rather than the content of population studies. You put up a straw man that your techniques are unique to demography. I am sure you don't believe it and I believe it even less.

Mr. IRWIN. I sort of believe it but I am ready to be shown.

Major MARTINO. I would like to start—at least, try to direct the discussion in that direction by making some assertions of my own. First, any forecasting must of necessity be based on data from the past. That is, there is no other way to make a forecast except to ignore data completely and just gues. If you attempt to forecast rationally, you must start with what you already know about the past, to this extent your methods must be somewhat similar to everybody else's. Next, I would like to point out that what you have called the component equation has an analog in physics. This very same situation is known as the equation of continuity. The amount of material or whatever you have inside some boundary is that derived from the sources less that lost to the sinks, plus the next influx across the boundary. There again is something in your model which is at least paralleled by something in somebody else's model. This is to reinforce my contention that your methods are not necessarily unique.

Mr. IRWIN. Very interesting. What about this question? What we are really interested in, is whether you people can see parallels that would help us in this question of period fertility. By that I mean the rate at which all the women alive at one time are having babies, and on the other hand the idea of following a women through time until she is through having babies. Can we tie these together in some way?

Dr. SLAFKOSKY. I question your definition of fertility, because you are applying it only to women who have proved their fertility.

Mr. IRWIN. Very true. The state of our art is a crude way to get it. In my discussion of migration and statistics, we are quoting a rate of having babies by all women whether married or not and whether fecund or not. We should quote rates of married women. I am realizing the dangers of having a verbal discussion. I left out one whole technique of projecting births which is the parity progression method. The completed fertility technique of projecting births is, we think best, in the longrun, but in the shortrun we are not doing so well. It is very hard to get long range estimates of fertility of women to come out on the short run. So when we put the computations together we very typically get nonsense results in the first few years. It is a big problem. There is another way of doing it but unfortunately there is not enough data, even at the national level, and this is to go back to a cohort progression of women in effect and use the cohort approach with the women. Take all the women of a given age and assign them a parity: How many babies have they already had, then you say the chance that these women of different parities will have a baby during the next time period is such and such. Then you say, "OK. This is all women, aged 20–24, who have had one baby, two babies, three babies, four and more, you assign different probabilities and multiply these out." There has been some experimentation on this and the tables are hard to come by. Particularly the tables showing parity by length of intervals since the last baby, which is an extremely vital factor, because a woman who had one child 5 years ago is less likely to have another child than a woman who has had one baby 12 months ago. This is called the parity progression method. You can read our comments in the paper at your leisure. We would like to use parity progression approach in the shortrun and join it with the cohort fertility in the longrun but it is quite a trick in computation.

Dr. SLAFKOSKY. May I suggest that—in some way you probably have a more difficult problem than most of us and I'm not sure we are ever going to get the kind of conclusions you want. You are dealing with a subject matter that has many variables and the kinds of influences you gave Mr. Monahan are the overly large problems. I have a feeling there are a lot of medium-size and smaller ones that work with individuals, families, groups, and sectors that you will never get your finger on and these are the ones that are always giving you trouble. Even if you talk about a parity for a cohort under similar conditions, carry it through and always look at it in that way: You are not going to be set on any firm basis to make your prognostications.

Mr. IRWIN. We could further disaggregate. At the present time it is a great problem using the parity progression because we are not working up good tables showing parity as it passes through time. We are going to want to look at some trends before we project it into the future. As a general observation which I might make, going back to the undercount. The basic quality of population in statistics is not that good. They are never going to get it. People don't report well. You miss too many people and we get a lot of resistance. We've got 29 housing questions and about 30 population questions, that will go to 20 percent of the people; and plenty of studies to show that the answers jiggle. For example, mobility and migration, we asked them where they lived 5 years ago and only 90 percent of the people who actually moved interstate on the basis of a reinterview study marked it that way on the questionnaire. There is about 10 percent—we are talking about big numbers. I feel that further disaggregation is not feasible at this time. It is certainly something to consider.

Dr. CHACKO. Would you be able to prove your allocation method by the apportionment of migration to the 50 States which is 2 percent on the basis of the null hypothesis? It seems to me you mentioned two very important factors, those of the military establishments and universities, the actual number of students and military personnel are nearly constant, invariant. So, by ascertaining these, couldn't we say that these populations had to come from some place and they arrive, and use that as a refinement of the allocation of the migrant population?

Mr. IRWIN. We do make some allocations actually based on tabulation. In making our population estimates, I have talked all about projections—the population estimates for areas generally use some of the same techniques; and in the case of making our national estimates by age, sex, and color, we start from the base population, add registered births, subtract registered deaths by age, add immigration as computed by our various methods, and in the process we have the Armed Forces. We get statistics from the Armed Forces which are based on samples that are different for each branch of the service, but we use them as one unit and this shows preservice residence of each member of the Armed Forces. Then we have to allocate them to their former home or assume where they are going to move, after being discharged. They are all estimates and are all pretty tricky. For example, I can get distribution of Armed Forces by branch for the Nation, but I don't know what it is for Alamogordo, Randolph Field, or Fort Benning. I know a group of young people are at Fort Benning for basic training but that's all I know, so when you get down to a , there is a problem.

Major MARTINO. I would like to return again to my previous thesis. You posed a sequence of models of improved nature, the first being a net migration model, net immigration, where you used, as I understood it, a proportionality factor—the net immigration being proportional to what was already there and you have shown that this had an inherent bias in the computation. This is a problem in the mathematics which has nothing to do with reality. Next you go to a modified migration stream method which again has an inherent difficulty in the mathematics because it assigns a constant proportion of the migration pool to certain States. This is biased against the rapidly growing States and for the lesser rapidly growing States and so on. I am suggesting in effect two things: You haven't gone far enough in this trend away from the net migration model and, barring difficulties of gathering the data which may be your biggest problem, what you can move to next is to recognize that this thing is not much different from a Markov Process. You actually have a 50 by 50 matrix of transition probabilities and the probability of a person in State A going to State B is an entry in each cell. The matrix is not triangular because above the diagonal you have from A to B and below the diagonal you have from B to A. The sum of the two, the net sum, must be the net probability of going from A to B. The point, however, is this would eliminate the difficulties both with the net migration model and with this peculiar allocation process of the migration pool model in that it creates no mathematical artifacts in your modeling, and assuming you get the data, it allows you to go directly to the growth rates of each individual state.

Mr. IRWIN. A very good point and I think I ought to meet it. I'm with you, but I'll go against you just for the record. First, I like the assumption that might be argued on a philosophical basis—I find the Bureau feels that we should assume a convergence toward a national unity in all of these variables. For example, births: We don't compute births separately for each State, we compute a ratio between the fertility rate per State A in the Nation and State B in the Nation, then we assume those ratios are going to be 1 at a point 50 years in the future. This illustrates the turn of mind. In the same sense, using a constant set of proportions will, in a period of time, produce zero net migration because sooner or later it will even out. We like it and we argue that the differentials which exist at the beginning of the projection period will tend to eliminate themselves as time proceeds. We don't know what the differential factors are going to be. They are going to enter into the future, so we won't try to estimate them —we like the convergence. If I don't like the convergence, it is extremely easy to use this model and not have convergence. All I have to do is compute inmigration as rates, build up a fictitious inmigration pool for each projection period, compute the proportions to the larger inmigration pool and then allocate, using those proportions. The large States will gain, too, and I can assume no convergence. The basic data is important because it is hard enough to come up with an estimate about the impact of special populations on this model—I'm not very good at Markov chains—but, it seems to me, it would be rougher to get into a place to place migration stream model although in my paper I do maintain and I will maintain it honestly and vigorously that we should explore this, it is obviously a direction that research should take, no disagreement.

Dr. LINSTONE. Do you take into account the technological? You mentioned the economic factor. How are technological factors like the oral contraceptive business?

Mr. IRWIN. Speaking of employment first, we are looking at other people's studies of employment and labor force. On births, there is a lot of work being done on fertility, fertility control, the impact of various methods of contraceptives on fertility. Work is being done in the colleges which we are studying. We have not explicitly built it into a population model, we are, however, keeping abreast of the research in this field. My feeling is the impact of the oral contraceptive on the birth rate at the present time is somewhat overrated. The fact that fertility got to the lowest period ever experienced in this country in 1933 without one is proof. If women decide they don't want children, they managed to prevent it although I don't deny the factor and obviously it is a factor. Contraceptives will have an impact but not a decisive one.

Dr. CHACKO. Following up Major Martino's remarks, your last statement about the relationship between economic and demographic factors brings up the similarity of forecasting techniques that you use in demography and elsewhere. I think I would take issue with Major Martino in using the Markov Process. In order to use the Markov Process, you need a transition matrix which has constant probabilities. The assumption that the probabilities are constant with reference to migration is too restrictive. I suggest you look at Leontief's Input-Output Table. It would be a 50 x 50 table. You can derive the technical coefficients for inmigration into California from other States. You can apply this coefficient, say 0.35 to the next period and see how close it is to the actual inmigration. Because the table requires that arithmetically the inputs should equal the outputs, you force the net migration to be zero. I am mentioning this as one technique. I didn't hear you refer to any logistic curves. Have you given up using these?

Mr. IRWIN. Well, pretty much. Just the same as our low projection of population in 1946 wiped out the period fertility and let us do so much work on that same technique; in the early 1900's some people named Pearl and Reed developed a complicated curve. This has been completely dropped. Again there and then, the fact that the fertility doesn't seem to line up with major economic shifts is another thing that troubles us. But I think it may begin again. Who knows?

Dr. CHACKO. In light of what Dr. Linstone has stated, I was wondering if you had considered the use of the exponential smoothing. What it does basically is take and weight the data differently, giving say 80 percent of weight to the last data and maybe 20 percent to all the previous period data. You would get a continued extrapolation which takes into account all the variance of the data. Brown at Arthur D. Little developed this program. Its advantage is that if you have sudden changes such as the one mentioned—the use of oral contraceptives—the smoothing program will reflect the latest and drastic changes. I have used this successfully in forecasting the growth of chemical products which are quite volatile, starting slow and zooming up. This has been found to be useful. Have you considered that?

Mr. IRWIN. We have talked about it and we have a great need for good shortrun projection. I want to figure this summary for foreign demography in the sense that I want a publishable figure for January 1, 1968. Well, it's a little embarrassing for us. We have a rather crude extrapolation which goes into the census for 1970. We don't have any consistent way of forecasting, say, 3 months or 6 months ahead. We do feel we should do that. This is a very good suggestion and we are interested in this line of reasoning.

Major MARTINO. I would like to add to Mr. Linstone's comment regarding technological impact. Do you attempt to forecast area population or could you use technological impact, in such things as replacement of coal by oil as an energy source and use this to conclude that the population of West Virginia is going to drop?

Mr. IRWIN. I am sure the Bureau of Census would feel that is a function of other analysts. We will use the research that other people do both public and private. I would ask about the idea I mentioned in the model which was outlined. In other words, give me an estimate of employment: I am going to let other people decide how you do it. I've got enough complexities of my own. I don't know if that is the right attitude but I feel we are in an age of too much specialization. The feeling is that we will try to coordinate our efforts with other people who are doing that.

Mr. CETRON. Going back to Major Martino's point again, where is it similar to the forecasting techniques that are being utilized now and I think what we are using now is extrapolation basically with all that it entails. If everything goes on as it did in the past, it will be fine in the future. We should be looking at this migration figure using Dynamic Forecasting Techniques, and use these trends for efficiency where the weights are put on. That's what Joe and Dr. Chacko are trying to get at.

Mr. IRWIN. This is a very interesting point because I have discussed it with various people in the Bureau and one thing we have noticed: Demographers as a general group feel rather guilty about the fact that we are just projecting those rates out, so we have a guilt complex; but the interesting fact is that the people who are forecasting the economy are usually doing the same thing, so what happens is when you equate these things, they are not as different as you might think.

Mr. CETRON. Yes, but that doesn't mean two wrongs make a right. You are checking your standard—if the standard is wrong, then they are both incorrect.

Mr. IRWIN. You are right. I would restate by saying, until I see an economic model is going to do it better, I'm saying, "Look, give me an estimate of employment and I will tie my projection to it but that estimate of employment has got to be more dynamically oriented than mine."

Mr. HACKE. This is really a question for information. I have the impression and it may be simply that I've been brainwashed, when you go to a smaller aggregate, neighborhood, community or metropolis, that the people in the utilities planning do a very worthwhile job of demographic forecasting. I want to know first, do you believe this and second, is there a microstructure of demographic forecasting for people who move where there are utilities and therefore fulfilling a prophecy.

Mr. IRWIN. By utilities, what do you mean?

Mr. HACKE. Sewage, water, lights.

Mr. IRWIN. I think the building contractors do that, as to where you put the houses, to the extent that determines where the people go; I think its very true. With respect to local projecting, from the national point of view, I can represent both positions because I've only been with the Bureau a year. I worked for Oregon and California and there is a lot of feeling on the part of a local person that he can better assess the individual potential and factors of his area. I do feel at the Federal level one great danger is, you lose contact with the local things. For example, if I were to retain the same kind of contact with each State, making State projections, it would take a huge travel budget which I'd never get one percent of. There is the danger. On the other hand, there is a kind of doubt that these things are not independent; when you come at it from the top—for instance, this gentleman mentioned finding a way to make a good economic employment projection, starting with an input-output projection in the national picture for 1970 or 1975, manage to work this down to the States by a good allocation system and then plug this into my demographic model and get it all lined up. On the average, we are going to do better than these areas would do alone. "On the average" is a weasel word. It reminds me of the only joke I know about statistics. A statistician is a man who when he comes in the room and finds a fellow with his feet in the icebox and his head in the oven, says, "On the average, you are very comfortable."

Dr. North has applied the Delphi procedure, described earlier by Dr. Dalkey, to the task of preparing plans for his company. His description of how the Delphi procedure worked in a practical situation should be of interest to those contemplating the use of it.

—Editor

TECHNOLOGICAL FORECASTING AND ITS ROLE IN PLANNING

Harper Q. North, Research and Development, TRW Inc.

I first first met Joe Martino at a conference at Lake Placid, N. Y., on May 23 sponsored by the Industrial Management Center of the Harvard business school. Joe invited my participation in this conference, and I still feel, as I felt at that time, that the setting of corporate goals in any company and the establishment of a sound research and development program constitutes a chicken-and-egg situation. Today, I want to elaborate upon that statement and to describe a program in which I—and more recently my peers and superiors—have expressed considerable interest and to which my office is devoting considerable time. I hope that you will forgive any flavor of advertising in the event that I become carried away and mention TRW. Any reference to a specific company living or dead—and TRW is certainly living—will be in the interest of making real an exercise which might otherwise be classified as pure theory.

Those of you who attended the Lake Placid conference may sleep through the first half of my talk, as I plan to start from the beginning of our technological planning, if only to use some of the excess time which Joe Martino has allocated to me. In fact, many of you will recognize our exercise as a variation and extension of a study conducted by Major Martino and his group in the Office of Scientific Research. Their study predicted events in politics, economics, and the military. Ours is confined to technology.

Late in 1965, my assistant, Don Pyke, called my attention to the now well-known article in "News Front" for April of that year by Olaf Helmer of Rand and Ted Gordon of Douglas entitled, "Probing the Future." I was quickly persuaded that the Delphi technique could readily be adapted to research and development planning, but persuading even an imaginative president and chairman of TRW of the value of the project required a little strategy. My entré was through Dr. Simon Ramo, longtime friend and associate and vice chairman of TRW's board. On the human side, Si Ramo is a friend but devil's advocate. He will pose probing questions one after another until the proponent is ready to say, Forget it! but just short of that, he will say, Let's try it. So, tentative plans to probe TRW's future were presented to about 200 of our key technical people at dinner meetings in Los Angeles and Cleveland. The response before Los Angeles engineers of the systems group was encouraging but reserved. In Cleveland, the experienced and conservative automotive engineers were merely reserved, but we had enough to start on.

I asked the executive vice presidents of our four TRW groups: Automotive, electronics, aircraft equipment, and systems for five of their most imaginative but practical engineers and scientists to serve as panelists for an exercise in the Delphi technique of predicting the technological future with which TRW would have to cope in the 20 years ahead.

During the short initial meeting with each of the group of experts (whose spare time had been promised), Don Pyke and I established the following ground rules:

1. Each man was asked to list—independently—those technical events which he felt might take place between 1966 and 1985 and which would have a substantial impact upon the current or potential product lines of his group.
2. Each panelist was then asked to indicate, for each event, a date by which he felt there was a 50-percent probability that the event would have occurred.
3. He was invited to consult freely with anyone throughout the company, but his list of events would contain only those which he thought would occur.
4. Although we recognized at that time that the environment of the future would have a major impact upon the directions which technology would take, we had no accepted or official picture of that environment to offer. We did supply each panel member with what scanty data we had at hand, including a McGraw-Hill economic forecast, the results of the Helmer-Gordon study, a "Time" magazine essay on the future, and a "U.S. News and World Report" article blueprinting the future United States. Beyond this, each person was

advised to make his own assumptions concerning the future environment, excluding only all-out nuclear war.

About a month was allowed for the return of predictions from all panelists. Event descriptions were edited, combined, and grouped according to technologies, and a cross-indexed first draft was returned to each panelist with the request that he edit freely in areas of his own expertise (1) defining events more precisely, (2) deleting, adding or otherwise modifying event descriptions, and (3) indicating his own estimated date of occurrence in case of disagreement with the dates predicted. At this point, we should have known better than to furnish dates, offering the opportunity to concur or disagree. Human nature being what it is resulted in concurrence much more often than disagreement, which requires careful deliberation.

Comments of the panelists were carefully considered, combined where appropriate, reworded as necessary, and listed under 15 categories chosen empirically to encompass all events. Events which could be included in more than one category were placed in that category through which the greatest impact upon TRW's future might be felt.

The result of our exercise was a 50-page booklet covering 401 events and entitled, "A Probe of TRW's Future/The Next 20 Years."

Figure IV–1 lists the 15 areas under which events were categorized. These relate to technologies, subsystems, and systems of interest to TRW.

By way of illustration, I am going to quickly cover figures which I presented to TRW's 1966 management conference. Figure IV–2 lists 6 of the events which we felt would have a significant impact upon our automotive group, which is the principal supplier of valves for the United States, and furnishes chassis parts for automobile manufacturers. Figure IV–3, IV–4, and IV–5 show the results for the other six groups. The lines indicate the spread of dates estimated by all panelists, and the median estimate is indicated by a dot. Where there is no spread, artificial concurrence due to laziness on the part of our panel members can probably be assumed.

In figure IV–3 are some of the predicted events which are certain to have an impact upon divisions in our electronics group. You will note that the nature of the events predicted is compatible with the concentration of this group upon components rather than upon systems.

Figure IV–4 illustrates selected events which will have a substantial impact upon our equipment group. This group, as you will recall, produces equipment primarily for the aircraft, but also for the aerospace and defense industries.

Figure IV–5 lists selected events which will have an impact upon our systems group. As you may know, this group cut its teeth on ballistic missile systems and then turned to spacecraft, participating in 90 percent of U.S. space launches. At its inception 14 years ago, sales were 100 percent software. They are now two-thirds hardware. Recent software activities (part of the one-third of sales remaining) include the study and design of such civil systems as hospital systems, high-speed ground transportation systems, economic land-use studies, etc.

Upon completion of Probe I, the exercise had caused 27 of TRW's most creative engineers and scientists – and the additional technical people with whom they had talked—to extend their imaginations beyond the specific demanding tasks of the moment. Not much more had been accomplished.

In July of last year, I presented the results of our study at TRW's annual management conference in Vermont, using the figures which you have already seen. The response was encouraging—12 copies of the probe, which I had left on a table near the door, disappeared immediately. Since that time, more than 400 copies of our first probe have been distributed by request throughout the corporation. A sanitized version of the probe was prepared in response to requests from our public relations group and from people in finance as background material for discussions.

The climax in publicity came in an article which appeared in "Business Week" for May, 1967. In that article, we read in print for the first time that technological forecasting was a part of TRW's long-range planning effort. The probe had been launched as an official document for TRW.

Don Pyke and I knew of the shortcomings of the initial experiment:

1. There had been no formal use made of the probe in corporate or group planning.

2. Some very important areas were sparsely covered. These included microelectronics and automotive developments.

3. There were many inconsistencies. Events were predicted at dates which preceded other events that were necessary for their completion.

An exercise is currently in process which is designed to correct errors made in the first experiment. The results will be entitled "TRW's Probe II—A Guide to Growth Through Technology." Several improvements have been made in the method used for this second edition which will be more than fine tuning on Probe I.

PROBE OF TRW'S FUTURE CLASSIFICATION OF EVENTS

TECHNOLOGIES

1. ELECTRONICS
2. ELECTRO-OPTICS
3. MATERIALS (INCLUDING COATINGS, FUELS, PROPELLANTS, AND LUBRICANTS)
4. MECHANICS & HYDRAULICS

SUBSYSTEMS

1. PRIME MOVERS (INCLUDING MOTORS AND ATTITUDE CONTROL)
2. POWER: – SOURCES, CONVERSION, AND CONDITIONING
3. INFORMATION PROCESSING
4. INSTRUMENTATION & CONTROLS

SYSTEMS

1. TRANSPORTATION
2. DEFENSE
3. AEROSPACE (INCLUDING WEAPONS)
4. OCEAN
5. PERSONAL AND MEDICAL
6. URBAN AND INTERNATIONAL
7. MATERIALS PROCESSING & HANDLING PLUS AUTOMATION

FIGURE IV-1.

1. MICROELECTRONIC TECHNIQUES WILL BE WIDELY USED IN AUTOMOTIVE APPLICATIONS SUCH AS VOLTAGE REGULATORS, RADIOS, HEADLIGHT DIMMERS, CLIMATE CONTROLS, ETC.
2. AN ULTRA-LIGHT, HIGH-TORQUE, COMPACT, LOW-HORSEPOWER, ECONOMICAL HYDRAULIC MOTOR FOR AUTOMOTIVE ACCESSORIES WILL BE AVAILABLE.
3. GAS TURBINES WILL BE WIDELY USED IN TRUCKS, OFF-HIGHWAY, AND OTHER HEAVY-DUTY EQUIPMENT - i.e., APPLICATIONS REQUIRING IN EXCESS OF 500 HP.
4. HIGH FIELD MAGNETIC BEARINGS FOR POWER EQUIPMENT WILL RESULT IN GREATLY INCREASED RELIABILITY. THEY WILL BE AVAILABLE FOR ENGINES AND MOTORS, ESPECIALLY FOR HIGH ROTATING SPEED APPLICATIONS.
5. NEW PERSONAL AND MASS TRANSPORTATION VEHICLES POWERED BY BATTERY-ELECTRIC SYSTEMS WILL BE IN COMMON USE.
6. A HYDROCARBON/AIR FUEL CELL WILL BE:
   - A. AVAILABLE COMMERCIALLY
   - B. INTRODUCED IN AUTOMOBILES
   - C. WIDELY USED FOR MOBILE POWER

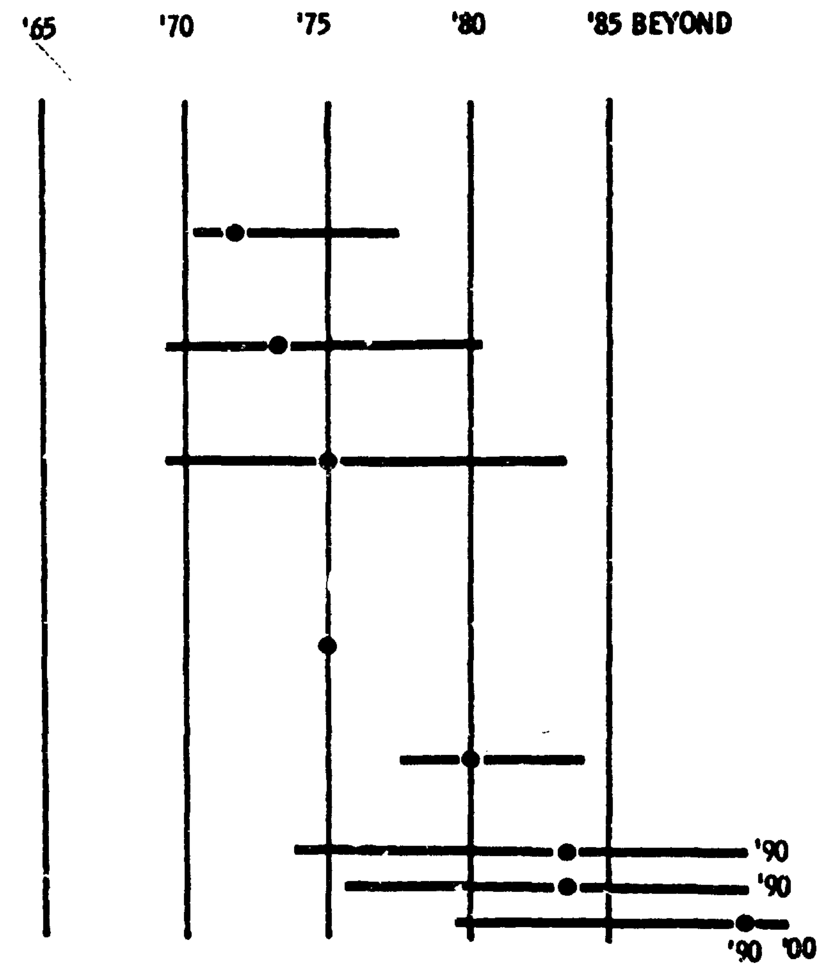


FIGURE IV-2.—*Automotive.*

1. SOLID STATE DEVICES WILL HAVE BEEN DEVELOPED FOR POWER GENERATION AT S-BAND
   - A. 10 WATTS
   - B. 100 WATTS
   - C. 1,000 WATTS
   - D. 10,000 WATTS
2. THIN FILM INTEGRATED CIRCUITS WHICH CONTAIN ACTIVE ELEMENTS WILL BE
   - A. INTRODUCED FOR MILITARY AND INDUSTRIAL APPLICATIONS
   - B. INTRODUCED FOR CONSUMER APPLICATIONS
   - C. USED WIDELY FOR INDUSTRIAL APPLICATIONS
   - D. USED WIDELY FOR CONSUMER APPLICATIONS
3. A SOLID STATE ACTIVE FILTER (OTHER THAN PIEZO) WHICH PASSES A DISCRETE BAND OF FREQUENCIES WILL BE IN PRACTICAL USE.
4. FURTHER IMPROVEMENT (SPEED, RELIABILITY, COST, ETC.) IN CIRCUITS MAY BE EXPECTED THROUGH "MOLECULAR ELECTRONICS" EMPLOYING DISTRIBUTED ELEMENTS.
5. ALL TV CIRCUITRY WILL BE MICROELECTRONIC.
6. INTEGRATED CIRCUITS FOR MOST (70%) APPLICATIONS WILL BE DESIGNED BY COMPUTERS.

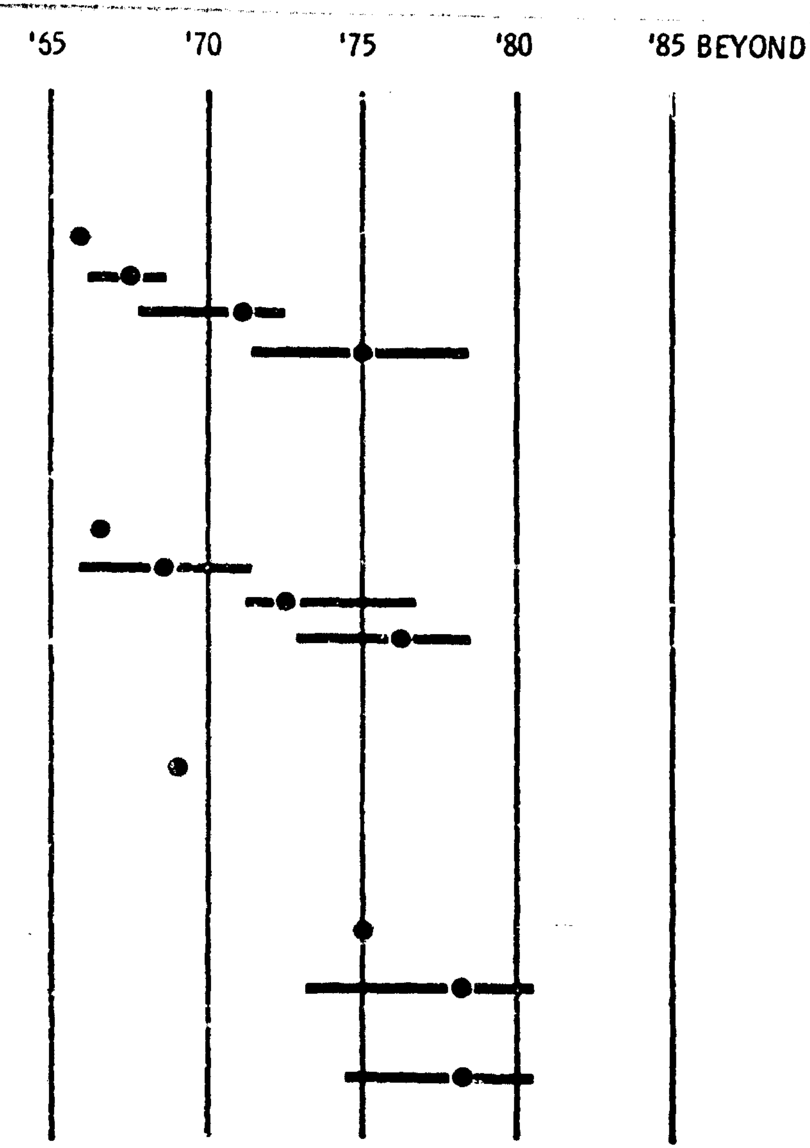


FIGURE IV-3.—*Electronics.*

1. THE SIZES OF PRECISION CASTINGS AND PRECISION FORGINGS WILL CONTINUE TO INCREASE AND WILL BE VERY LARGE COMPARED TO TODAY'S PRODUCT.

2. THERE WILL BE SUBSTANTIAL USE OF COMPOSITE MATERIALS EMPLOYING "WHISKER" TECHNOLOGY IN GAS TURBINES AND JET ENGINE AIRFOILS.

3. "RED HOT" DIES WILL BE USED TO MAKE FORGINGS THAT ARE LARGE, THIN-WALLED, AND FREE FROM CONVENTIONAL FLASH AND PARTING LINES.

4. A FRICTIONLESS TURBINE WITH ROTOR SUSPENDED IN THE WORKING FLUID WILL HAVE BEEN DEVELOPED.

5. COATED REFRACTORIES WILL REPLACE COOLED TURBINES IN AIRCRAFT GAS TURBINE ENGINES.

6. THE FOOT SOLDIER WILL BE COMPLETELY EQUIPPED WITH THROWAWAY WEAPONS.

7. THE FIRST CONTROLLED THERMONUCLEAR (FUSION) POWER PLANT WILL BE DEMONSTRATED.

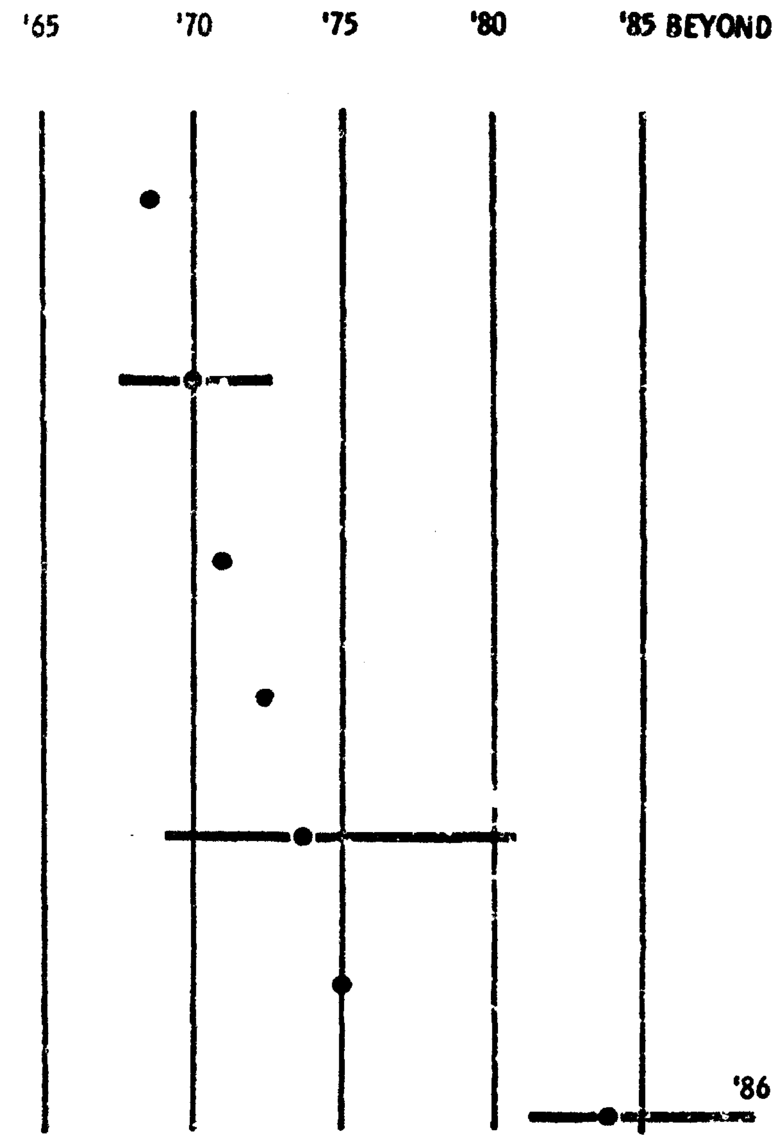


FIGURE IV-4. Equipment.

1. PHOTOGRAPHIC TECHNIQUES UTILIZING PULSED LIGHT SOURCES WITH GATED CAMERAS WILL BE USED IN (A) PHOTOGRAPHING OBJECTS OBSCURED BY A SCREEN OF SCATTERING LAYERS, AND (B) OBSERVATION FROM THE SURFACE OF PERSONNEL AND EQUIPMENT WORKING UNDER WATER.

2. LARGE-THRUST, AIR-AUGMENTED MANNED RECOVERABLE BOOSTER ROCKETS (AEROSPACE PLANES) WILL BE INTRODUCED ACCORDING TO THE FOLLOWING SCHEDULE:

   A. DEVELOPMENT WILL HAVE BEGUN
   B. DEVELOPMENT WILL HAVE BEEN COMPLETED

3. THE FIRST (UNMANNED) SPACE FLIGHT OF A LARGE-THRUST NUCLEAR FISSION-POWERED ROCKET WILL BE LAUNCHED.

4. A CAPABILITY WILL EXIST FOR INTERCEPTION AND INSPECTION OF ENEMY EARTH SATELLITES BY MANNED INTERCEPTORS.

5. THE FIRST MARS MANNED FLY-AROUND WILL TAKE PLACE.

6. A SOLID STATE ACCELEROMETER HAVING A VERY WIDE DYNAMIC RANGE (PERHAPS 10^{12}) AND A "PERFECT" SOLID STATE GYRO WILL BE AVAILABLE.

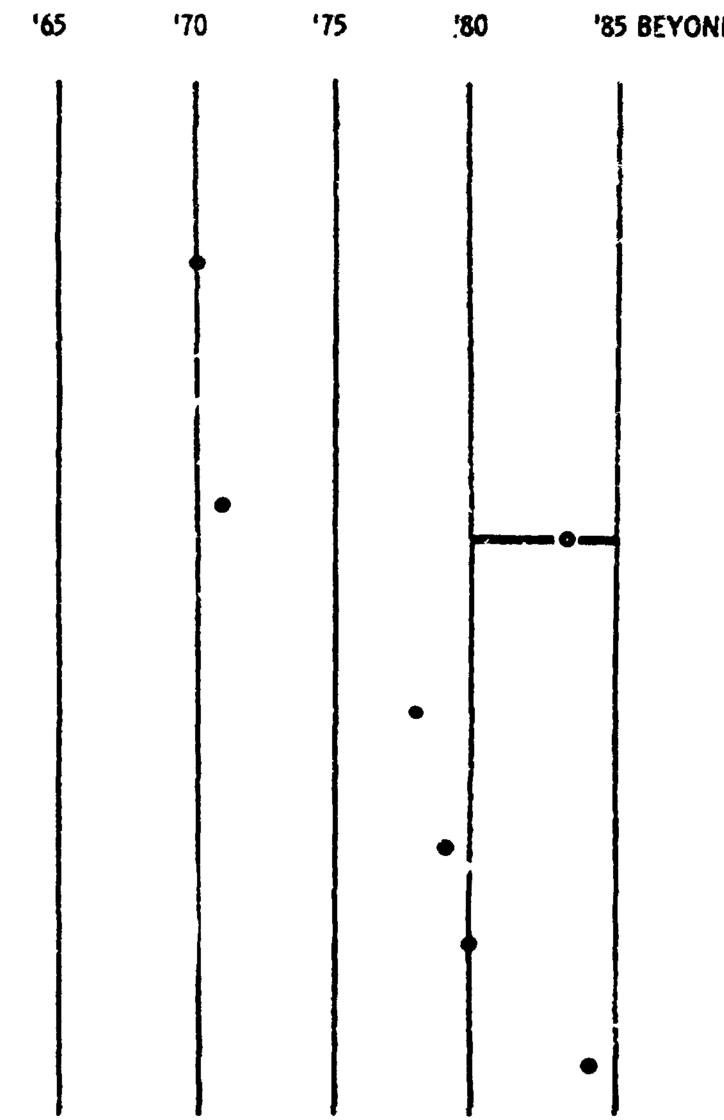


FIGURE IV-5.—*Systems.*

1. It assumes a socioeconomic environment left to the panelists in Probe I. That environment is the one assumed for TRW's 1975 Long-Range Plan.

2. It will focus on areas of prime interest to TRW, however romantic and sensational other possible speculations may seem.

3. Specialists will be concentrating on their specialties.

4. It will be extended into formal planning as practical.

5. It will involve members of TRW's technical staff, and, I hasten to add, on a spare-time basis. Thinking of the kind that is required here is not done at the expense of current assignments. It is almost entirely the result of off-campus thinking by people who are already well acquainted with their fields.

For figure IV–6, matrix for Probe II, panelists have been chosen to make predictions in each of the 15 categories shown. The categories shown on the left were chosen as a minimum number into which we could fit the predictions of the panelists involved with Probe I. One or two categories have been added, and a few have been combined. The rest of the chart illustrates the fact that a matrix was used to identify areas of interest to each of TRW's divisions. Where such interest was indicated by group captains, one or more panelists were chosen from that division. By this method, we are hoping to involve each of TRW's divisions and to spread the word about Probe II, making it a part of the planning of every division in the corporation.

This time, we are asking more penetrating questions.

As we now plan it, Probe II will be conducted in two rounds plus a followup as required.

1. In round one, panel members have been asked to list their forecast events on the form shown in IV–7.

Each event is to be weighed in accordance with the factors shown. Desirability is to be considered from the viewpoint of the customer. Accordingly, this rating should reflect an estimate of the potential demand which would indicate the importance of the event from a marketing standpoint. Feasibility, on the other hand, is to be considered from the viewpoint of the producer. It should reflect an estimate of both the technical feasibility and the difficulty likely to be encountered in prerequisite developments. Timing should reflect both an estimate of the date by which the probability is 0.5 that the event will have occurred and the degree of uncertainty associated with that estimate; i.e., for that date by which there is a reasonable chance that the event may have occurred: $p=0.2$, and for that date by which the event is almost certain to have occurred: $p=0.9$.

2. In round two, each panel member will be provided with a list representing our composite of all predictions of his own panel plus those of other panels which also relate to his category. In this round, he will be asked to evaluate all events with respect to the same three factors as in round one. Self-consistency in the list, particularly with respect to the order of anticipated dates of occurrence, will be a primary objective.

3. In an informal third round, resolution of differences of opinion will be sought. Subsequent attempts to resolve such differences, if necessary, will be conducted on an individual basis.

Present plans call for the production of these items (all of which will be labeled "TRW Private"):

1. A series of monographs—one for each category—for distribution to panelists involved and to TRW employees concerned with a specific category. They will contain a list of all events developed during our probe which may have an impact upon TRW's operation in that particular category.

2. Probe II—a document similar to its predecessor (but more complete) in which each predicted event is listed only once under that category in which the impact upon TRW is likely to be the most significant. Probe II, as such, will be used principally by group executives and their planners. For the convenience of these people, predicted events will be indicated for group attention in columns marked for that purpose.

We have still to deal with the problem that major events to appear in Probe II will be of little use to planners unless subdivided into more manageable units. We found that Probe I was similar to a road map on which only the locations and names of large cities appeared. The predicted events were so all-consuming that TRW was unable to participate in them as a sole developer and financier. The roadmaps containing only large cities needed further development. My industrious assistant, Don Pyke, invented logic networks which plotted the roads and intermediate cities with the help of technical people acquainted with the areas in question. Having the plots of intermediate cities of importance, or developments prerequisite to the final events, engineers pointed out to Don that there were many fallout products which should be considered. I look upon these as interesting little villages surrounding the intermediate cities, and it is in just those villages that TRW will probably find its greatest rewards. It is there that the food is grown which may find its way to the big cities.

By this time, we had dubbed the charts with the highways, intermediate cities and villages, "SOON charts," the word being an acronym for Sequence of

TRW

PROBE II CATEGORIES		AUTOMOTIVE								ELECTRONICS						EQUIPMENT							SYSTEMS					
		MICHIGAN	MRC	ROSS GEAR	VALVE	REPLACEMENT	TP, LTD.	STAFF	RAMSEY	EL. COMPONENTS	SEMICONDUCTORS	CAPACITORS	UTC	ELECTRO. INSUL.	GLOBE IND.	EQUIP. LABS	ACCESSORIES	J & O	METALS	MAGNA	MARK 46	STAFF	E[illegible]D	PSD	SVD	SEID	SL	INSTRUMENTS
TECHNOLOGIES	ELECTRONICS & ELECTRO-OPTICS							X		X	X	X	X			X	X			X			X		X		X	X
	MATLS. (INCL. COATINGS, FUELS & LUBRICANTS)		X		X				X	X	X	X	X	X	X	X	X	X	X	X	X			X			X	
	MECHANICS & HYDRAULICS	X	X	X		X	X	X	X	X					X	X	X							X	X		X	X
	POWER-SOURCES, CONVERSION & CONDITIONING		X	·	X			X	X	X	X		X		X	X	X				X		X	X	X			X
	INFORMATION PROCESSING							X			X											X	X		X		X	
	INSTRUMENTATION & CONTROL		X					X			X					X	X	X		X	X			X	X		X	X
	MANUFACTURING PROCESSES	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	
SYSTEMS & SUBSYSTEMS	PLANT AUTOMATION-PRODUCTION & BUSINESS	X	X	X	X	X	X	X	X	X	X	X	X		X							X	X				X	
	TRANSPORTATION	X	X	X	X	X	X	X	X	X	X					X	X					X	X	X		X	X	
	DEFENSE & WEAPONS (EXCLUDE MISSILES)		X	X				X		X	X				X	X	X	X				X	X	X	X	X	X	
	AEROSPACE (INCLUDE MISSILES)		X					X		X	X	X	X	X	X	X	X	X	X			X	X	X	X	X	X	
	OCEANS									X	X					X				X	X	X	X	X		X	X	X
	PERSONAL & MEDICAL									X	X				X							X	X			X	X	X
	URBAN & INTERNATIONAL										X		X									X	X			X	X	
	ENVIRONMENTAL CONTROL	X	X					X			X				X	X							X	X	X		X	X

FIGURE IV-6.—*Matrix for Probe II.*

- 1st ROUND QUESTIONNAIRE -

PROBE CATEGORY______________ PANEL MEMBER ______________ TRW DIVISION ______________

1. LIST BELOW ALL ANTICIPATED TECHNICAL EVENTS (INDICATING SOURCE, IF EXTERNAL TO TRW) WHICH WILL HAVE A SIGNIFICANT EFFECT ON TRW IN THE ABOVE CATEGORY.

2. EVALUATE EACH PREDICTED EVENT WITH RESPECT TO THE THREE FACTORS AT THE RIGHT IN VIEW OF THE ANTICIPATED ENVIRONMENT.

DESIRABILITY			FEASIBILITY			TIMING		
						YEAR BY WHICH THE PROBABILITY IS x THAT THE EVENT WILL HAVE OCCURRED.		
NEEDED DESPERATELY	DESIRABLE	UNDESIRABLE BUT POSSIBLE	HIGHLY FEASIBLE	LIKELY	UNLIKELY BUT POSSIBLE	x =.20	x =.50	x =.90

Figure IV-7.—*A Guide to Growth Through Technology.*

Opportunities and Negatives for the company. We decided to plot logic networks and SOON charts for a couple of important examples. Our first covered the event, predicted to occur about 1972, "Holographic techniques, utilizing two or three color lasers for recording, and ordinary white light for viewing, will be used to produce 3–D color movies."

Figure IV–8 outlines the logic network for this event. We have tried to identify the sequence of developments which would have to precede the occurrence of our event. Those in the upper part of the chart relate to the development of lasers and pulsing technology. The middle band traces developments required in production techniques, while the lower part of the chart is concerned with projection for small- and large-screen viewing.

The next step as shown in figure IV–9 was to expand the network to include the logical spin-offs which might be expected by way of corollary developments. For example:

1. As a result of the development of techniques for achieving the desired intensity and coherence of lasers required to produce holographic movies, exploration under water with green lasers and pattern recognition technology will be enhanced; laser machining techniques will be improved; and complex synthesis of microcircuits may become more practical.

2. The development of appropriate pulsing techniques should open a number of new possibilities in stroboscopic applications; telemetry; and communications.

3. The development of recording media should lead to improvements in: information storage capabilities; filter and light technology; high-speed photography; and text illustrations.

4. The development of techniques for the protection of actors and viewers should contribute to defense against laser weapons.

5. One can't think of movies today without considering television, and a logical extension of the predicted event would suggest the possibility of 3–D color holographic TV in the home. Before this can happen, however, it would have to either (*a*) achieve the ability to transmit information at a rate of approximately 2.5×10^{12} bits per second, or (*b*) develop alternative techniques which would eliminate the need for transmission of redundant information. On this SOON chart, the "N", or negatives, are not identified as such. They are the threats to current product lines

At the company's Vermont conference in July of this year, I presented SOON charts related to developments required for the widespread availability of electric automobiles. The specific event was "New personal and mass transportation vehicles powered by battery-electric systems will be in common use by 1980." The negatives presented at that time were associated with the fact that electric automobiles (except hybrids) will use no valves. We are the leading producer of automobile valves in the United States. Thus, the electric automobile poses a threat in that area, as it does in the possible obsolescence of current suspension systems and steering mechanisms, which we produce in volume.

There is another step which can be taken in SOON charting, namely, the production of what we call Phase II SOON charts. These consist of the superposition of those portions of Phase I SOON charts related to a specific technology. This should help identify the key developments in that technology which are common to a number of final events, even though TRW may have no interest in those events, per se. Back to my analogy, though, the villages of interest are those whose produce finds its way to several cities.

It is too early to assess merits and shortcomings of Probe II. At the present time, however, we know that enthusiasm is high among some 140 technical people whom we have instructed not to neglect their day-to-day job commitments in the course of imagining the future. We don't think that this will occur. In fact, we look upon technological forecasting as a very healthy vision-extending exercise which will place in context the current activities of people engaged in the exercise.

In closing, I would be remiss if I were not to locate our current effort in the framework of a corporate planning effort. Figure IV–10 shows the location of our technological forecasting in the planning cycle, with a little art work thrown in to show that it is a chicken-and-egg proposition as claimed in the Lake Placid conference presentation. If I were to choose one egg which had to precede the other three, I would pick the one under the hen, entitled, "A description of probable environment of the future." From there we can complete the cycle clockwise.

A PROGRAM FOR THE DEVELOPMENT OF 3-D COLOR HOLOGRAPHIC MOVIES
LOGIC NETWORK

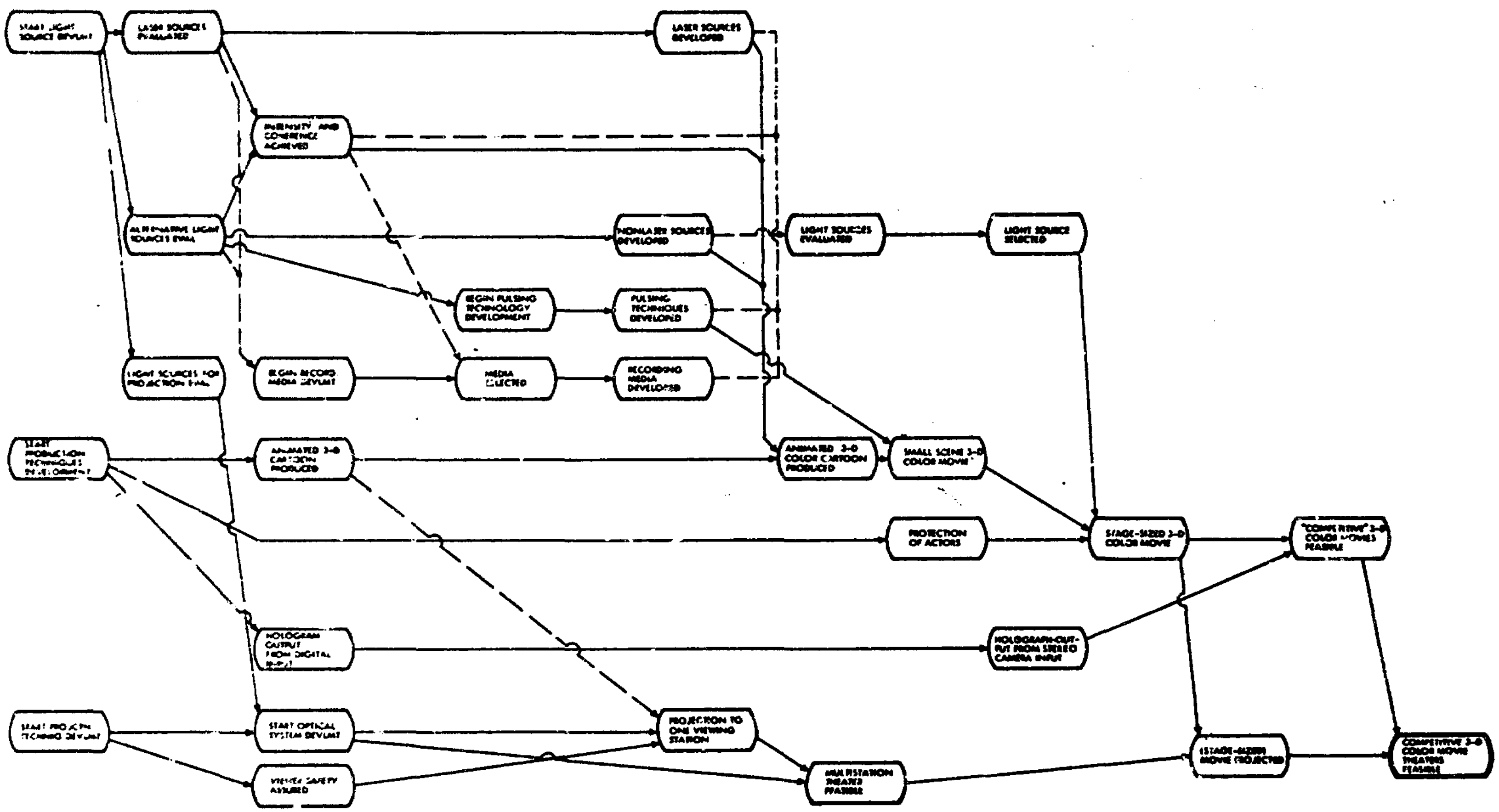


FIGURE IV-8.

A PROGRAM FOR THE DEVELOPMENT OF 3-D COLOR HOLOGRAPHIC MOVIES
WITH SOME PROBABLE COROLLARY DEVELOPMENTS INDICATED
LOGIC NETWORK

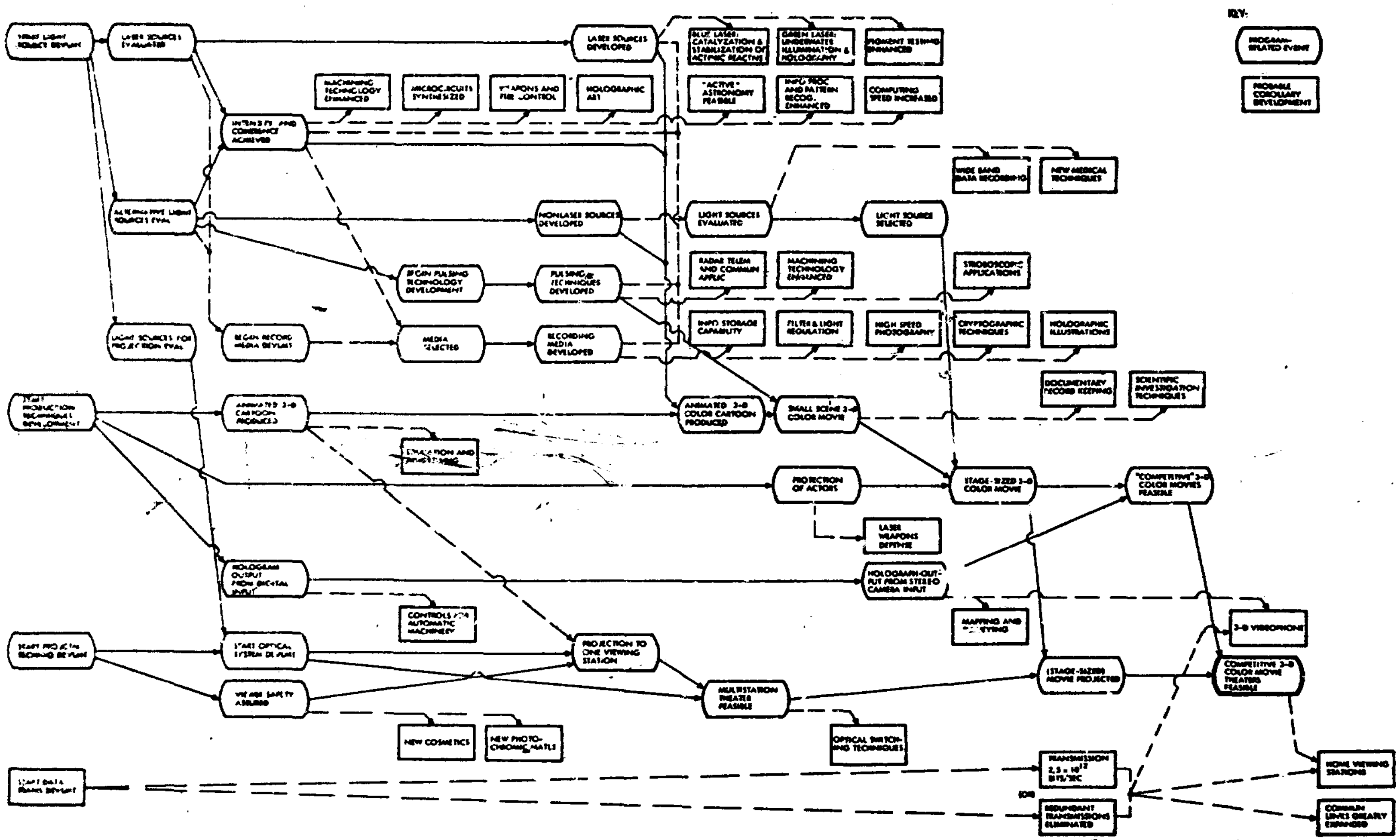


FIGURE IV-9.

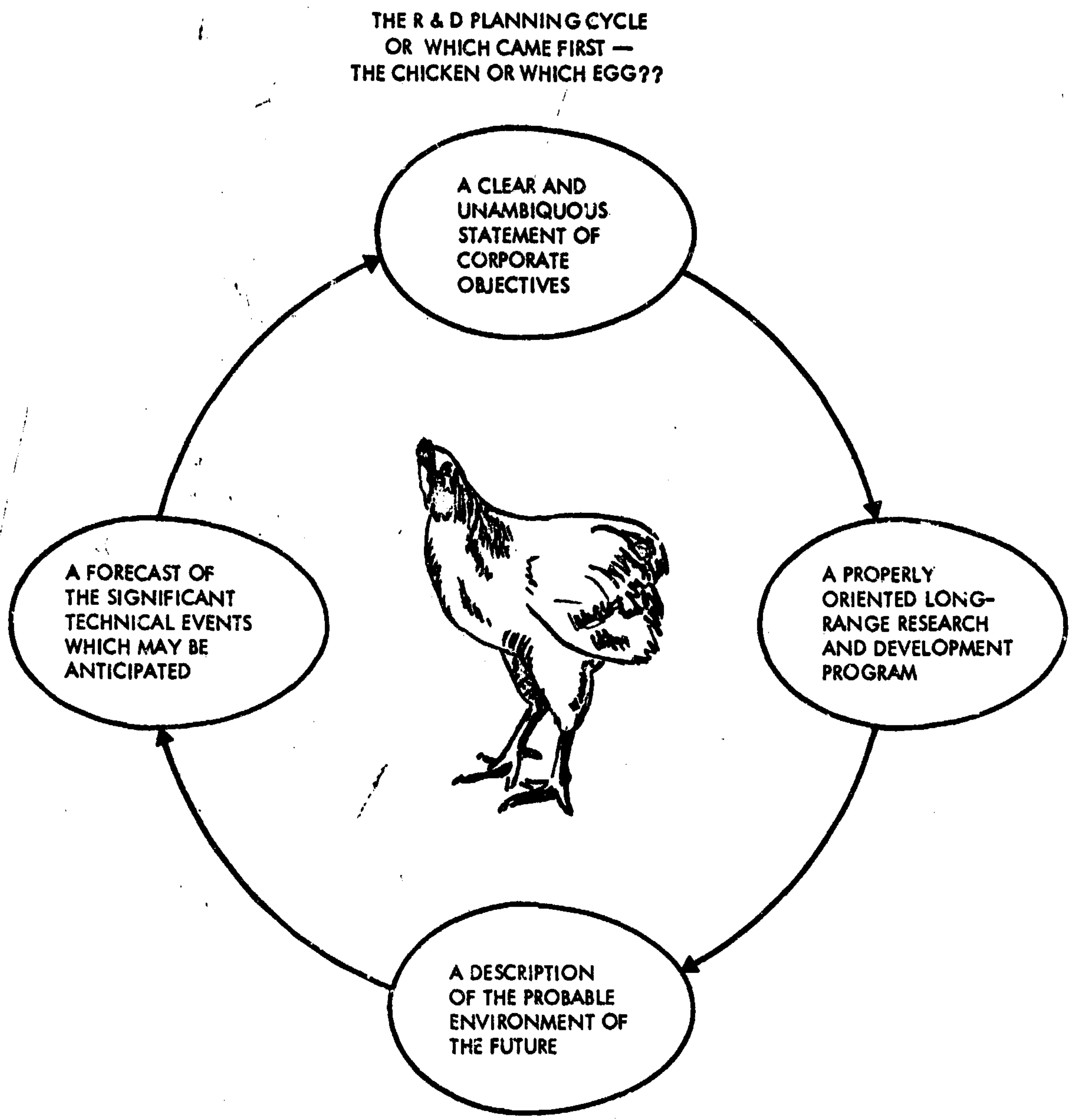


FIGURE IV-10.

Dr. LINSTONE. I am a little concerned about your statement essentially that the market analysis is done by engineers. This puts the engineers in a superman category on the entire job and particularly, of course, there are the nonengineering aspects of the environment, the market and so on. Engineers have been wrong in the past and I am a little hesitant to rely on them that much. The other point is that the entire approach is techoogy oriented. This is a technological forecast meeting so maybe you covered this part, the facilities for planning and personnel development as a mix. By 1980 you may need different mix of people, so you don't have to worry about this, corporate goals and the facilities among other things. I am wondering: Is this integrated and the other aspects being done in a compatible way so we can put this all together?

Dr. NORTH. I think we obviously have not done the whole job. First, let me talk about the use of engineers. I think even marketing people are being chosen from the ranks of technical people these days because of the large technological content. These fellows can do a pretty good job. Granted they have been wrong in the past and they can be again.

Dr. LINSTONE. I am less concerned about the marketing people than the economists and the sociologists.

Dr. NORTH. I think it is very important that we have it in there. I doubt whether the economists and the sociologists should make predictions with respect to the technical events. They certainly should feed into the background with respect to the socioeconomic environment. The other comments are certainly valid, that is, that more needs to go into decisions to proceed than just the technical factors. Our company is a little different. We are highly decentralized so that each of the four groups has its own problem and laws of responsibility and the screws are put down on them pretty hard to meet their goals. Therefore, before they would engage in anything—these are merely suggestions for them—but we are counting on them to do their own charting or use the information in any way they see fit. If they are not using it, we will complain and say, "You should be using this and here are some suggestions to guide you." It is going to be up to them to introduce such things as markets, availability of plant space, availability of engineers, plant workers, and what have you. We are not trying to put those factors into it. They will be parts of the business decisions made in the selection.

Mr. FRIEDMAN. Do you have any data, historical data primarily, upon which you can base the validity of your tech forecasting? For example, something you might have thought of 5 years ago and today stop and look at what happened?

Mr. NORTH. No, it is too young. This is only a 2-year-old project. I may not even want to come to the meeting next year because they do so poorly, but I don't think it's going to do that badly. We have seen within this 2 years some things change. All of them have been brought in closer, and all the changes we think should be made now. I think people have found in the past, who haven't done things this way, that they have been too conservative in their estimates. I have no background figures on it.

Mr. FRIEDMAN. Do you have any information concerning that which affected your forecast most, things which perhaps suggested what you have forecast is not exactly the rule? Was it economic within the corporation?

Dr. NORTH. You mean things that brought it in closer?

Mr. FRIEDMAN. Yes.

Dr. NORTH. No. The things I was referring to were the technological pace being greater than we thought. I mentioned in passing the microelectronics in television that costs have dropped much faster; prices have dropped much faster than I would have predicted. I think that is the all-encompassing factor.

Mr. FRIEDMAN. Have you come up against a situation where someone in the decisionmaking place in the organization simply decided they did not want to put money in a certain area which later turned out to be or could have been profitable?

Dr. NORTH. Yes, very definitely. Not in connection with a particular forecast, because it is just 2 years old, but I have known many people in the corporation who have forecast "that is going to be a very promising field." Only last week, one of them told me we would have in that area instead of about $50 million sales, $250 million sales and I think I believe him.

Major MARTINO. I believe the experiment with the use of the Delphi procedure you are now conducting is the biggest in history. I know of one which was run with a panel of a hundred people but youre is even more complex in that you have apparently a little over a hundred people divided into several panels. I am pleased to see that you are able to manage it with two professionals part time and some administrative support. This is very interesting from the standpoint of a number of other tests that I am aware of to check the feasibility and the useability of a Delphi method. Dr. Dalkey may have some other information on the size of the panels in the past, but to my knowledge this is the largest that's ever been run.

Dr. DALKEY. This is, I believe, the largest study both with respect to the number of participants and the complexity of the kinds of questions and action that are involved. I must say the most interesting statement you made is when you went on from Probe 1 to Probe 2. From the standpoint of this group, I think this is very significant: that you didn't find it expedient to quietly forget it after Probe 1.

Dr. NORTH. Some people criticized us by saying, "you haven't used Probe 1 yet. Why are you going to Probe 2?" My response was "I didn't feel that the foundation was sound enough because of the speed with which we put through the first one, and it was lacking in the most important areas where we needed it." I might add that I don't think we are going to have 140 people in the end. There are many people who just can't meet deadlines then we say "forget it," and if we end with a hundred, I'll be happy, a hundred good thoughtful inputs.

Dr. SLAFKOSHY. I couldn't help but agree heartily with Linstone, but in terms of economists and sociologists, I would like to pose the question very simply. Why did you only use engineers? I assume you have other types available, systems analysts, scientists, physicists, mathematicians, who were not engineers, who might make better mixes between what is now available in terms of basic, and applied research and would be available in the future. Engineers are notorious for doing things the way they were taught in school and doing the simplest things as a matter of fact. I am not as convinced that they are the ones who have the best knowledge of basic research and applied research in ascertaining and foreseeing the forefront. That is my question.

Dr. NORTH. First, I included scientists among engineers; scientists and engineers are together. Secondly, analysts in our company are engineers and there are a lot of system analysts included. We don't have many economists, per se, on our staff. There are a few and they contributed through the socioeconomic background that is a preamble to our 1975 plan which we are also using for this guide. I don't think we've put to bed all of the problems, or all the shortcomings.

Dr. DALKEY. I would like to ask a couple of questions about the timing of the exercise. In Probe 1 you wound up with categorization of events and with the logic network tying together those you did after the Delphi part of the response. As I understand it was done by a small group of analysts working with that data.

Dr. NORTH. The group is even smaller than that. The inputs we received from panelists were rather loose the first time; some were unbelievably loose, such as, there would be quite a few electric automobiles by this date. Don Pyke and I took it upon ourselves to change that to read whatever is read here. It would be a major factor to the industry and will go as far as to say that 53 percent will be electric, but we sharpened it a good deal and panelists in the second round did a further sharpening job. The next step was cross-index fashion so a man could look at the portion he was best equipped to handle— there were some 40 categories; not 15 as here. Then when we had all the results we said, "Isn't there a small number of categories in which we can put all of these; we're not going to change them, we're going to categorize them." Don and I sat down and worked up these first 15. We seemed to, by a little shoe-horning, get the comments in one of them and we talked to the people subsequent to this and they didn't see any way—before this (probe 2), we said, "Are there any categories that we need in here?" Yes, there were a couple to be added. As far as the logic network is concerned, that didn't come until Probe 1 was completed; the early part of this year. Probe 1 was completed last year.

Dr. DALKEY. Secondly, do you intend to include in Probe 2 any consideration on the part of the respondents on a logic network? Are you going to let that remain until after the responses?

Dr. NORTH. I think we will let it remain and not cause the panelists to call out their pet project but let the management exert some prerogative as to what they may or may not be interested in.

Mr. CETRON. You have a forecast now, of what would be feasible in technology. Now how do you determine what you're going to do, out of all these possibilities?

Dr. NORTH. I would say: that out of the total picture which will be indexed for groups, group planners will look it over and decide what they would like to detail first. We will give them help in detailing. I should have mentioned earlier I don't think these SOON charts are the only way of going about this, but people will have different ways of looking into this. We will give them help where they want it. If there are things they don't look at that they should have, we will do them ourselves. The planning of the functions of a group, company, or division is a function of opportunity as well. For instance, we hope that some thought would have been given before a person makes an acquisition, let's say, of a company that gets them into a whole new field. Is it really promising? Does it look promising? This should be used there, but I think it will be up to those people to use it as they see fit and we hope it will give them some guidance. That's just where it stands now: guidance to be used, if useful.

Mr. IRWIN. A little bit along that line. The chicken and egg was very interesting to me because a couple of times when I've gotten involved in getting the demographic population progression with an economic one. We were always severely pressed by the men who wanted to project economics for a population progression, so at the start we might base our own population on his. I'm not completely familiar with the procedures of the National Planning Association but they seem to solve this by going around the circle once or twice. My question would be: Is that a good way to handle it? It doesn't matter so much which part of the circle you started but maybe you ought to make a circuit or two.

Dr. NORTH. I think that's true. Any corporation that does this has pretty good ideas of what kinds of business they are going to be in, so the top one is filled and then you go around the circle or back up and realize you don't have enough foundation and then move back to that, and a lot of that amounts to going around the circle.

Dr. Linstone has pioneered in devising an approach to forecasting, known as Mirage, and has applied this technique iteratively over a series of years. With three completed Mirage forecasts behind him, he is in an excellent position to discuss the situation of a forecaster who has to live in the time period he once made forecasts for. His description of what he learned through these iterations of the Mirage procedure should be of value to all forecasters.

—Editor

THE MIRAGE STUDIES AND SOME IMPLICATIONS

Harold A. Linstone, Lockheed Aircraft Corp.

The request to discuss the Mirage studies fills me with considerable trepidation. Studies of this type are part science and part art. I fear that analysis of such studies is likely to be as superfluous for those who have the knack as it is inadequate for those who don't have it.

Byron Johnson of the University of Colorado tells the story of the philosopher troubled by a certain problem. In the years before psychedelic drugs become common those desiring to expand the consciousness of the mind were forced to rely on primitive substitutes—still in fairly wide use—containing alcohol. One philosopher imbibed freely of this primitive mind-expanding drug. As he sat in solemn, but unsober, reflection, he realized he had finally found the key to the problem which bothered him. Fearful that he might forget, when sober, what had been so clear to him when drunk, he wrote feverishly upon his tablet before turning to bed to sleep it off.

The next morning, as he pulled himself out of the fog, he realized something quite important had happened the night before, but he could not remember what it was. So, he struggled over from his bed to his desk and there he found, written in his own somewhat shaky scrawl, this pregnant sentence: "Think in other categories."

This is precisely the thrust of our effort in the Mirage series.

In a corporation, each division invariably undertakes planning studies of its own. There are always tactical plans which tend to be dominated by: (*a*) medium- and short-range projections, and (*b*) extrapolation of current lines of business or normal system improvement cycles.

Strategic planning may occur at both the division and corporate levels, or at the corporate level only, depending on the degree of decentralization of the organization and the size of the particular division. This planning requires a long-range perspective and must be concerned with means to achieve the growth and evolution set forth in the statement of objectives of the corporation.

Stimulation of imaginative interaction between new needs and new technology is one major element in this process. In other words, our thinking has "opened up". Such endeavors should help identify "good risks" and "poor risks" in a future environment characterized by great uncertainty.

The timing of the studies has been as follows:

	Performed	*Subject Period*
Mirage 70[(1)]	1959–60	1965–70
Mirage 75	1963–64	1970–75
Mirage 80	1965–66	1975–80

Although the studies exhibit differences in execution, their general approach and content is similar. The MIRAGE 75 study also include a supplementary "mission flow chart approach originally developed by the author at the Rand Corp. (B) This model was expanded in a study performed at the request of the U.S. Army Electronics Command in parallel with MIRAGE 80. (*3*)

It should be noted that Mirage 70 can be assessed today in hindsight: It concentrated on the period in which we are living today.

In commenting on these studies I shall view them in toto.

I. GENERAL FEATURES

A. *In-House Sponsorship.*

All studies have been done with private funding. The foremost advantage is that complete objectivity can be maintained in this manner. A disadvantage is the unavailability of much pertinent analysis data. Mirage 80 was a one year effort with an average of

15 technical people.[1] Obviously, a small organization has neither the financial nor manpower resources to undertake such a project on its own.

B. *Organization.*

The focal point of each study has been at the corporate level.

In the case of Mirage 80, the study was authorized by the president of our corporation and his staff at the annual long-range planning meeting in 1965 and reported at the same meeting in 1966.

Staffiing represents a major challenge and few general guidelines can be provided.

Youth is an advantage—but brilliant young Rhodes scholars are difficult to corral. In MiRAGE 70, the oldest team member—the author—was 35 years old. The seven-man group had worked as a unit at the corporate level for several years before tackling this project.

A disadvantage was the noninvolvement of the divisions, making implementation of recommendations difficult. For the larger Mirage 80 project, an ad hoc team was formed and individuals from several divisions participated. A small nucleus remained with the project throughout the year but most participants were with the study for a few months only. The group worked physically together for only a limited time. However, the deputy project leader, Dalimil Kybal, was located on the premises of the division furnishing the largest level of support. There are also significant advantages—and disadvantages—in using an ad hoc group.

Advantages

- communication with, hence implementation by, division is facilitated.
- little chance of staleness in succeeding versions of study.
- wider basis of technical support by informally tapping other expertise in divisions.

Disadvantages

- difficulty in developing a cohesive team (desirable individuals often unavailable).
- scattered physical location of participants.

C. *Contents*

The chapter headings of Mirage 80 are representative of the content of all studies.

I. Introduction
II. The World Environment of the Seventies
III. Basic Needs
IV. Strategic Lift
V. Tactical Operations
VI. Strategic Offense and Defense
VII. Conflict Management and Control
VIII. NASA
IX. Summary and Conclusions
X. Implications for Lockheed (separately bound volume for internal circulation only)

In addition, a total of 21 backup reports was produced.

It is evident that the studies are restricted to national security areas, i.e., DOD and NASA.

A schematic of the structure of the Mirage 80 study shows the logical relationships (figure V–1).

The study of the environment (ch. II) focuses on the widening gap as the most crucial issue for the 1970–80 period. This leads to an emphasis of problems at the extremes of the conflict spectrum:

(a) unsophisticated warfare involving the underdeveloped areas.

(b) sublimated technological warfare involving the superpowers.[2]

The search for new concepts then leads to the "development force" and "rapid action control and strike force" (ch. III).

The following four chapters (ch. IV–VII) consider the operational needs and new technological possibilities in the recognized military categories.

Chapter VIII takes up the NASA programs and the summary and conclusions form chapter IX. The stress is on good risks and on the crucial technological bottlenecks in satisfying the needs for the 1970–80 period.

[1] *This is by no means the largest in-house effort undertaken by a manufacturer. Minneapolis-Honeywell's Project Pattern, for example, included "20 full-time technical personnel . . . plus many outside consultants" at a cost of $500,000 ("Aviation Week," Dec. 28, 1964, p. 57).*

[2] *A superpower is defined as one having the capability of annihilating any other country.*

II. THE STUDY PROCEDURE

A. *The Work Plan.*

Figure V–2 shows the general work program of both Mirage 80 and the parallel mission flow chart approach.

A key feature was the development of hypotheses. These were potential conclusions hammered out in early roundtable discussions on the basis of the needs and technology interactions. They then formed the basis for analysis by small teams. Clearly, judgment had to be exercised at every juncture to maintain good balance in the choice and allocation of resources.

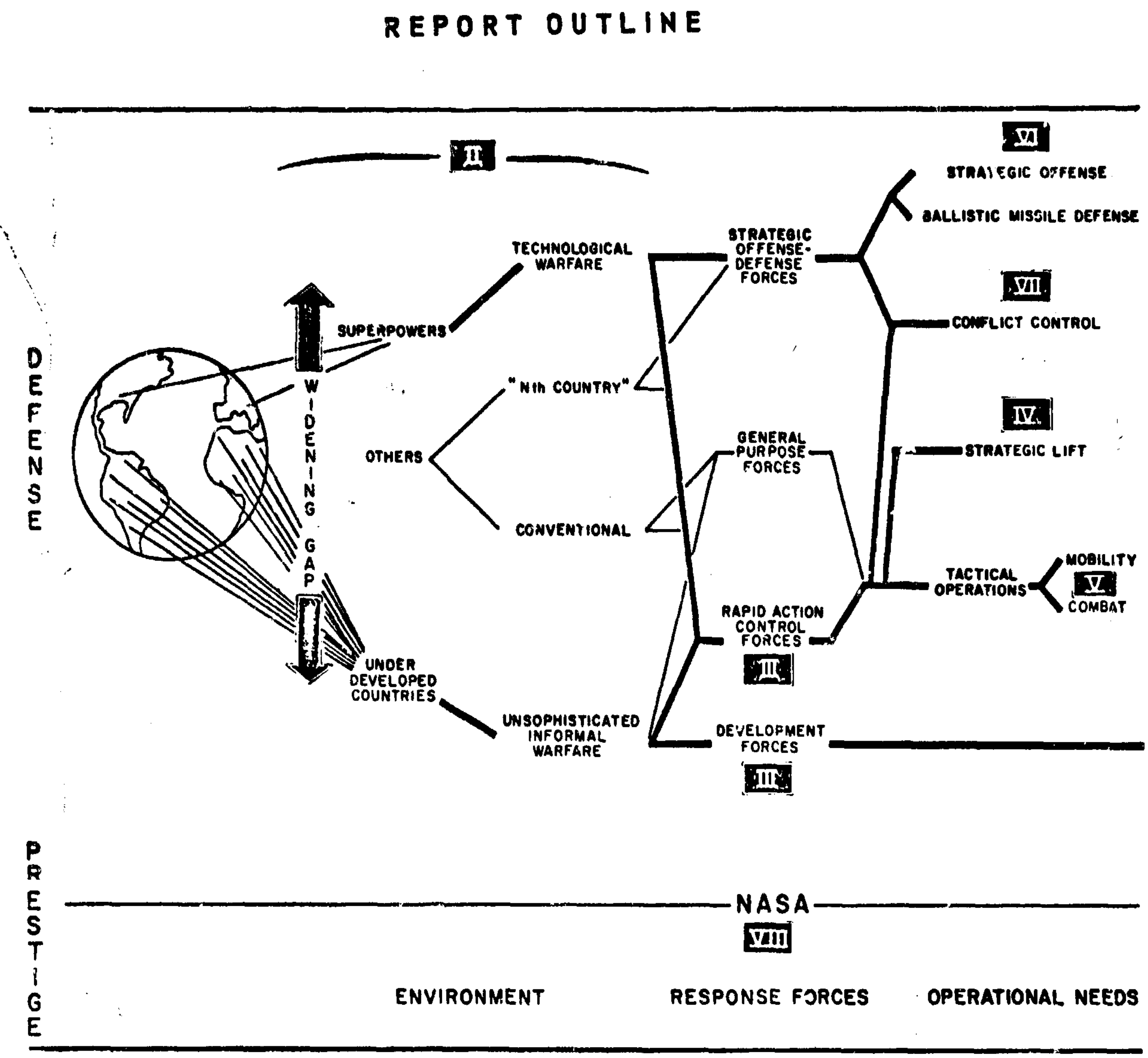


Figure V-1.

B. *The Analysis.*

There was no single formal model or methodology employed for the various hypotheses with exception of the supplementary mission flow chart analysis described below.

The reasons are twofold:

(1) no generally satisfactory model is known to us.

(2) formalized procedures tend to constrain free interaction between needs and technology.

The second point requires amplification. Formalized models cannot substitute for creativity. The morphological approach to new concept formulation, proposed many years ago by F. Zwicky of the California Institute of Technology, has proven disappointing.

War gaming is a fine tool for teaching, or for gathering data on, known tactics; it is rarely the source of new ideas. Formalization tends to impose a certain rigidity. Another problem with such models in technological forecasting and planning is the danger of self-delusion. Dubious input combined with superficially complex models may ultimately be more misleading than illuminating. Our point is not that all such approaches are useless; rather, a technological forecasting and/or planning effort should not be based on any one such model no matter how sophisticated it appears.

Thus, we observe no uniformity in the tools for analysis of the various hypotheses. A few examples from Mirage 80 will suffice to indicate the diversity of techniques.

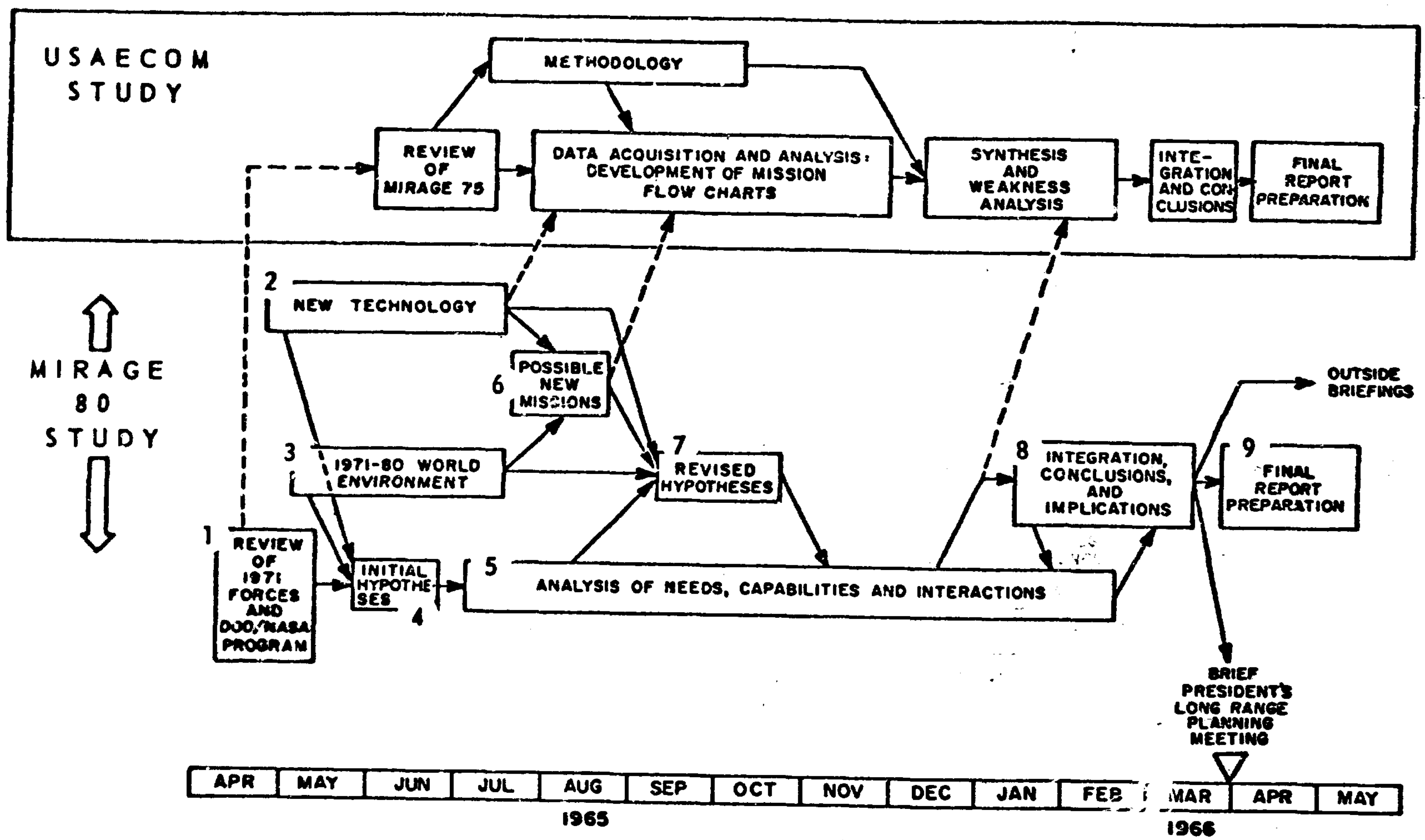


FIGURE V-2.—*Work Plan.*

Example 1: Basic Force Needs—New Concept Derivation.

The interaction of needs and technology in the analysis of scenarios and derivation of concepts of operation appropriate to the future environment is facilitated by stipulating a variety of artificial constraints. This is entirely analogous to the consideration of alternatives by President Kennedy's "kitchen" cabinet in the Cuban missile crisis. T. C. Soronsen reports (*4*) that the President was presented not merely with the conventional alternatives (1) do nothing, (2) invasion, and (3) air strike, but with an equal number of unorthodox courses of action, (4) diplomatic pressure, (5) secret approach to Castro, and (6) indirect military action (blockade).

The imposition of constraints is an effective technique to avoid the familiar more of the same response from the user and to elicit more imaginative concepts from the technologist.[3]

It was found in Mirage 80 that two capabilities, (*a*) rapid presence, and (*b*) control by neutralization without destruction, are central to many scenario-response combinations.

A list of neutralization concept candidates is shown in table V–1.

Example 2: Implications of a New Mission.

(*a*) A rapid reaction control capability of the kind mentioned in example 1 may prove useless if contingency planning cannot be performed with compatible speed. There is little need to consider a SAC-type strike force response speed if the revision of a contingency plan requires months. It is therefore necessary to study the automation of planning. Figure V–3 illustrates the result of one feasibility analysis. There is throughout a conscious attempt to avoid the trap of considering a new concept and ignoring the needs which it, in turn, generates.[4]

(*b*) Land barriers are indicated in table I as one of the neutralization concepts. Two avenues are pursued. First, a theoretical Lanchester-type analysis is performed to determine the relation between cost of troops in combination with a barrier and cost of troops alone, to maintain equal degrees of permeability along a certain line segment (see figs. V–4 and V–5). Second, the technology is explored and a rich lode of ideas is struck, suggesting entirely new systems possibilities.

It is noted that there was at the time (1965) no sign of the very visible interest in this subject which is currently displayed.

Example 3: Strategic Offense/Defense—An Option Matrix.

The problem of China as an *n*th country leads to a most difficult analysis problem. As Herman Kahn has pointed out, list making can be a useful tool even where quantitative analysis is not possible. Figure V–6 shows the matrix of *n*th country and U.S. alternatives. This

[3] *It is, for example, most unlikely that the U.S. response to a Vietnam-type conflict situation in the 1970's should—or will—resemble the current response.*

[4] *The search for interactions between new capabilities and new needs leads to other areas of vital concern, e.g., "hiding technology."*

TABLE V–1.—*Some Neutralization Concepts.*

Effect	*Means*
1. Military operational capability:	
Inhibit mobility	Create land barriers (see VA3), force down aircraft, incapacitate animals and vehicles (BW/CW), stop ship movement (e.g., antipropeller devices), jam navigation or guidance systems, and choke vehicular engines.
Prevent resupply	By above means, plus deep raids and guerrilla warfare.
Disrupt command and control capability.	Jam communications, kidnap leaders, and destroy radio equipment (e.g., CW against electrical wiring).
Isolate (e.g., separate two hostile forces).	Create land barriers (see VA3), jam communications, and quarantine.
Prevent covert operations.	Destroy natural cover by fire or other means and marking of personnel.
Weaken will to fight (including pacification).	Temporary incapacitation by nonlethal BW/CW (e.g., LSD and psychological warfare.
Capture or dismantle materiel (e.g., nuclear weapons).	Deep raids.
2. Population and governments:	
Disrupt governmental control.	Kidnap or assassinate leaders and guerrilla warfare.
Weaken will to fight.	See item 1, guerrilla activation, and communications control.
Weaken economy	Blockade and embargo.
3. Natural resources:	
Animal and fish	Alter ecology by local climate modification or introduction of new species, and alter breeding pattern.
Agriculture	Divert irrigation water temporarily.
Energy	Capture dams and incapacitate power lines, refineries.
Minerals	Disrupt mining without incurring permanent damage.

AUTOMATION OF MILITARY OPERATIONS PLANNING

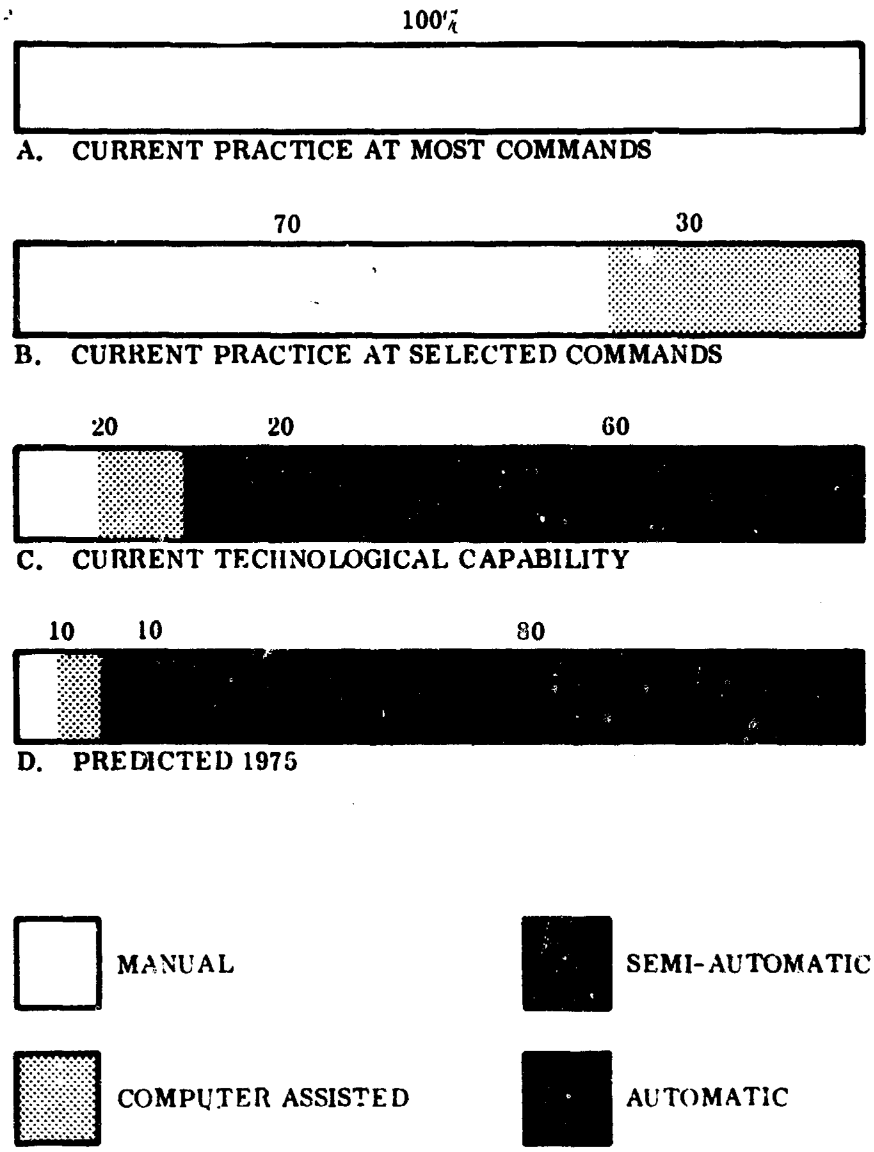


FIGURE V-3

matrix has so far defied solution. It has not been possible to determine a good way for the United States to divide any given budget among the available alternatives in light of the threat uncertainties, although it is evident that there are good and poor mixes of U.S. options.

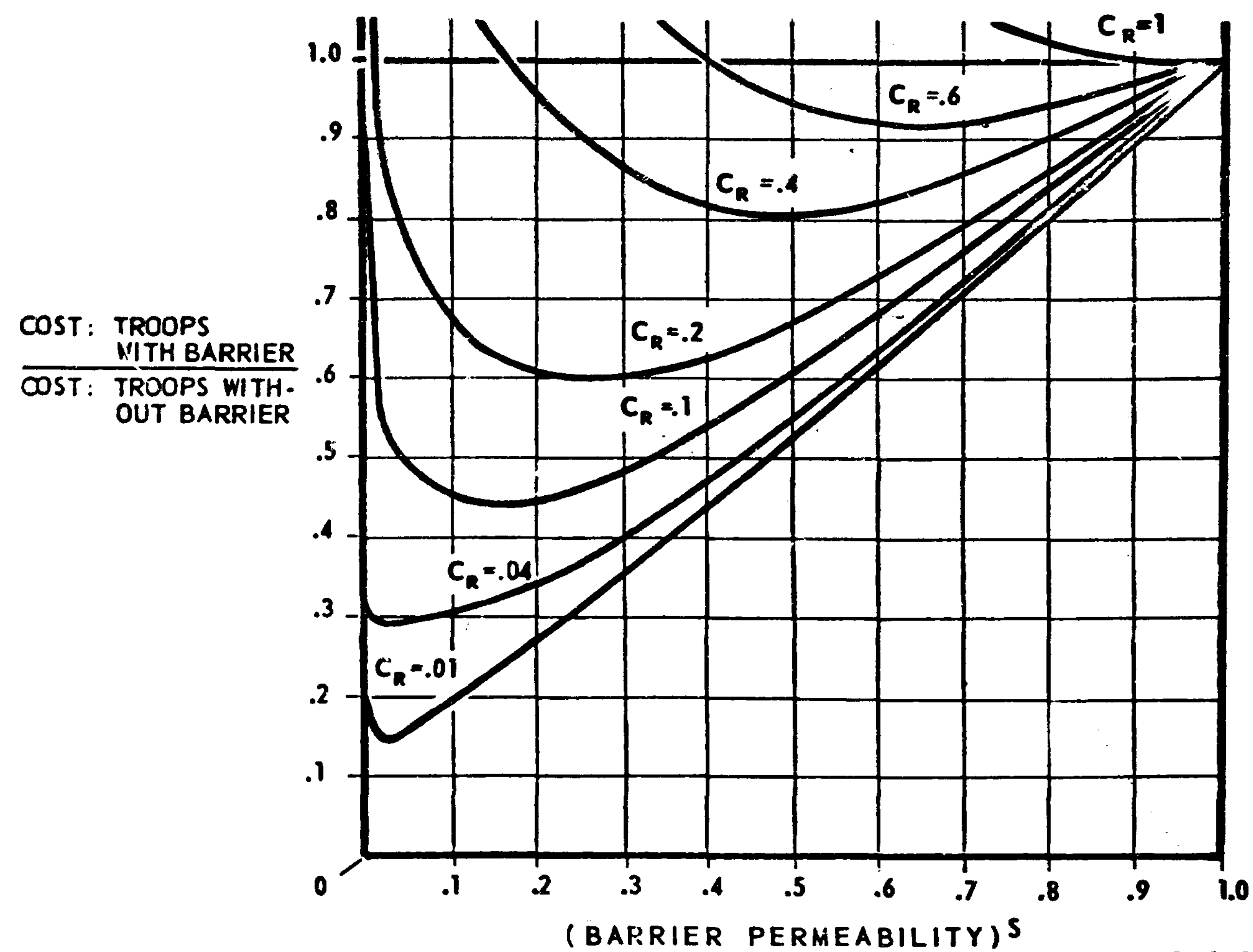
On the other hand, the United States-Soviet strategic offense/defense trade-offs to 1980 have been subjected to detailed quantitative analysis.

Example 4: Strategic Lift.

The steps in the analysis process in this case are as follows.

Step 1.—Review of Mirage 70 and 75 analyses and DOD program in this area.

GUERRILLA ATTACK THROUGH BARRIER - DEFENDING TROOPS USING AREA FIRE



(C_R is the cost-effectiveness ratio of troop element lethality to barrier element lethality. Barrier permeability defines the ratio of mobility through the barrier to mobility without the barrier.)

FIGURE V-4.

THEORETICAL BORDER SEALING OPERATION

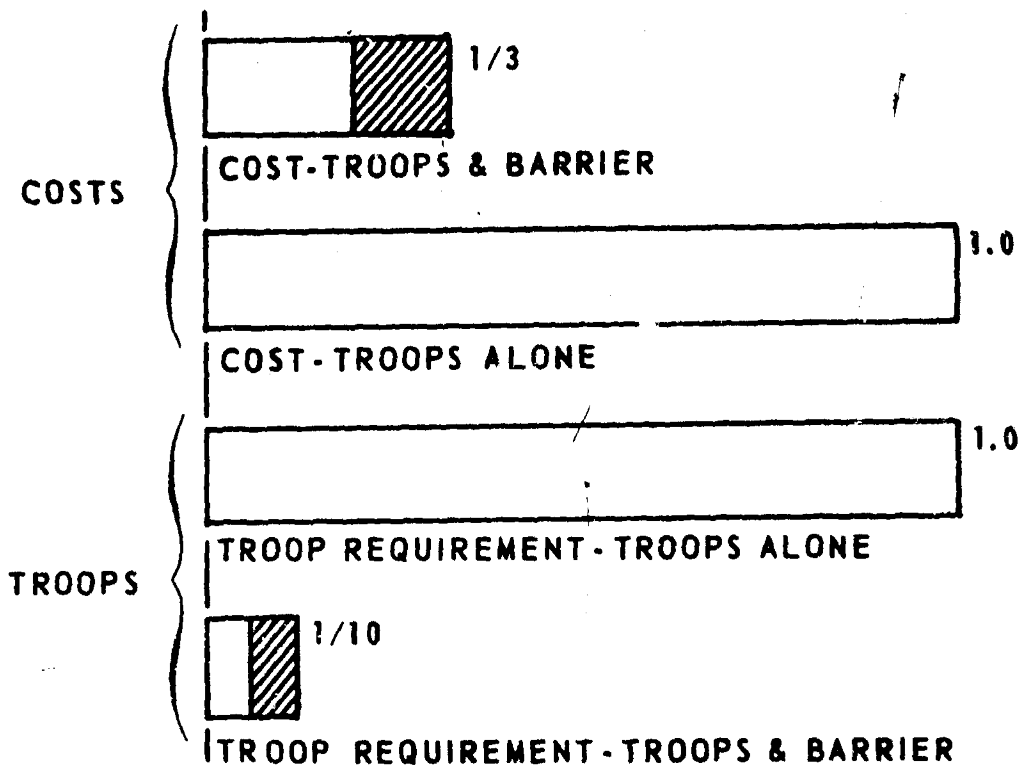


FIGURE V-5.

We observe in Mirage 70 (1960): (F)

It is a paradox that weakness in a prosaic area—transport—may drastically curtail the entire U.S. limited war capability in 1965–70.

This is compared with post-1961 changes in DOD planning for this field.[5]

Step 2.—Development of hypotheses.

The impact of authorized new systems in the post-1970 period is considered. The family of scenarios and new force concepts in the environment and basic needs discussion are assessed in terms of lift requirements.

[5] *An indication is provided in the published House Armed Services Committee Reports: Airlift/sealift forces budget in billions of dollars:*

Original fiscal year 62	*0.9*	*Fiscal year 65*	*1.5*
Final fiscal year 62	*1.2*	*Fiscal year 66*	*1.6*
Fiscal year 63	*1.4*	*Fiscal year 67*	*1.5*
Fiscal year 64	*1.3*	*Fiscal year 68*	*1.6*

Thus we find that current spending is about 70% above the original fiscal year 1962 budget.

For example, the rapid action control and strike force must perform unique tasks. The most likely bottlenecks in deployment for the post-1970 period are estimated. These might be in the form of software or hardware, tactics or SOP's, aircraft or ships, rapid deployment or steady state lift. The result is the formulation of hypotheses involving a novel operational concept in rapid deployment.

Step 3.—Analysis.

Considerable quantitative analysis is possible in this problem. A typical analysis concerns the comparison of the new rapid deployment modes with known ones (e. g., mariner ships, C–5A, pre-positioning). Suppose a 100,000-ton deployment to an inland area in country *X* is to be accomplished with a closure time of 30 days or less. Which mode represents the lowest cost system? Recognition is given to the fact that certain assumptions have a critical impact on the result. For example, the practicability of land pre-positioning and the allow-

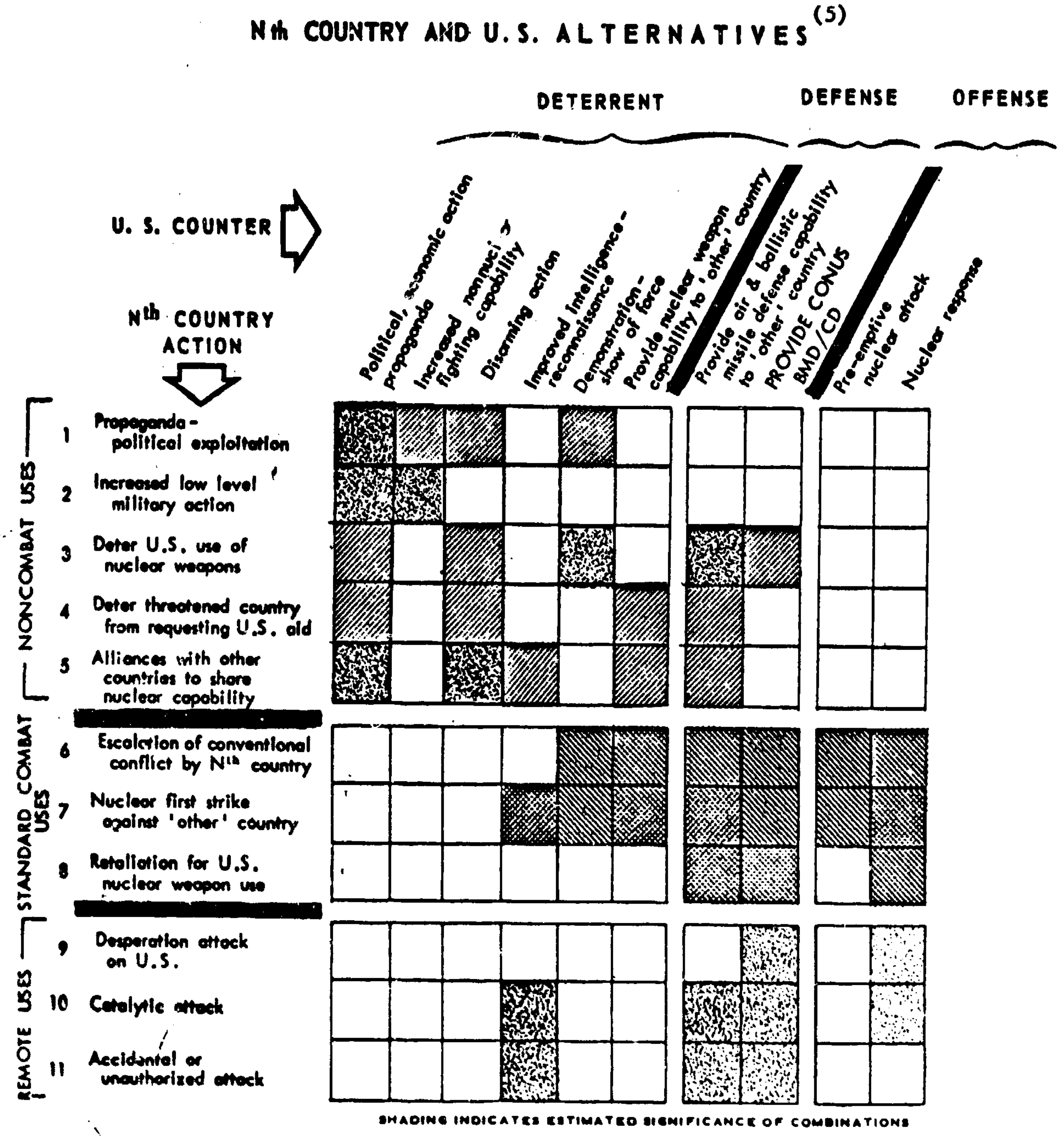


FIGURE V-6.

able peacetime earning power of the military transport aircraft have a very significant effect. Next, the closure time is reduced to 20 days and 10 days. The results may be depicted as in figure V-7. Sensitivity of the results is then checked.

Step 4.—The implications of the results are considered. Changes in other parts of the total lift system may be necessary to make the preferred alternative meaningful. *Examples:* Cargo handling, comprehensive logistics control system.

Other examples of tools which have been used:

- Historical trends: Ratio of initial opposing ground forces and its relation to battle out-

STRATEGIC LIFT
LOWEST COST ALTERNATIVES
(procurement +10 year operating)

ASSUMPTION:				
peacetime earning power	YES	YES	NO	NO
land prepositioning	YES	NO	YES	NO

DEPLOYMENT FOR MISSION X

n days	A	B	A	C
n + 10	D_1	D_1	A	D_1
n + 20	B	B	D_2	D_2

FIGURE V-7.

come; relation between mobility, firepower, and dispersion in past ground wars.

- Simulation of tactical operations: Armor—antiarmor combat (assault and defense, direct and indirect fire support).
- Decision diagrams: Graphic means to show indifference points in decision alternatives with major uncertainties.
- Program analysis: Analysis of programming under uncertainty with determination of decision timetable which permits maximum flexibility in future choices (e. g., NASA programs plan).

In the final assessment, two factors are of overriding importance: judgment and quality of the analysis. A Rand-type "murder board" provides a most desirable review technique for the various component studies.

C. The Mission Flow Chart Analysis.

The supplementary approach grew out of the difficulty (1962) in coming to grips with needs for general-purpose forces. In this field every system can perform many tasks and every mission can be performed in many ways. Further, in any given mission the glamorous systems tend to receive major attention while the most crucial bottlenecks may be in small, unglamorous phases of the mission.

The approach is summarized in figure V–8. On the basis of the environmental analysis a hierarchy of missions is defined. The scenario suggests alternative concepts of operation; concepts lead to strategic level missions and these to operations missions; finally, we are led to support or basic service missions.

After the hierarchy is developed an anatomical flow chart is created for each mission. The chart shows (*a*) the alternative means or systems for performing the mission step-by-step, and (*b*) the location of major weaknesses in any of the systems. The general structure of a flow chart is shown in figure V–9 and the corresponding detailed chart is reproduced in figure V–10. Columns of each chart indicate the sequential activities while the rows represent alternative approaches.

Problem areas are indicated by vertically written comments superimposed on the functional flow lines. Those problem areas considered most critical to the mission performance are further emphasized by enclosing the vertical comments by arrows.

It is found that the most incisive results are obtained when the analyst deduces the initial version of the chart structure without extensive prior external input. The analyst can intuit rather complex charts, even in fields he has not previously been close to, by calling on his general systems experience. After development of the general outlines of the chart, standard source material is consulted. The chart-drawing exercise forces the analyst to think hard about the mission under study, frees him from standardized approaches in a given field, and acts as an organizing tool.[6] Following this step, experts are consulted (1) to pinpoint the crucial weaknesses and (2) to indicate new alternatives made possible by technological achievements. At this stage, the relative weakness ranking within a mission is independent of the need of the corresponding mission itself.[7] In general, a weakness that cuts across all alternatives for performing the mission will rank high.

Weaknesses will vary in nature according to mission level. Those at the highest level pertain primarily to policy; those at low levels involve both hardware (e. g., lack of equipment) and software (e. g., obsolete standard operating procedures).

The charts greatly facilitate and stimulate the discussions and the interaction between the technologist and user or needs analyst. They act as a provocateur for brainstorming purposes, keep the discussion focused, and assure that weaknesses are not confined to the glamor items. Not infrequently, after several iterations, the final version of the chart and its crucial weaknesses yields significant surprises.

If a relative ranking of weaknesses is desired the following steps are followed:

(*a*) relate the missions at one level to those at the adjacent levels by means of relevance numbers.

(*b*) assign weights to all scenarios in the conflict spectrum.

(*c*) combine the values in (*a*) and (*b*) to obtain mission need values for all missions.

(*d*) rank the crucial weaknesses in each mission flow chart.

e) combine the values in (*c*) and (*d*) to obtain relative overall weakness values.

Table V–2 shows a sample ranking of mission (*c*) at each of the three levels and for three different scenario weightings (*b*).

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[6] *It is thus not unlike Helmer's contextual map ("Social Technology," Basic Books, 1966, p. 10).*

[7] *The ranking may depend, however, on which portion of the spectrum the mission is to be used with. Thus weakness W, may be crucial for all low intensity conflict scenarios but not for any high intensity scenarios. A division of the scenario spectrum into subsets is therefore assumed, and the normalized weakness weights are specified with reference to the applicable scenario subsets.*

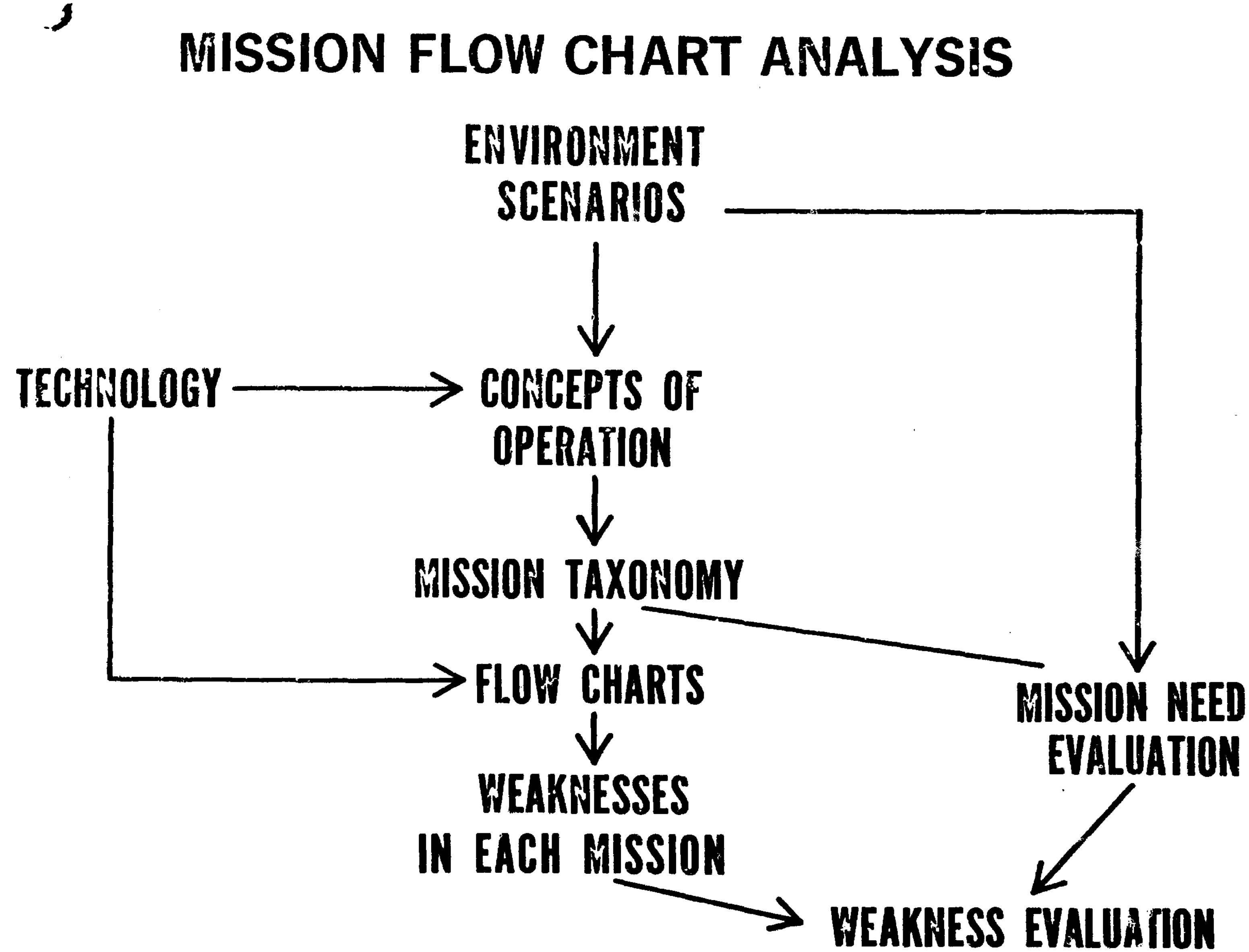
MISSION FLOW CHART ANALYSIS
ENVIRONMENT SCENARIOS
TECHNOLOGY
CONCEPTS OF OPERATION
MISSION TAXONOMY
FLOW CHARTS
WEAKNESSES IN EACH MISSION
MISSION NEED EVALUATION
WEAKNESS EVALUATION

FIGURE V-8.

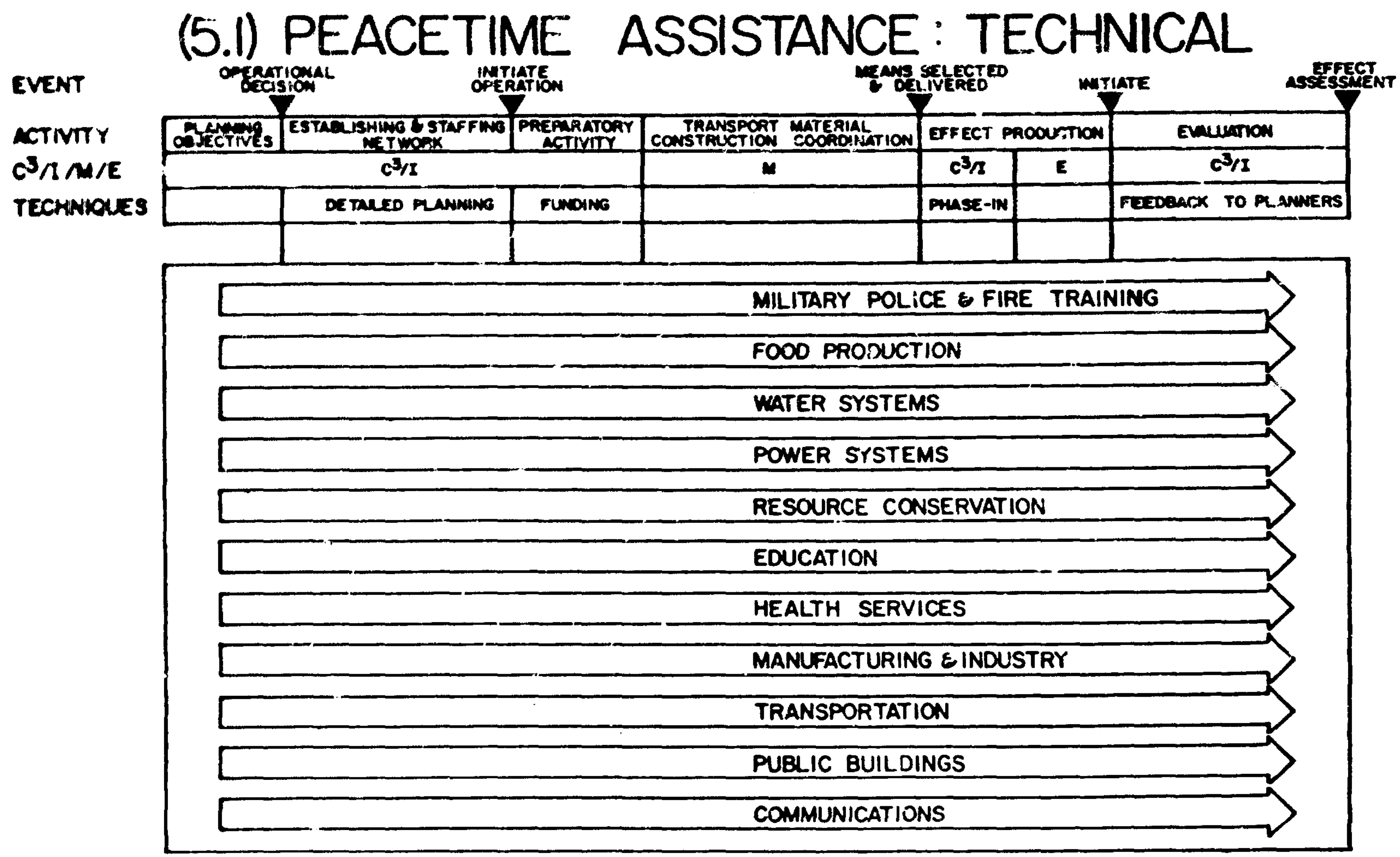
(5.1) PEACETIME ASSISTANCE : TECHNICAL
EVENT
OPERATIONAL DECISION
INITIATE OPERATION
MEANS SELECTED & DELIVERED
INITIATE
EFFECT ASSESSMENT
ACTIVITY
PLANNING OBJECTIVES
ESTABLISHING & STAFFING NETWORK
PREPARATORY ACTIVITY
TRANSPORT MATERIAL CONSTRUCTION COORDINATION
EFFECT PRODUCTION
EVALUATION
C3/I/M/E
C3/I
M
C3/I
E
C3/I
TECHNIQUES
DETAILED PLANNING
FUNDING
PHASE-IN
FEEDBACK TO PLANNERS
MILITARY POLICE & FIRE TRAINING
FOOD PRODUCTION
WATER SYSTEMS
POWER SYSTEMS
RESOURCE CONSERVATION
EDUCATION
HEALTH SERVICES
MANUFACTURING & INDUSTRY
TRANSPORTATION
PUBLIC BUILDINGS
COMMUNICATIONS

FIGURE V-9.

(5.1) PEACETIME ASSISTANCE: TECHNICAL

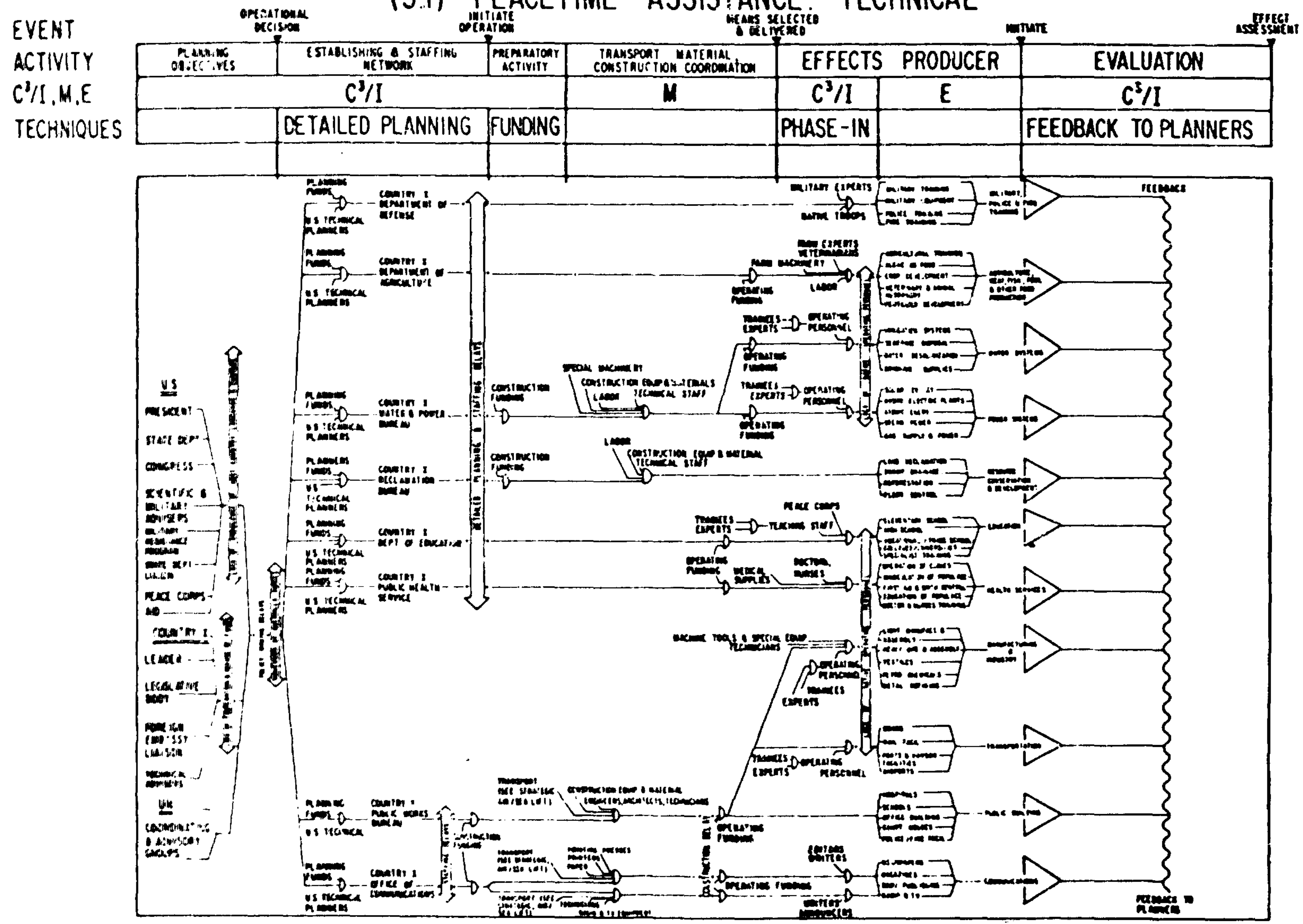

FIGURE V-10.

RANKING OF MISSION IMPORTANCE

Mission Category	Situations		
	Case I	Case II	Case III
Strategic Missions	Emergency preparedness Neutralization (population and government) ——— • • • •	Neutralization (population and government) Counterinsurgency Peacetime technical assistance Emergency preparedness ——— • •	Emergency preparedness Strategic warfare ——— • • • • •
Operations Missions	Barriers Neutralization (military operations capability and natural resources) Destruction of ground targets (by air and ground action) ——— • • •	Barriers Neutralization (military operations capability and natural resources) Destruction of ground targets (by air and ground action) ——— • • •	Barriers Defense of natural resources Destruction of ground targets (by air and ground action) ——— • • •
Service Missions	Combat surveillance and reconnaissance Communications - - - - - - - Target acquisition Intratheater mobility ——— • • •	Combat surveillance and reconnaissance Communications - - - - - - - Target acquisition Intratheater mobility ——— • • •	Communications Combat surveillance and reconnaissance - - - - - - - Target acquisition Electronic warfare ——— • • •

TABLE V-2.

It is also interesting to group the weaknesses according to the various levels of authority that should be concerned with the problem and its solution. Levels in DOD include Joint Chiefs of Staff, Labs, theater commanders, etc. Figures V-11 and V-12 suggest the content of the problem and solution matrix. (7)

Authority \ Activity	GROUND COMBAT
FIELD LEVEL	Problem Category: (B5) Lack of special capability Problem: Most currently available barrier devices are destructive in nature; sensor barriers are not emphasized enough Mission Chart Reference: Chart 3.10 Impact Level: Important in low and medium level warfare Solution Reference: See Matrix below

FIGURE V-11.—*Problem Matrix.*

Authority \ Activity	GROUND COMBAT
DEPARTMENTAL LEVEL	New interservice doctrine will be required if extensive use of barriers is to be made. For example, special coordination will be required between the air sower and the ground troops
FIELD LEVEL	Special field tactics must be devised to exploit barriers, e.g., the relation of sensor barriers to indirect fire support capability
SYSTEMS PLANNERS	Sensor barriers must be coordinated into overall surveillance and perimeter defense systems
LABORATORY LEVEL	Research on sensor barriers should be emphasized

FIGURE V-12.—*Solution Matrix.*

III. SOME COMMENTS

A. The Forcing Function—Need or Technology?

It was noted that we stressed the early interaction of needs and technology. We find here a classical chicken-and-egg argument. Which comes first—the need or the technology?

Quinn, in the "Harvard Business Review", (*8*) states: "Clearly perceived demand—not excess technological capacity—tends to be the primary force stimulating technological change. In fact, a technology is only utilized if it responds to a need."

Project Hindsight states: (*9*)

> If we identify . the events which were motivated by a clearly perceived DOD need, we see that they add up to 95 percent of all events. . . . It became clear early in the study that technological innovation was highly correlated with need-recognition.

The problem is with the terms "clearly perceived demand" and "clearly perceived DOD need." They can prove quite misleading. Perception of a new need is not a dramatic peak in a flat landscape. In 1939 the U.S. War Department clearly perceived the need for tanks but not for atomic bombs. On the basis of research advances in nuclear physics, it was the technological community which suggested the need for atomic bombs.

Clear perception of a need by the user usually occurs years after the ideas are first proposed and discussed. The initial impetus may come from either technology or need (fig. V–13).

Imagination must be stressed on the needs as well as on the technology side. Several years ago an issue of the U.S. Naval Institute Proceedings[8] was devoted to a series of articles on the Navy of the 1970's. Interspersed in this issue was a set of artists' conceptions of new naval systems for this period provided by the Martin Co. It is curious but significant that we find no relation whatsoever between the pictures and the text. Whereas the former are highly imaginative technologically, the discussion of needs is conservative and never refers to the illustrated concepts.

In 1961, I was invited to participate in the Army's annual national strategy seminar at Carlisle Barracks, Pa. There were more than 10 teams of officers completing their term papers on a national strategy for 1971. To my amazement, the significant differences between these efforts were in such items as numbers of U.S. divisions in Europe (e.g., five or six divisions in 1971).

Part of the difficulty stems from the education system. Just as the civilian schools are slow to adjust to changing needs (e.g., obsolete engineering curricula) so military schools are slow to alter their curricula to place proper emphasis on material suitable to the future environment in which the graduates will operate. Today's academies still consider wars between near equals, characteristic of the period 1800–1945 (e.g., Napoleonic Wars, World Wars I and II), as the norm. For the period of the seventies and eighties this seems strange indeed. We observe that in the last 3 years the world has witnessed over 1,000 days of unconventional war and 6 days of conventional war.

Another source is the traditional approach of the State Department which does not tend to encourage the kind of research on future worlds done, for example, by Herman Kahn's Hudson Institute.

The unsophisticated warfare involving the underdeveloped areas is not likely to restrict itself the guerrilla warfare now familiar in South Vietnam. A sea of poor surrounding a few islands of prosperity will develop other means. There are already some clues. *Example:* the recent Soviet-Chinese border incidents along the Amur River—thousands of unarmed Chinese peasants forced at (Chinese) gunpoint to cross into Soviet territory. (*10*)

[8] *January 1963.*

Military response also faces the complicating factor that the poor world has a 2 : 1 population advantage over the rich world and by 1980 will have a 3 : 1 edge. On the other hand, we see that in Vietnam a 300,000 enemy force (including 60,000 North Vietnamese troops) are tying down 1,200,000 U.S./RVN troops. A ratio of 10 : 1 or even 15 : 1 may be necessary for military victory—a necessary, but not sufficient, condition for total victory. The United States simply does not have the manpower resources to invest and a citizen army may be unavailable for Vietnam-type military duty in this period. Reliance for strictly military tasks may have to be placed on an elite professional army.

It matters little whether technology or need provides the original spark. But it matters very much that an imaginative and continuing dialogue between the user and technologist be assured so that we do not find ourselves with future technology to satisfy a current (but not future) need.

The problem of planning a balanced growth for an underdeveloped country system without the stresses and temporary reverses which produce mass revolutions is today largely unsolved. It is hoped that this challenge can be met by social technology if the massive U.S. intellectual resources are brought to bear on it. The point is that the bottleneck is on the technology—not the need—side.

(*b*) The very rich versus the rich.

The widening gap between the United States and the Soviet Union as well as Western Europe in the 1970–80 period suggests two alternatives to European or Soviet long-range planners:

(1) *Passive.*—One may wait for the historically inevitable reversal of this widening gap. The pattern of exponential growth followed by saturation, so familiar to biologists and technological forecasters, seems to apply to nations as well as to cells and technological improvements.

(2) *Active.*—Mirage 80 considers the possibility of a close tie between the Soviet Union and several major West European countries. This could lead to "isolation and decline of the United States by severe economic competition in Europe, Asia, Africa, and Latin America. It would constitute a unique way to negate the widening lead of the United States." (*13*) This approach may be described as strategic economic warfare against the United States.

It is evident that here, too, there is first and foremost a requirement to marshal our resources towards social technology to address this potentially serious threat.

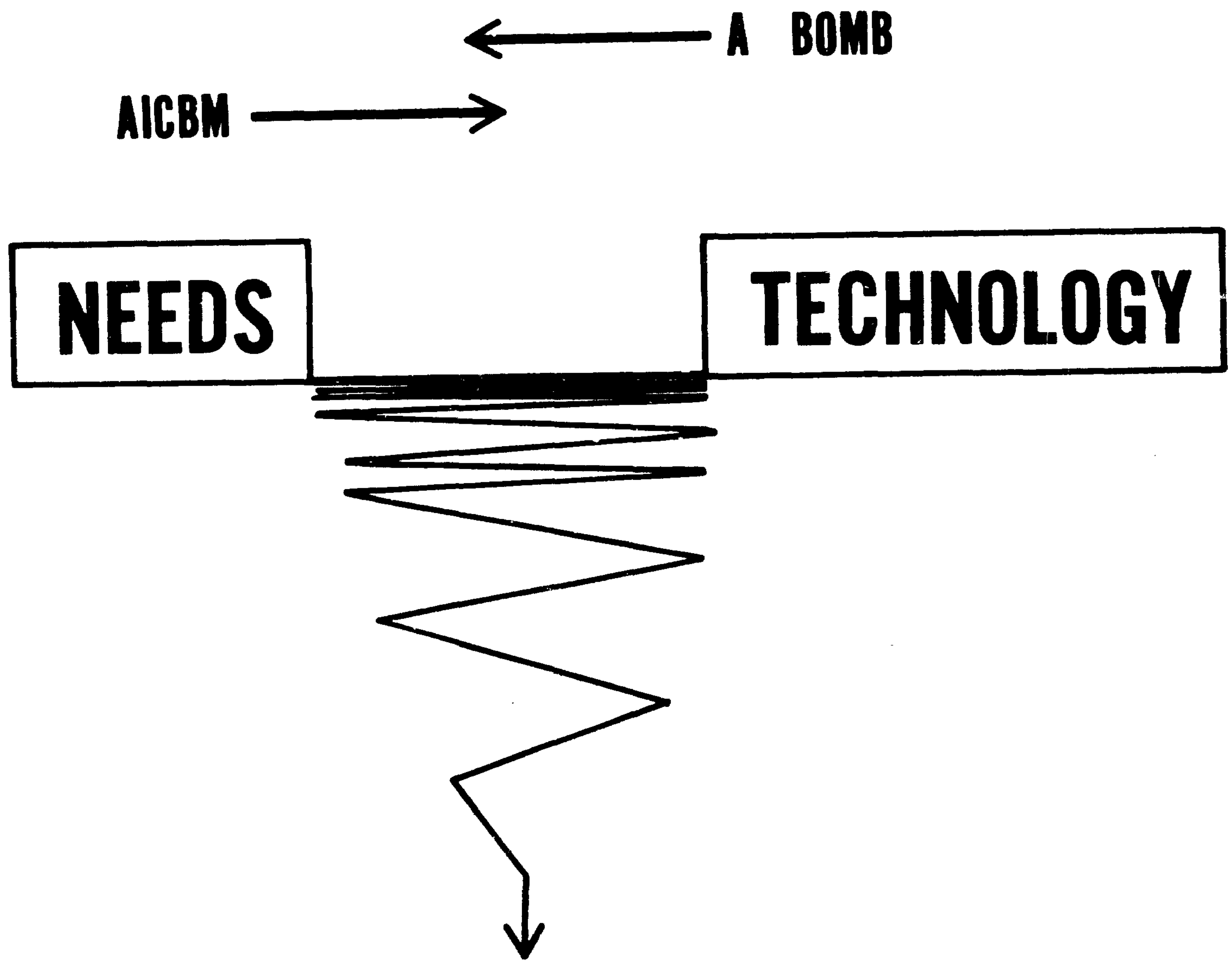

FIGURE V-13.—Needs-Technology.

These two examples suffice to illustrate the impending bottleneck on the technology side. As if this were not enough, we must add that the entire gap problem is gravely exacerbated by the inevitable widening gap in communication as the societies grow apart. Effective ballistic missile defense against a Soviet threat was enunciated as a clearly perceived need almost a decade ago but hard science technology could not develop an adequate solution. We now face an even more serious challenge to find answers in social technology.

B. A Crucial Weakness: Social Technology.

Our own analysis of needs points to a rapidly growing dilemma. The new needs pose requirements on social technology [9] which we may be incapable of meeting due to lack of requisite advances in the social sciences.

I shall illustrate this with two examples from Mirage 80, both related to the most salient characteristic of the 1970–80 period the widening gap (see fig. V–14).

(a) The rich vs. the poor. In Mirage 80, page 15, we read:

> A strictly military response in any conflict involving the underdeveloped countries is at best a palliative. Without a massive attack on the underlying problems, even a decisive military victory may lead to an equally decisive final defeat (e.g., Algeria).
>
> The 30 plus advanced countries have the scientific and technological resources potentially adequate to alleviate the social, political, and economic causes of unrest in the 90 plus underdeveloped countries. However, the present and planned efforts to apply these resources are entirely inadequate to forestall widespread violent upheaval.[10]

We are not simply talking here about larger funding of AID and Alliance for Progress programs or about conscientious native reformers. More massive assistance and rapid improvements may simply increase the violence.

There are striking clues: (a) In the dozen years after the death of Louis XIV, French trade increased nearly 100 million livres—there followed the French Revolution. (b) The Russian economy experienced rapid growth in the period 1895–1913 (e.g., exports 5.5 rubles per capita 1895–99, 9.1 in 1910–13—there followed the Russian Revolution. (*11*) (c) Violence in our ghettos has come hard on the heels of the most dramatic progress in civil rights legislation in 50 years.

The outstanding research by I. K. and R. L. Feierabend (*12*) and W. W. Conroe on 84 countries for the period 1948–62 clearly supports the hypothesis that—

> The faster the rate of change in the modernization process within any giver society, the higher the level of political instability wtihin that society.

[9] *A term popularized by Olaf Helmer (cf. "Social Technology," Basic Books, 1966).*

[10] *The world is spending over $150 billion per year on military establishments; the transfer of resources from developed to underdevlopd countries is $6–$8 billion per year. There is, in addition, the time element: simple calculation shows that it takes a country with $100 GNP per capita and 6 percent growth rate 88 years to catch up with a $3,000 GNP per capita country growing at only 2 percent.*

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THE WIDENING GAP - A WORLD DILEMMA

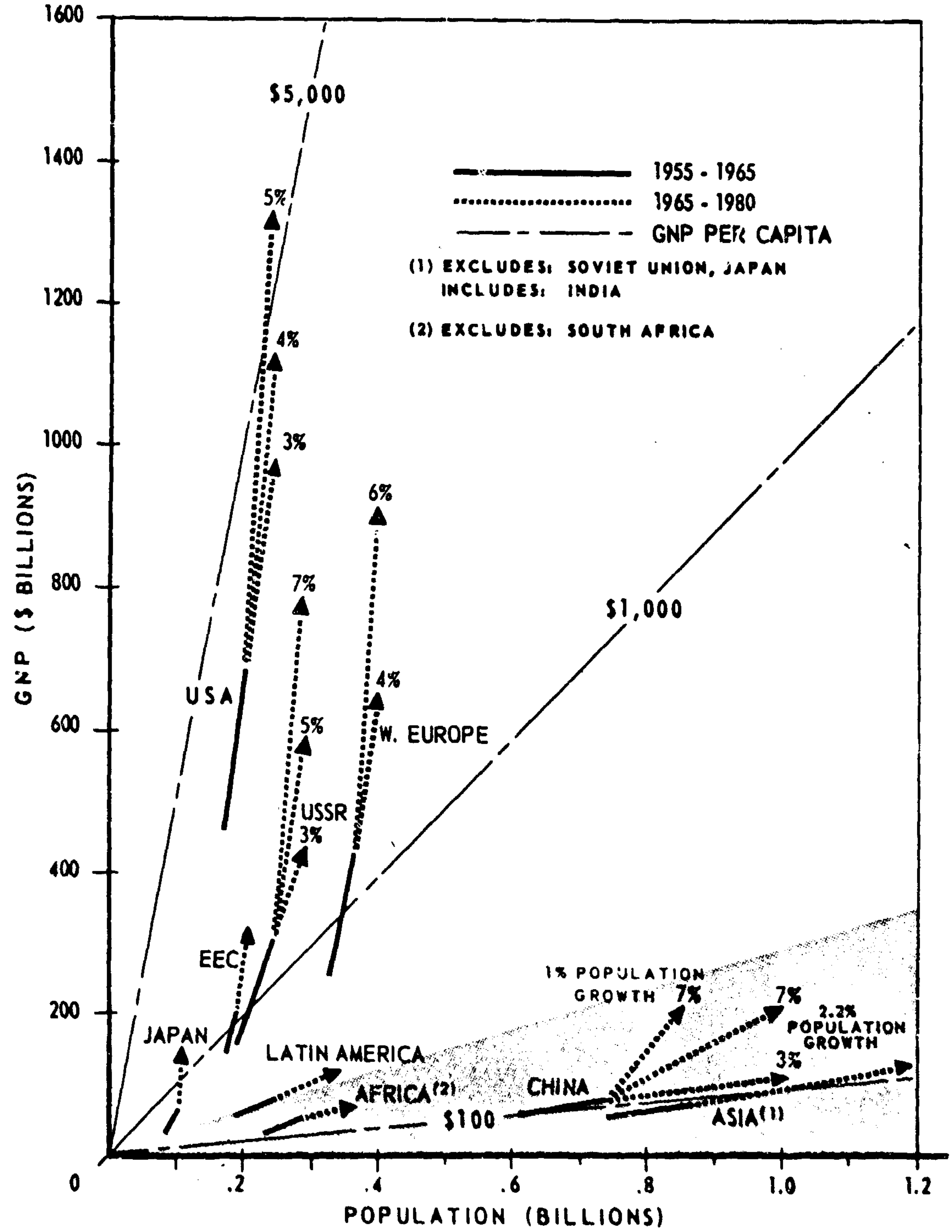

FIGURE V-14.

Mr. FRIEDMAN. One thing that prompted us was based on needs, not technology. Technology was developed as a result of needs. I will fortify that by this statement. The Germans were developing A-bombs and if we didn't, we would be in trouble. Therefore, as a result of the need, it was a triggering factor and had more of an impact towards getting action; that's what I am talking about. It is true that later it becomes hazy and difficult to determine where the beginning point lies.

Mr. CETRON. Is there a near orientation or a technological feasibility? Aren't they made up of both parts? Even the A-bombs are part of both.

Dr. LINSTONE. Looking through forecasting, a clearly percieved need, not innate technological change. This was from Professor Quinn. If we identify the events that were motivated by clearly perceived need, they would add up to 95 percent of all events. Early in the study the technological innovation was correlated with need recognition. What do you mean by clearly perceived need? This is a critical question where one can get into a tremendous number of arguments.

Dr. EBER. You should change needs to deficiencies.

Dr. LINSTONE. I want to be careful about future deficiencies, not necessarily current deficiencies. We came up with 10 years as a solution to the problem. There will be other problems of deficiencies. In Vietnam, at a manpower ratio of 4 to 1 over the enemy, we still have problems. Past experience indicates it ought to be 10 to 1 for success. I don't think anything has happened in Vietnam to disprove this ratio.

Colonel LUKE. I'm trying to figure out that chart. I see the little underdeveloped countries and China and it is on record that we are going to spend $5 billion because of something done by a country which has a $150 per capita income. We have Vietnam and don't know how to solve the problems. It seems you can have this but in the context of combat, to the people of the world, it doesn't mean a thing. We may be richer but we are not necessarily smarter.

Dr. LINSTONE. That's right. The fact that the ghetto problem, for example, surprised so many people. Why do we have the riots right after the biggest improvement in civil rights legislation in 50 years? There was rapid improvement in many past prerevolutionary periods. You say we are not any smarter but this paper is important because we are beginning to learn something. First, it is quite apparent we don't just spend money in these areas and expect to close this gap. Even if we could get Congress to appropriate it, it would be a foolish way to spend the money. We are just beginning to understand the interactions and what causes the revolutions. It's only in the last 12 months that things have been published on the problems that have been involving us for a long time. The importance of looking into the system comes in gaining understanding and that is the first step to solution. The reason for the theory in one paper is: When you get an improvement, hopes go up and people assume this is going to continue but you have an unbalanced growth, then their expectations are upset and this becomes what is called the revolutionary gap.

Dr. JOHNSTON. This appears to be an example of technology creating need, because in many countries they have advanced far enough so that every peasant family is going to have a transistor radio, bicycle, boat, and even TV. The point is they have found this gap; see it; are aware of it, even though they can't understand this kind of chart. Their notion of wanting a Mustang 10 years after they got bicycles is important to understand, and they think more about transistors than hunger.

Dr. LINESTONE. When you begin a curve and essentially have to take off at a point—that's when your aspirations are high. Further research may contradict it but this seems to be central. Coming up with the crucial needs of the seventies is the thing that needs number 1 priority.

Mr. IRWIN. I was interested in whether you have plugged age-distribution analysis into this type of thinking?

Dr. LINSTONE. No, we have not. This is a very simple curve, the GNP per capita type of thing. We have to consider for China. the impact of changes and assumptions for instance population control, 1 percent per year growth instead of 2 percent. We contradict Professor Fuller because it is impossible to see China industrialized, as he said, in 1975. Even if they get good economic growth and strong population control, they are still under the $300 level.

Mr. IRWIN. The point is the last 20 years have seen the most dramatic changes in age distribution, probably in the history of the world, by enormous levels. For instance, the Japan situation. There is a negative side to this and comment was the Japanese have overdone it rapidly by lowering the birth rate, and drained themselves of young people which has had a serious effect on the whole industrial system and the way people think. The alternative age implications, in those two population assumptions, are enormous. You are going to have a huge mass of young people or few. The impact of military and government employment, the Air Force, has created a tremendous impact on the age distribution in the State of New Mexico and there are so many children in the 5- and 10-year-old groups. I imagine I will find they are having problems with schools and other things. I'm thinking in terms of very general psychology, if you will, mass psychology. The population—how it is changed by these alternative shifts and the relative number of people in different ages.

Mr. CETRON. How can we solve these problems with inadequate social technology?

Dr. LINSTONE. We have done a study of Sudan due to the unpleasantness last year. In order to study transportation we had to look at the entire national planning and the entire economy. It is like climate control. People used to make tremendous technological forecasts in the 1950's: How to retard evaporation and make Florida more pleasant? Then it occurred to people: What will happen to the vegetation if this is changed? You have to know what you are doing here as well as in climatology and in these different countries. You don't understand the interactions well enough so that when you make a change it is a controlled change. An article I read—I think was in the "Columbia Business Review," by Pincus from Rand, pointed out that the most effective assistance has been given where the countries have been small. Taiwan is an example. I suspect one reason is that it is easy to understand where to put the money in a small country, a smaller problem than India. We'll see what happens on the research. The social structure has a system: Where do you put the money and the technology? This business, as Dr. Johnston pointed out, about creating problems with technology and creating revolution by the assistance which in many cases we tend to do.

Mr. CETRON. Something came up in a meeting about 4 months ago and I think it had a great bearing. General Marshall said the war in Vietnam is going to be a long war in the wrong place at the wrong time—we all agree with this. A briefing was given the Marine Corps and Colonel Vaughn, we asked him, Where would you like to fight least? He said, "The war in Detroit is the one I would like to fight least." Naturally, anyone with any brains is at least smart enough to know you'll fight the war you are not prepared for, or the one you want to fight least and what you are really doing in all your Mirage is using an old adage of "least regrets." Where do I lose least? If you are not building a capability.

Dr. LINSTONE. The point is that you have to do the planning. In this country it is certainly possible to have a balanced capability. Isn't this what our military friends constantly thought about, having a balanced force, so you won't have any particular weakness? An enemy will look for weak spots but, what must be done is to make every spot equally unattractive. We have superior resources to look into these problems. In long-range military documents you see the need for addressing these. But some of this doesn't get implemented; it isn't attractive. Unfortunately, in some way it is like the problem that President Eisenhower talked about: That neither military nor industry find it particularly attractive at this stage. It may be, as Professor Fuller alludes to all kinds of possibilities. Imagination is all we are trying to stimulate and in a sense, in a small group like this, we shouldn't be—there should be other groups far superior in addressing the military. What always strikes me is when I gave the actual Mirage briefing, I say "these are new missions that we have considered." I asked, What are the new missions? What are the new kinds of things you want to do? And you get a blank stare. I'm not saying these are correct missions but things to look into and it looks like there ought to be significant departures from the current ones.

Mr. CETRON. How do you know you can get so many things in? Red China sent all the people across the border and if they kept walking until they reach Vietnam, what would we do with them? You can't put that up as a mission. Yet the problems involved could be horrible.

Dr. LINSTONE. We have this barrier problem because very often you want to stop large masses of people from moving. You need a different approach from past traditional approaches to the whole barrier concept. We are making some beginnings in Vietnam. It is a different order of sophistication in the thinking of the subject and the China problem. You just can't handle or forget about the Chinese masses.

Dr. CHACKO. You feel there should be adequacies in social technologies. I was trying to find the latest word. You referred to an article and all I could gather was there is a gap between the expectation and the realization. Is this the extent of the report?

Dr. LINSTONE. The other phase of having it stay at a fairly moderate pace doesn't have the crash program funding approach it deserves. We are doing a lot in Thailand in having anthropologists there and people who are soft science technologists, researchers. You can't solve this if you don't know the interactions. Let me give you an example from a U.S. Office of Education study made recently. What would be the right way for legislatures to spend more money in depressed areas? An obvious technological view is build attractive school buildings so the children would like to go to school. Somebody else would point out, reduce the teacher-student ratio so each student gets more attention. In this pathbreaking study, both factors were minor in significance, but the one key issue was the attitude toward education in the home and everything paled to insignificance in comparison. This in a sense is research. When you want to spend money every problem at the present time is quite a shock to a number of people recommending expenditures for new school buildings. It came as quite a shock to the legislature in California. In the underdeveloped areas we have an obvious gap. We've got to understand the problem better than we do at present in order to come up with a technological approach. There are the quick-fix solutions "Yes, we've got some technology. Let's give everybody a TV set, modern bathrooms, and so on, to make them happy." But this would be disastrous because putting money in the wrong way gets you into more trouble than you were to start with.

Major SWETT. I am intrigued with what you are saying. We ran a study similar to Delphi except we sorted for problems instead of solutions. The problem is identifying future Air Force missions. It boils down to a demand problem which is the same phenomena you are calling needs versus technology. The demand side of the equation rests ultimately on the theory of utility, human values, human needs, attitudes; the areas that are nominally covered by the social sciences. The supply side: Technology, aircraft capability, et cetera, rests on the hard sciences. For our purpose, the areas most relevant were the least developed and the least relevant were the most developed. On social technology, it might be correct to say that technology is based on a science and the present status of the social sciences makes it a mutually contradictory term. The requirement for our purpose is philosophy-of-science-type basic research and especially the concept of utility.

Dr. SLAFKOSKY. I am interested in one aspect of the technique used with the Mirage. You implied this was primarily your in-house people who were part and parcel to this. What association did you have with the military? Were there retired military working with you and how was the military side brought in? The reason I am asking is not that they should dictate anything but I have a feeling that your engineers and personnel may not be as familiar with military operations as they might be required to be to do this work.

Dr. LINSTONE. Let me give an illustration of what happened during the time I was at Rand. We asked the Marine Corps, when we were working on the flow chart concept and gave them 16 scenarios, to do one for 10, 5, 15 years ahead, not a 6-month's scenario. They consented to come up with the responses for each scenario, and what happened. The results were very disappointing. The returns were always the same familiar approach. It isn't just the Marine Corps, our retired military are the same.

Dr. SLAFKOSKY. When I was involved with the CNA (Center for Naval Analyses) on a study, there is a broadening just in the Navy Department, in terms of having officers do this. CNA has available approximately 80 Navy officers who are parceled to each study the CNA does. They are actually in-house workmen. Marines also have their allotment. This study was started some time around 1960 or 1961 and finished in 1963. I am familiar with the reluctance to do this kind of thing. I think this would be more widely recognized on the part of the military, certainly those of us concerned with studies from the civilian side working with the military to open these doors, considerably, and I am sure they are being opened. I suggest that it may be a new way of getting some military people. You have enough military people to help you and they would not make it official in any sense of the word. The idea of an operational concept doesn't need official sanction. All you need is to be aware, which not only makes your study worthwhile, but more attractive to the military side.

Dr. LINSTONE. There was an issue of the Naval Institute Proceedings in 1963 or 1964 which was very symptomatic. It was strange indeed. There was a whole issue devoted to the Navy in the seventies including articles by admirals. The most imaginative one was a Marine Corps article by Colonel Saxton. In the same issue were illustrations of new concepts provided by the Martin Co.—and the interesting thing about it was there was absolutely no relation whatsoever between the new and interesting technological ideas and the entire written material. I say it is symptomatic, because this is the kind of things we were up against.

Major MARTINO. Another problem mentioned during your talk was that the work on United States-Soviet nuclear interchange had been studied with a great deal of precision.

Dr. LINSTONE. Compared to the limited war, it was an earlier analytical problem.

Major MARTINO. In many problem areas which have been intensively studied, I find a phenomenon best described as inbreeding, where a study becomes an art form like the modern detective novel. There is a certain way to write a detective novel and if you depart from the standard form readers won't like it and the same thing tends to happen in areas that are studied intensively by the same people.

Dr. LINSTONE. The modelitis problem, in the Mirage 70, we studied intensively with an entirely different group of people and a different approach, and they stood up well. We are happy to use those results. They are still quite reasonable. You are correct. The inbreeding is to be avoided. We did not use the same techniques as Rand, but the emphasis was on the action, reaction, and counteraction. We got it to what we called technological warfare. We went right to 1980 to our scientists and said, "We have a multiple interaction here; as a scientist, what do you think can be done on both sides" and as we go to this point, the 1980 point, you get into a set of alternatives for defensive action, offensive strategies and so on, and when you are through make sensitivity checks and realize certain conclusions you can draw. I admit the inbreeding problem is always there. It was quite a problem and I don't know if we were entirely successful tearing them loose from that kind of orientation. There are bound to be traces of parochialism.

Dr. LINSTONE. We learned another lesson in Mirage 70 on this. For example, we made strong statements about guerilla war in 1959 and 1960. Two and a half years later people asked us about the feasibility of starting a little guerilla warfare laboratory. By then, any possible advantages you might have gotten were lost. Everyone is a prophet except in your own house. It always looks better when an outsider says exactly the same thing. The only reason for the difference between DOD studies and our study was we were not paid by DOD money and were really interested in seeing the outcome. We learned a lesson from the first study and this is an advantage of having people from the division take part in the study. The doers are more closely tied in with the corporate office and not working in an ivory tower. Actions have been taken by the divisions very much in line of the recommendations. We can see the evidence. There are a number of areas where things have not been done but we don't expect a perfect batting average. Obviously, the divisions have an option. I agree, if you do this it is your responsibility to be able to communicate, and if you can't, don't blame the boss for not understanding and not taking action. We have one advantage—we briefed a study for the planning meeting a year after receiving the go-ahead, to the top level of the corporation including the chairman of the board. About 2 months later, at a regular staff meeting the president made it a point to ask what we were doing about this. He didn't say "Are we implementing everything?" He said, "What is being done? What are the divisins doing?" We had to have a committee of vice presidents in a couple of the areas where we were making recommendations for the corporation. After the study was completed, they formed a council of vice presidents to look into what was being done and as we look at these 10-years plans we see there has been an impact. It has been particularly strong in a couple small divisions, because they don't have the opportunity to do this themselves. They used our study in orienting their resources or research resources. Definite actions have been taken in the large divisions. One recommendation had nothing to do with anything military. The only recommendation was to completely operate in the system in-house capability in your own organization.

Colonel VAUGHN. I was on a Marine Corps long-range study panel. It was formed in 1963 as an ad hoc group by our commandant who gave us a charter to look at where the Marine Corps wants to be in 1985; he said take a look in the future and come up with some ideas, examine everything, and it possibly might be revolutionary. Those were his exact words "possibly revolutionary." He made the headlines shortly afterwards for his idea of sending a battalion of marines by rocket anywhere in the world in 45 minutes and has concluded that by 1985, it was technologically feasible but not practical. They came up with the Marine Corps of 1985 document which was a concept. From the concept they derived certain objectives which was the start of the long-range planning process. I might say as an aside, in looking over the history of what has happened in the past 4½ years, it appears there has been a tremendously increased interest in long-range thought. A few years ago long-range was thought to be 5, 6 or 7 years—later up to 10 and has jumped to 20. The Army has gone up 25. Dr. Helmer and Mr. Gordon from Rand go even further. It is spreading throughout the entire United States, but one thing of particular interest—informally discussing this document and came up with the concept called 1985 which was produced about 1960. How often should we do this and how often do we want inputs? One of the prime inputs was the Syracuse University Research Corp. The Marine Corps actually got the contract. I believe it is used as a basic document by a great many people called, "The World in 1985: Technological and Environmental Forecast." Dr. Linstone you mentioned that Lockheed has a 10-year plan, on a 10-year basis, and I was wondering how often this is revised and then, if it is not proprietary. Since you have gone through this Mirage cycle several times, do you have any feelings on how often it should be revised? Do you have any comments on whether the same group of people should be used over and over again or are you getting this inbreeding process?

Dr. LINSTONE. I think the biggest advantage of this kind of center corporate office—this kind of Mirage study and having people from the division—is at least inbreeding in terms of a long-range planning group, is minimized. Basically it is a help if you use a different group the next time except possibly one or two for their experience. There tends to be kind of a defensive attitude from the group that did it first; it isn't going o completely contradict itself the next time. It is like a comedian who has had a TV show for a long time where there is a limited number of jokes and routines, and you have exploited them in one study. You just have to get another routine. The 10-year plan is a fairly recent innovation which is required as an annual document and is still being revised. It's an evolutionary process in a sense, the first time it was a very poor document, the next time it was better, and the third time around better yet. I say poor because people don't take into account the uncertainties and problems. We are beginning, and I am very deeply involved, in trying to improve our own operation. We are using systems analysis now to look inward rather than outward. If it is good enough for McNamara, why don't we use some of these tools in our decisionmaking. For example, there has been the risk analysis which I might say the aerospace industry has not been a leader in—it's been outside the aerospace: risk analysis; decision trees, all these kinds of things, all these kinds of tools that we are beginning to use. Some amusing and some serious. For the first time some of the results were used by our management. We weren't happy with this 10-year plan, for example, they came up with certain goals and essentially filled in the business to reach these goals. We asked the question: Isn't it true you have to aim for a much higher level of business if you are going to get the level you are talking about? You know you're not going to be successful in everything you do. The question is: What is your range of certainty? How much do you have to aim for in order to get a reasonable likelihood of getting what you are after? This shocked people a little. Then we asked, What is the impact of one area on another? For example, if you do Project A, does this increase your probability of doing Project B and so on? I think Dr. North indicated something similar in TRW work. They were reluctant to do this but we are getting better each time, I might say. Regarding the substance of a 10-year plan, the variation from year to year is important but at this stage I don't think it is the key element. What is important is an exercise and each is a trial run and we will give an improved set of instructions each year.

Colonel VAUGHN. Your mirages were spaced—Mirage 70 came in 1959 and 1960 and then 1963, 1964, 1965 and 1966. You're out 14 years in the last one. Where do you think is your peak? How far out do you want to go?

Dr. LINSTONE. I'm not particularly concerned about going too much farther out at this point. Within a couple of years we might want to look up to 1985 but at the moment it is not the redoing of the Mirage study that is the problem but finding the technology. I suspect if we did it over again now, the results would not be much different. In a sense, the problem is more: you better suggest some answers than state the problems again.

Dr. Johnston's paper looks at a specialized aspect of demography, namely the forecasting of the labor force. The Department of Labor has been involved in this effort for many years, and has developed an approach to their task which has a number of aspects that might be of value to forecasters in other subject areas.

— Editor

LONG RANGE PROJECTIONS OF LABOR FORCE*

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I. SELECTED TECHNIQUES OF PROJECTION

One of the more intuitively appealing ways to discuss techniques of projection is, of course, to range them from simple to complex. Such a ranking is a sensible pedagogical device, but it is not without its dangers. The simplest techniques, when applied to projections, are commonly labeled "naive" while the more complex approaches come to be regarded, at least by implication, as sophisticated. While the users of such terms might deny any pejorative intent, the connotation that it is bad to be naive is inescapable. When econometricians refer to a projection as naive, they mean that it has been developed without a theoretical grasp of the underlying mechanism. As we shall see, extrapolative techniques are typically naive in this sense. However, the term is still unfortunate, particularly in view of the fact that the superiority of the sophisticated techniques is by no means clearly established either in economics or demography, at least according to such conventional criteria as the accuracy of the results. We shall therefore refer to simple and complex techniques, with the admonition that true sophistication consists in knowing when to use each of the available techniques.

The most simple projective technique is to assume no change over the period of the projection, from the time of the last observation. Such a forecast (since it provides no range of values, it is, in effect, a forecast) is of course among the least likely to materialize, since the occurrence of some change is surely one of the most dependable of expectations. Where change is so gradual as to be insignificant over short periods, a forecast of constancy is of course perfectly defensible, but such circumstances are rare indeed.

Next in simplicity is a projection (forecast) obtained by assuming the same direction and amount of change over the projection period as occurred over an equivalent period in the past, up to the date or base line from which the projection extends. By this simple technique, if the labor force increased by 12 millions between year $x-10$ and year x, it will be projected to increase by 12 million between year x and year $x+10$. Although this technique at least recognizes a time trend, the rate of change that it implicitly assumes is of a very particular and implausible nature—one that decreases regularly through time with the increase in the size of the base, such that the amount of increase (or change) in each period remains constant.

The next degree in complexity is introduced by assuming a constant rate of change over the projection period. This technique, like the one preceding, requires as a minimum two observations in the past, from which an average rate of change can be computed. By retaining a constant rate of change, this technique yields changes of gradually changing magnitude as the size of the base changes through time. This is perhaps the simplest technique enjoying any degree of verisimilitude. Among human populations, rates of change relating to many social and economic phenomena do remain approximately constant over considerable periods of time, particularly if we ignore minor fluctuations. As we have noted, the above techniques are typically employed in obtaining forecasts. However, such forecasts can readily be modified from point to interval predictions by the simple expedient of introducing a range of error or probable variation around the specific forecast values. Where such simple techniques are called for, however, an assumed range of probable variation could only be arrived at by guesswork.

Somewhat more complicated to carry out are the extrapolative techniques which involve superimposing a curve of some kind (of which a straight line is of course a limiting case) on a time series of observations, deriving the forecast by reading off the curve where it

*This paper contains the major part of a study carried out by the writer during fiscal year 1967 under the provisions of the Department of Labor Career Service Award program. The opinions, interpretations, and conclusions expressed herein are those of the author, and do not necessarily represent the views—official or unofficial—of the Department of Labor or the Bureau of Labor Statistics.

intersects the time axis at each forecast date.[1] Since most time series show some fluctuation in either or both the magnitude and direction of changes between successive observations, any curve-fitting technique entails some kind of averaging or smoothing of these fluctuations. The conventional procedure is to fit a line of least squares or one of its variants through the given series of observations, reading the forecast from the extrapolated line in the usual manner. When a set of projections (or range of forecasts) is desired, it is sometimes possible to fit such lines through the peaks and the troughs of the given observations, thus obtaining a high and a low series surrounding the basic extrapolation. Unfortunately, if the amplitude of the fluctuations in the given time series is changing through time (i.e., if the data are heteroscedastic) this procedure will yield absurd results.

In fitting curves to any time series, the technician must seek a trade-off between a curve of higher order which closely follows the fluctuations in the observed series and one which averages out these fluctuations in order to provide a more straightforward forecast. The more simple curves (including the straight line as a limiting case) are commonly favored intuitively. They can also be defended on theoretical grounds, insofar as the fluctuations in a time series can be attributed to irregular changes, sampling variability, the occurrence of random shocks, and the like, all of which would tend, in the longrun, to cancel each other out. Hence a smoothing procedure which, in effect, accomplishes this cancellation by an averaging process is sensible. Projections derived from higher-order curves which attempt to capture some of these fluctuations involve the more questionable assumption that the pattern of fluctuations observed over a finite segment of past time will be repeated during a finite future time period.

Time series relating to social or economic phenomena commonly reveal a strong cyclical pattern, or else the operation of such a cycle may be assumed on theoretical grounds. In such cases, the technician may wish to employ a higher-order curve which reflects the cycle and carries it forward through time. This usage represents the most legitimate employment of curve-fitting techniques to obtain extrapolations for projection. Unfortunately, theories supporting the existence of cycles in the field of manpower analysis are typically too general in their formulation to provide specific guidelines as to the type and phasing of the cycles which may be operative.[2] Meanwhile, extrapolative techniques typically suffer from an absence of theoretical underpinning, and thus represent little more than the assumption that the future will be an extension of past trends.

For the technician, these curve-fitting extrapolative techniques offer the sole advantage of being quick and easy to employ, and readily defensible, at least in a superficial sense, given the usual "ceteris paribus" clause. However, he can learn very little either from forecasts which prove to be accurate or from those which fall wide of the mark, since his efforts are not based upon a theoretical grasp of the underlying mechanisms in operation. For the policymaker, on the other hand, these simpler techniques can provide a very useful check on the reasonableness or plausibility of projections derived from more complicated procedures. The basic difficulty with any extrapolative technique is its inability to forecast structural changes which may modify past trends quite drastically. The central problem in long-range projection is therefore that of developing an understanding of the dynamics underlying structural changes so as to determine the likelihood that such changes will occur over the forecast period. A theory which could take account of these underlying factors in a systematic fashion would provide a powerful tool for purposes of projection.[3]

A promising extension of extrapolative techniques is the technique of cohort analysis.[4] This technique can best be illustrated in terms of population projections, where it has already proven its worth as a substitute for more conventional techniques of projection. In extrapolating the size of a population group by conventional methods, successive observations over time of the size of each particular age-sex group are obtained.

[1] There is considerable literature relating to the fitting of exponential, logistic, and similar curves to time series of data on aggregate population size to obtain projections of total population for different future items. A detailed summary of earlier efforts in this area is Joseph J. Spengler (*180*). For a critical review by one of the rediscoverers of the logistic curve, see Lowell J. Reed (*150*). More recent analyses of the problems of population projection, tempered by the sobering experiences of the 1930's and 1940's, include Harold F. Dorn (*46*), John Hajnal (*81*), and John V. Grauman (*65*). For a balanced summary of current developments in the field, see Grauman (*66*).

[2] Significant progress has been made in this area. See Simon Kuznets (*102*) and, more recently, Richard A. Easterlin (*48*). Easterlin's more extensive work on this subject, entitled "Population, Labor Force, and Long Swings in Economic Growth—The American Experience," is forthcoming.

[3] On this issue, see V. Lewis Bassie (7) and Rex F. Daly (*36*).

[4] A useful summary of the application of cohort analysis in projecting the fertility of a population is provided by Jacob S. Siegel et al. (*171*), pp. 12-23. The technique is also described in Pascal K. Whelpton (*200*). Pioneering work in this area includes the study by Whelpton (*201*) and by Norman B. Ryder (*157*). For a detailed evaluation of the cohort-fertility method of population projection, see Whelpton et al (*202*).

From this time series, a measure of its average rate of change over the base period is obtained and applied to the latest available estimate to yield a projection for the desired date in the future. Since the age-sex category to which the successive observations relate remains fixed throughout the base period, the actual population observed at each point in time is changing constantly. In contrast, a cohort analysis involves successive observations of the same population group (or cohort) as it passes through time. For example, the number of persons aged 10 to 14 in 1940 is linked with the number 15 to 19 in 1945, 20 to 24 in 1950, etc.[5] The obvious advantage of cohort analysis as a source of trends to be used for projection is that each cohort series represents the actual historical experience of a specific group of people with respect to the parameter being estimated. This provides greater theoretical justification for the assumption that past trends will continue into the future.

The principle disadvantage of this technique is that it requires a great deal of historical data of comparable quality with observations at frequent intervals. Decennial census data are generally unsatisfactory for this purpose, since they yield observations only at 10-year intervals. However, it is possible that useful data might be obtained from a series of annual-average observations of the current population survey, classified by single years of age, provided that the sampling variability of such disaggregated data would not be prohibitively large.[6]

The experience gained by demographers in cohort analysis during the past 15 years should serve as a warning against its uncritical application in areas where the available data are less adequate than in the field of population, where its use has given rise to a number of problems despite the apparent wealth of available data. Nevertheless, with the improvement in both the quality and quantity of data that might be expected in the future, techniques of cohort analysis offer great potentiality for the development of improved projections in a number of areas related to population, including labor force.[7]

The techniques employed in short-range economic forecasting offer a wider range of approaches than any other area of forecasting. Furthermore, the preponderance of effort in forecasting, particularly in the United States, has been expended in this field. Therefore, a review of these techniques, aimed at assessing their potential applicability to long-range forecasting, provides a useful supplement to the techniques discussed so far.[8] In one of the best critical summaries of these techniques, Roos distinguishes five approaches:[9] "naive," "leading indexes," "comparative pressures," "opinion polls," and "econometrics." The naive techniques as outlined by Roos are essentially those that have just been summarized, with the addition of cyclical or harmonic analysis of deviations from trend and the use of autocorrelation functions. Cyclical or harmonic analysis aims at removing the effects of cyclical fluctuations from a given time series, so that a cleaner description of the basic trend can be obtained for purposes or extrapolation.[10] Autocorrelation functions provide a means of projecting a given series by correlating it with itself at selected points in the past.

Without trying to defend the use of naive extrapolative techniques in forecasting, Roos does recognize one reason for their persistent employment, besides the advantages of simplicity and speed: they turn out to be correct more often than not, for the simple reason that over the shortrun, persistence of a given trend is more common than reversals of trend. Hence the sophisticated forecaster may well prefer the naive technique, insofar as his performance is measured by the ratio of his successes to his failures. Unfortunately, it is precisely the reversals that most concern the policy-maker, and such techniques are useless in forecasting reversals.

The method of leading indexes involves a search for data or some combination of data in the form of an index whose fluctuations through time regularly precede those of the series to be projected. It is not difficult to provide examples of this kind of precedence in the realms of economic activity and in population dynamics as well. For examples, new orders precede shipment of goods, shipments precede sales and earnings,

[5] A "birth-cohort" or "age-cohort" is a group of persons born within a specified time interval (usually 1 or 5 years). Similarly, a marriage-cohort, often used in the analysis of trends in fertility, is a group of women married within a specified time interval. The term "cohort" usually refers to a birth-cohort, unless otherwise specified.

[6] Exploratory research in this direction has been undertaken by Dr. Malcolm S. Cohen of the Bureau of Labor Statistics.

[7] Quite apart from its use in projections, cohort analysis provides information of great interest in historical studies, where it is inherently superior to period analysis in delineating the actual historical experience of a group of persons. Examples of studies using cohort analysis in both senses include James M. Beshers (*12*) and Karl Taeuber (*190*). For a critical comparison of cohort-fertility and parity-progression methods in projecting fertility, see Donald S. Akers (*3*).

[8] Useful summaries of these techniques are provided in Croxton and Cowden (*35*), and in Ferber and Verdoorn (*53*).

[9] Charles F. Roos (*153*). For an earlier discussion of extrapolative techniques, see Albert G. Hart (*85*), pp. 78–82.

[10] This is effected in labor force projections by using seasonally adjusted data in their development.

an upsurge in marriages precedes a rise in births which in turn precedes a rise in school enrollments, etc. In practice, however, such indexes are of limited value because the period of the forecast is necessarily limited to the length of the timelag between the last observation relating to the leading index and the corresponding successive observation on the series being projected. In general, the longer the timelag between the leading index and the variable being forecast, the less precise is the association between them. On the other hand, if the timelag is short, it may not be much longer than the delay in reporting the last observation on the components of the leading index. In other words, by the time the forecaster has obtained the latest data relating to his leading index, much of the period over which he can safely project may have elapsed. This technique is useful, however, in the development of target projections, wherein one or more dependent variables are projected by means of their association with a selected independent variable whose future values have been projected separately. For example, given a hypothetical projection of the gross national product, it is possible to derive projections of growth in the several sectors of the economy.

The method of comparative pressures implies a somewhat greater theoretical grasp of the processes governing changes in the series being projected. Examples in the field of economic forecasting, as given by Roos, include the use of trends in the ratio of inventory to sales, production to capacity, now orders to shipments, and inventory to new orders.[11] Comparable examples in the field of labor force analysis are not easy to visualize. Trends in the ratio of school enrollments to new labor force entrants might be of some value in short-range labor force projections, but the former are largely a function of population size, at least in the United States. Thus, this technique is not promising for labor force projections so long as no simple stimulus-response mechanism can be found to rule over significant segments of the population of working age.[12]

The hope that better forecasts might be developed on the basis of opinion polling has motivated their recurrent use, and the accompanying literature has been enlivened by considerable controversy. The fundamental idea, of course, is that the behavioral scientist should avail himself of the unique opportunity afforded him by the fact that those whose behavior he studies are capable of rational action and can communicate their plans and intentions to the observer or interviewer. For the forecaster, the obvious course is to select a sample of actors or agents in the area of concern, obtain from them information as to their intentions, and develop his forecast on the basis of this information. The usefulness of this approach in the area of labor force participation has yet to be tested, although information collected on reasons why persons are not working or looking for work does provide some insight into the circumstances under which such individuals might become active workers. While this approach might be of some value in developing short-range forecasts, such as forecasting the retirement decisions of older workers, it is of doubtful value for purposes of long-range projection.

Roos' criticism of opinion polling as a technique of economic forecasting takes a different turn. He argues that because the expressed intentions of most businessmen (and, a fortiori, of most consumers) are formulated by naive extrapolations of trends and are greatly influenced by strong bandwagon effects, any sampling of these intentions is likely to yield results closely similar to those obtained by naive extrapolation. In addition, the strong dominance of trends observed over the period immediately preceding the time at which the forecast is prepared makes it highly unlikely that this technique could reveal a turning point in advance of its occurrence. On the other hand, where a bandwagon effect becomes dominant over current trends, the resultant forecast is quite likely to predict a false reversal.[13]

The experience gained in surveys of establishments whereby information is obtained on anticipated employment needs casts further doubt on the utility of this technique as a means of developing reliable forecasts. In his study of the employment forecast survey in Canada, Hartle concludes that these surveys cannot be expected to provide a suitable basis for reliable forecasts of employment.[14] His findings reveal that a number of ingenious efforts to improve the forecasting reliability of the data obtained in these surveys proved unavailing. For example, efforts aimed at eliciting more

[11] Roos op. cit., pp. 379f.

[12] The application of the additional worker theory in forecasting labor force growth under stipulated conditions of unemployment would belong under this rubric, insofar as the growth of the labor force was predicated on some assumed ratio of additional workers to unemployed workers. Such methods have had only limited success to date, because of the difficulty of separating the additional worker effect from a host of other factors which operate simultaneously to encourage or discourage labor force participation among the more marginal workers.

[13] Roos op. cit., pp. 381–386. The central problem here, as with most opinion polling techniques, is that expert or carefully formulated views tend to be submerged in an averaging process which is heavily weighted by the larger mass of inexpert opinion.

[14] Douglas G. Hartle (*86*), pp. 93 and 105ff.

careful responses were not successful. Most schedules are filled out hastily, and often by persons well below the decisionmaking levels in their organizations. Also unsuccessful were attempts to adjust the responses to take account of an apparent seasonal factor in the reported intentions of the establishments surveyed. Hartle's analysis of the individual forecasts of particular establishments showed that they were actually less accurate, on the whole, than they would have been had they been derived by simple extrapolation of past trends. Furthermore, these forecasts were characterized by the same regressive feature noted in an earlier study by Ferber—i.e., a pronounced tendency to forecast reversals of trend in employment.[15] Hartle is unwilling to end his analysis on a negative note, and suggests instead that information on employers' forecasts of employment might be incorporated into a larger model, rather than being used to project employment directly.

The use of econometric techniques in the preparation of projections, forecasts, and predictions is a very large topic in itself. The term "econometric" refers to a combination of economic theory, quantitative economic data, and mathematical-statistical tools of analysis. This definition at once suggests certain limitations in the applicability of econometrics to labor force analysis or labor force projections, insofar as the labor force activities or proclivities of a population cannot be fully subsumed under a scheme of economic determinism. However, it does have great potential for fruitful application in labor force analysis inasmuch as it becomes possible to incorporate measures of appropriate behavioristic variables into the econometric equations.

Broadly conceived, econometric techniques comprise an even wider spectrum of approaches than those discussed up to now, ranging from simple regressions to highly complex models and model systems. Econometric techniques cannot therefore be distinguished by reason of complexity. What characterizes these techniques is the systematic integration of quantitatively expressed economic theory, the methods of mathematical statistics, and data in the form of quantitative measures or indices of economic phenomena. The theory is the source of the models that are constructed; the data provide their content and thereby provide the crucial linkage between theory and empirical reality; the statistical techniques permit the expression of the postulated relationships in testable form—typically as equations to be solved. A basic requirement in all econometric procedures is the detailed explication of the assumptions and procedures adopted in a given operation. Hence the strong reliance upon quantitative expressions and measurements of relevant variables.[16]

Insofar as forecasting or prediction is concerned, the basic requirements of the econometric approach are readily set forth. The difficulties commonly encountered in trying to fulfill these requirements are not so easily summarized.[17] According to Christ, the equations used in econometric analysis should have the following properties: relevance, simplicity, theoretical consistency, explanatory power, accurate coefficients, and forecasting or predictive ability.[18] Relevance means that these equations must have a direct bearing on a significant problem. Simplicity means workability—i.e., the equation system must be amenable to manipulation and must be comprehensible. Theoretical consistency means that the relations postulated by the theory must not be contradicted by those expressed in the equations. Explanatory power means relevance to actual phenomena, as reflected in available data. Accuracy of coefficients means measurability within a finite range of variability. Finally, predictive ability means the ability to generate accurate forecasts with the given equations. Christ makes it perfectly clear that the latter objective cannot be attained unless the former requirements are met.

The implications of these requirements for the problem of projection can be brought into focus in terms of Christ's useful distinction between forecasts (predictions) designed to guide the policymaker and those designed to test hypotheses.[19] A forecast is most fruitful

[15] Hartle's study revealed that over a 3-year period, 388 manufacturing establishments experienced about 1,700 individual turning points, while predicting over 3,000 of them. (Hartle, *Ibid.*, p. 102.) The earlier study cited by Hartle is Robert Ferber, "The Railroad Shippers' Forecasts" (1953). An excellent collection of articles on the uses and limitations of data on expectations is Mary Jean Bowman, ed. (*15*). More recent studies include Franco Modigliani and Kalman J. Cohen (*133*) and Andrew S. Ozga (*145*). For a careful review of the predictive value of consumers' attitude surveys, see Eva Mueller (*140*).

[16] An elementary introduction to econometric techniques is Michael J. Brennan (*18*). Three works which place particular emphasis on the practical techniques of economic forecasting are Henri Theil (*194, 195*) and Robert G. Brown (*21*). A recent, somewhat advanced presentation of the statistical methods being developed to cope with econometric problems is E. Malinvaud (*117*). An excellent and somewhat more general treatment of econometrics is Carl F. Christ (*26*).

[17] As noted earlier, most econometricians use the terms "forecast" and "prediction" synonymously. Both Christ and Theil use either term in relation to statements about behavior which has not yet been observed (although it may have occurred in the past). In the English translation of his work, Malinvaud uses the term "prediction" exclusively.

[18] Christ *op. cit.*, pp. 4ff.

[19] Christ, *Ibid.*, p. 293. Christ repeats Pigou's apt phraseology here, distinguishing between forecasts which are designed to yield fruit and those which aim to yield light. Compare Knowles (*99*), p. 7.

to the policymaker if it is conditional upon variables whose values reflect alternative policies or which can be affected in predetermined ways by such policies. Ideally, such forecasts, granted the inevitable ceteris paribus caveat, will tell the policymaker that the adoption of policy A is likely to produce condition X, B will yield Y, etc. These forecasts must also extend far enough into the future to allow the consequences of given policies to work themselves out, and to allow the policymaker some freedom of decision.

The needs of enlightenment in hypothesis testing are somewhat less restrictive, since forecasts which extend only from an earlier past period to one in the more recent past frequently afford highly useful tests of specific hypotheses as well as complete models. Nevertheless, confirmation or disconfirmation of an hypothesis in the light of purely historical data is seldom entirely conclusive with regard to its possible status in the future, at least in the behavioral sciences.

With the above uses of forecasts in mind, we can now summarize the problems encountered in satisfying the six requirements for econometric equations as set forth by Christ. First, the requirement of relevance imposes the severe restriction that the zeal to simplify matters by means of arbitrary classification and artificial quantification must be tempered by a willingness to struggle with the complexities of the real world. Highly abstract models provide neither guidance to the policymaker nor tests for the hypotheses of the researcher. In his highly critical exposition of economic methodology, Schoeffler questions the relevance of what he terms "mechanical behavior models" on the grounds that they necessarily assume fixed patterns of response to given sets of environmental factors. He argues that in its actual operation, a firm (for example) may or may not act to maximize its profits, minimize risks, etc. Thus any model which assumes a necessary rather than voluntary response to given conditions is artificially mechanistic.[20]

The simplicity requirement must be considered at two levels—that of the technician and that of the user. For the former group, simplicity is largely a matter of explicitness of assumptions and sufficient clarity and detail in the explanation of one's procedure—requirements which are fully satisfied if and only if the model can be described by means of equations, inequalities, or similar mathematical constructs. The advent of computers and their rapid improvement has greatly reduced the restrictiveness of another simplicity requirement, that of computational facility. Nevertheless, the finite capacity of existing computers shall always constitute an ultimate limitation on the complexity of models or equation systems which can be handled effectively. To the users, particularly those lacking in technical training, the clearest econometric model is typically the most opaque, and the greater the mass of quantitative detail offered, the less intelligible the totality becomes. The common reaction of the technician to this dilemma is inadequate: He would merely insist that persons who use the results of his work be equipped to understand it in his terms. But this view either underestimates the intellectual achievement of the technician, or overestimates the time, energy, and possibly even the capacity of the user. A more promising alternative, and one which often yields beneficial side effects, is to append a nontechnical or verbal account of the work. This task requires considerable skill in its own right, since it is necessary to explain technical matters without distorting them by oversimplification, and yet without writing a basic textbook for the uninitiated.[21] The beneficial side effects are readily apparent in Baumol's apt distinction between explicit premises or assumptions and transparent ones. The great power of mathematical expressions lies in their explicitness and economy. However, the implications of these terms, when they purport to relate to the real world via the equations of a given model, are often hidden from view. Thus their interpretation in ordinary language may often bring to light the full implications of a given set of assumptions and procedures in a way which cannot be accomplished by a purely mathematical statement.[22]

[20] Sidney Schoeffler (*161*), pp. 18ff. The employment of stochastic models wherein patterns of response are fixed only in a probabilistic sense would appear to overcome this objection. See, for example, the admittedly grandoise Markovian model for the analysis (only in principle!) of historical processes, as outlined by Franklin M. Fisher (*54*). Earlier descriptions of the general nature of stochastic models in economics include Trygve Haavelmo (*80*) and Jacob Marschak (*123*). Marschak's article is reproduced in Schoeffler's book. Schoeffler's criticism of the probability approach appears in Schoeffler, *Ibid.*, pp. 124ff.

[21] A small but growing number of econometricians seem to be endowed with sufficient patience and expository skill to reveal the workings of their models in readable English. See, for example, Christ, *op. cit.*, Clopper Almon, Jr. (*4*), Daniel B. Suits (*187*), and Jack Alterman (*5*).

[22] William J. Baumol (*8*), p. 94ff. One of Baumol's examples relates to the assertion that the supply or demand function, expressed in terms of price only, is homogenous of degree zero. As he points out, this assertion is persuasive in that it seems to argue only that the unit of currency chosen is of no economic significance. However, its uncritical adoption comes to grief when it is recognized that it implies that a doubling of all price quotations would not affect decisions even if the number of units of currency failed to double also. Only a fuller explanation of the assumption in question rather than merely listing the appropriate function as an element in the model, would avoid this kind of difficulty.

In short, the demand for greater explicitness, which is the road to simplicity for the technician, must be matched by a demand for transparency whereby the user can assess the true implications of the assumptions which are so neatly expressed in the form of equations.

This leads us to the requirement of theoretical plausibility. As Crossman puts it so well, a theory which can provide a basis for adequate prediction or forecasting must not only explain the static relationships among the relevant factors, but must also provide a quantitative description of their dynamic interaction and change through time. This is indeed a tall order.[23] The manifold difficulties to be overcome before such a theory can be developed are brought out in Schoeffler's critique of macroeconomic models. Schoeffler stresses the weaknesses of their theoretical underpinning, particularly in reference to their use in forecasting and prediction. He points out that the need for manageable equations reduces the number of variables whose effects can be determined, so that many influences are ignored or subsumed under a residual "error" term. Furthermore, the parameters in the given equations necessarily reflect past influences only; there can be no assurance that these will remain fixed in future time. Most damaging, however, is the fact that the equations reveal nothing concerning the chains of causation which underly the statistical estimates that are obtained. In other words, the macroeconomic approach still involves the manipulation of a black box whose inner workings remain hidden from view. Schoeffler concludes with an expression of qualified pessimism. While he recognizes that any judgment concerning the future development of econometric models is premature, he argues that in their present form, they cannot be expected to provide dependable forecasts.[24]

[23] E. R. F. W. Crossman (*33, 34*). The latter report provides a more detailed development of his thesis.

[24] Schoeffler *op cit.*, p. 116ff and 130ff. Of course, no other technique can do better, unless by accident. Comparisons of forecasts made judgmentally with those produced with econometric models are generally inconclusive, partly because these techniques are not really as distinct as they appear in formal descriptions. On this point, see George Jaszi et al. (*94*). In actual practice, the judgmental forecaster typically uses whatever quantitative data and techniques he deems appropriate, while the actual work of the model builder is infused with judgmental decisions at every step. For an enlightening comparison of the more "judgmental" British economic forecasting techniques with the more rigorous Dutch techniques, see R. L. Marris (*122*). Both Marris and Jaszi and associates recognize the superiority of the more rigorous econometric techniques "in principle and in the long run," while admitting their practical limitations at present.

The inability of existing theory to fulfill the demand for causal understanding poses a dilemma for the forecaster whose insistence upon methodological rigor outweighs the felt need for immediate results. For those with a more pragmatic orientation, however, the dilemma is avoided by recognizing that reliable forecasts can often be made on the basis of perceived regularities in the absence of a complete theoretical understanding of the underlying mechanisms. In other words, reliance on simple techniques of extrapolation will often yield good forecasts. Unfortunately, as we have already noted, such procedures assume structural stability and provide no basis for determining either the suitability of that assumption or the likelihood that a structural change will occur.[25] This problem is less serious in relation to hypothesis testing than in relation to forecasting, since hypotheses can often be tested over past periods known to be structurally stable. In practice, however, our inability to discover long periods of structural stability in the past hardly supports the convenient assumption of such stability in the future.

The requirement of explanatory ability amounts to the assertion that an equation system cannot be properly designated "econometric" unless it correlates with empirical reality according to certain rules of congruence. The internal, logical consistency of such a system is a necessary, but not a sufficient condition for its designation as a model. A further implication of this requirement is that the data to which the model refers must have a wider domain than that of the model's definitional properties; otherwise, the model can have no utility except as a descriptive summary of its internal structure. Obviously, the usefulness of any econometric model in forecasting is largely dependent upon its explanatory power. What is perhaps less obvious is that a model lacking significant explanatory power cannot be used to test hypotheses either, since such a test necessarily requires some extension beyond the model's definitional properties.[26]

The need for accurate coefficients, as described by Christ, is equivalent to the familiar statistical requirement of significant associations or correlations. It can only be met by the discovery of explanatory (independent) variables whose estimated (nonzero) coefficients

[25] The underlying structure must be partially known, of course; otherwise, the notion of structural change would be meaningless. For a more thorough exposition of this issue, see Marschak (*124*).

[26] A recent summary of the relations between theory and data in science is that of Henry Margenau (*121*). Margenau has expressed these principles in relation to a variety of behavorial sciences over the years. Also useful in this regard is the excellent article by Fritz Machlup (*116*).

have acceptably low estimated standard deviations. The search for such variables is of course a central concern of all applied scientific research. Unfortunately, none of the prescribed research practices can guarantee the discovery of independent variables displaying the sought-for regularities in association with the given dependent variable. Even when statistically significant and sufficiently precise coefficients are obtained, the question remains as to their economic significance. In terms familiar to social scientists, we can say that reliability of a measure does not imply its validity. The latter, like the question of economic significance, can only be determined on the basis of a broad theoretical and practical understanding of the subject area.

The last of Christ's catalogue of objectives for econometrics is the requirement of forecasting ability itself—an objective which, as we have noted, implies the others and is an ultimate test of their achievement. The ability to forecast within tolerable limits of error is essential not only to provide some notion of probable trends or outcomes of current decisions, but also to test hypotheses by comparing the model's prediction (reflecting a specified hypothesis) with the actual outcome. In short, some forecasting ability is essential if econometric techniques are to be useful either in policy-making or in the testing of hypotheses.[27] Here also, Schoeffler's comments express the current situation with respect to forecasting ability in blunt terms. He observes the familiar classification of econometric equations into four groups (technological, behavioral, legal, and definitional) and notes that stability over time, which is crucial in determining the reliability of a forecast, cannot be guaranteed for any of these, save for the definitional group, which is arbitrary to begin with.[28]

The advent of automatic data processing via computers has given rise to another promising projective technique—the development of projections by means of computer simulation. The essential idea is clearly described by Orcutt and associates as the construction of behavioral (or econometric) models whose elements comprise data on the observed behavior of a sample of individuals or household units (or firms).[29] Conventionally, the elements of a model are integrated according to the postulates of a given theory and the hypotheses derived therefrom. Since the model is designed to reflect the patterns of interaction postulated by the underlying theory, it can be used to predict the outcome of specified hypotheses derived therefrom. By using a microanalytic approach, the model can be constructed to reflect actual behavior of the microunits (individuals, households, firms, etc.) as they interact over time, rather than being confined to the dictates of a theory that has been adopted a-priori. Once such a behavioral model is operative, it can be used to simulate the probable responses of outcomes reflecting a variety of specified initial conditions, thus, in effect, testing a variety of hypotheses or a range of predictions.

The basic requirement for the construction of such dynamic models (given the requisite data processing facilities) is the availability of disaggregated data of sufficient quantity and quality to provide a series of comparable observations through time. As might be expected, the construction of a dynamic model (dynamic because it incorporates changes in its constituent elements occurring through time) involves some trade-off between the desire to maximize fidelity to the complexities of the real world as observed and the need to retain a model of sufficient simplicity to be manipulable. Greenberger's recommendations for handling this problem are threefold: First, the construction of the model requires the services of a subject-matter specialist working in concert with a computer programer. Unless the model to be used in simulation takes account of whatever peculiarities the data may contain, the resultant simulation is unlikely to be sufficiently realistic to have much practical value. The subject-

[27] As Theil points out, the problem of determining the accuracy of a forecast must be distinguished from that of verifying the forecasting procedure itself. The latter problem can be handled, at least in the case of point predictions, by testing the null hypothesis that the observed prediction errors are members of a parent population whose errors have zero means, etc. See Henri Theil (*193*), p. 30. In their discussion of Theil's paper (*Ibid.*, p. 47), Lehman and Knowles stress the further distinction between the accuracy of a projection and its usefulness in serving the purpose for which it was prepared.

[28] Schoeffler *op. cit.*, p. 130ff. Schoeffler's strictures lose some of their force when applied to current efforts at model construction, where greater attention is paid to the more limited inferences which can be drawn under conditions which only approximate the theoretical requirements of the given model. See, for example, Albert Ando et al. (*6*). For Schoeffler's detailed schema for scientific prediction in economics, see chs. 4 and 8 of his study.

Jouvenel's comments are pertinent in this regard. In summarizing Theil's analysis of the Dutch experience with econometric forecasts, Jouvenel indicates that the bulk of the forecasting errors disappeared when actual values for the model's exogenous variables were substituted for the projected values originally used in forecasting. Jouvenel concludes, therefore, that the problem of guessing the proper values to insert in the forecasting model remains a serious limitation in the use of such models for long-range projections. See Bertrand de Jouvenel (*95*), pp. 19ff.

[29] Guy H. Orcutt et al. (*143*), pp. 8-12. A more recent summary of current trends and problems in computer simulation in the behavioral sciences is James M. Beshers (ed.), (*13*).

matter specialist alone can provide the necessary information as to the peculiarities of the data to be used as inputs. Second, the model should be constructed incrementally, so that comparisons between the model and the data can be made at each stage, and new data be incorporated as they become available. Third, there is need for an on-line programing system associated, ideally, with a multiaccess computer installation, so that the researcher can enjoy easy and frequent access to the computer.[30]

The outlook for the satisfaction of these three requirements is highly favorable at the present time. The development of Fortran and similar computer languages, together with the rapid proliferation of "canned" programs, greatly facilitates the use of automatic data processing machinery by subject-matter experts. The major obstacle is the unavailability, at least in convenient form, of microdata (i.e., disaggregated data) in sufficient quantity for large-scale analyses.[31] If such data as are available prove to be adequate for this purpose, and if future data-gathering processes can be readily geared to the needs of microanalysis, computer simulation techniques offer great promise as a means for generating a wide variety of projections reflecting many different combinations of specified conditions and assumptions. Of even greater promise is the possibility, through computer simulation, of experimenting with changes in a variety of social and economic factors under conditions which approximate the potential impact of alternative policy decisions in the real world.[32] The inability of social scientists to carry out meaningful experiments on large-scale social systems has long been recognized as one of the fundamental obstacles to the development of social science. While no model can ever be a fully adequate substitute for the reality it represents, the patient accumulation of the results of experimentation with a variety of models, each of which captures a selected aspect of social reality, may eventually yield far more reliable information regarding the processes of change and interaction than can be hoped for through the analysis of aggregated data alone. In short, experimentation with models via computer simulation may represent a useful substitute where experimentation in the social system itself is impossible.

A further stage of possible development is already evident in the on-going research of Richard Stone and associates. Stone's efforts are directed toward the development of a system or hierarchy of econometric models representing, in their totality, the functioning of the British economy. As this system is developed, each submodel is designed to reflect the operation of some major sector of the economy. These submodels are linked together so that their exogenous portions acquire input from the composite model, and, in turn, provide output which modifies the exogenous portion of the main model at a later stage. One advantage of working with a number of submodels is that each of them can be kept at a manageable level of complexity. Another advantage is that subject-matter experts can be utilized most effectively in the construction of submodels relating to their particular specialties.[33] We shall return briefly to the far-reaching implications of this kind of long-range research in the concluding chapter.

The preceding discussion of techniques of projection has dealt exclusively with quantitative projections—at least, with projections which could be expressed quantitatively, even if arrived at judgmentally. To complete the discussion, it is necessary to consider one more technique, one which is designed to yield reasoned judgments concerning selected features of a possible future in qualitative terms. This approach has been termed the "Delphi technique," recalling the Delphic oracles. The basic purpose of the Delphi technique is to express a systematic pooling of expert opinion with respect to prospective outcomes of current trends and developments in a particular subject area.[34] The procedure involves, first, the preparation of a questionnaire relating to the subject area of concern. The

[30] Martin Greenberger (*68*), pp. 148 and 155f.

[31] J. E. Morton's recommendations are relevant in this regard. See J. E. Morton (*138*).

[32] The advantages of computer simulation in a variety of experimental research projects are summarized by Richard E. Dawson (*39*) and Guy H. Orcutt (*144*). Also see Diana Crane (*31*). Crane's article includes a useful bibliography on the subject. Suggestive indications of the directions which might be taken by computer simulation of socioeconomic behavior are contained in Herbert A. Simon (*175*, *176*). A more technical and very general summary of the potential uses of simulation is Andrew S. Scott (*162*). For a report of specific experience with such simulation in demography, see Jeanne Claire Ridley and Mindel C. Sheps (*152*).

[33] Richard N. Stone (*182*), p. 280ff. Preliminary results of an exercise designed to yield estimates of the supply and demand for labor in 1970 are given in Richard N. Stone and Alan Brown (*183*), p. 74f. For a brief account of the anticipated benefits of an economic model system for purposes of social and economic planning in Britain, see Stone (*184*).

[34] T. J. Gordon and Olaf Helmer (*63*). In this interesting study, the authors report the results obtained via the Delphi technique on a wide range of subjects, including such diverse matters as anticipated population growth, the prospects for peace, anticipated weapons development, anticipated progress in space exploration, and predicions of sicentific breakthroughs in a number of fields. For an excellent summary of this technique and its limitations, see Daniel Bell (*10*).

phrasing of the items on the questionnaire may consist of any combination of statements with which the panel of experts can express agreement or disagreement, or questions which they can answer by selecting the most appropriate of the responses provided, as in a multiple-choice examination. The respondents may also be permitted to write in their views or answers to specific questions. Next, an appropriate panel of experts in the particular area of concern must be selected. When the panel members have received, filled, and returned their questionnaires, the first phase of the operation has begun. The results of this first phase are then pooled, coded, and averaged. For the second round, the panel receives the pooled results of round one, and those whose initial responses differ markedly from the group norm are asked either to modify their original response if they wish to do so, or to provide a defense of their position by stating their reasons more fully. These responses are again pooled and circulated once more. The cycle can be repeated as long as the process yields significant changes in the expressed opinions.

The Delphi technique is an outgrowth of the long-term interest of Helmer and Rescher in the development of projective techniques appropriate to the inexact sciences. Its chief advantage is its avoidance of time-consuming committee meetings which are the conventional substitute for this kind of intellectual interchange. The avoidance of meetings affords a further advantage in that it minimizes the effect of psychological pressures commonly induced by a committee setting, such as the familiar "band wagon" effect (giving rise to spurious consensus, or what might be called the "hail-to-the-chief" syndrome). A further distortion that is sometimes observed in the committee process is the rigid defense of publicly-expressed views because their subsequent modification is thought to imply lack of conviction or egregious error. In addition, the repeated circulation of pooled responses to all panel members is, in itself, a powerful educational device which serves to create a common fund of knowledge and opinion on which to base one's forecasts. The committee setting affords no opportunity for the leisurely contemplation of divergent views, so that its educational value is severely limited.

The authors recognize certain difficulties with this technique, to which may be added one or two criticisms of our own. They admit to the difficulty of deciding, initially, who is an expert. Persons who enjoy the highest professional reputations in a given field may be fully in tune with the present, but they are not necessarily harbingers of the future. Particularly when seeking long-range forecasts, it may be desirable to obtain a mix of older and younger minds, so that exclusive reliance on the criterion of established reputation may be self-defeating. Further difficulties stem from the inevitable delays in obtaining responses from laggard panel members, and the attrition of the panel itself.

A more serious problem is the design of the questionnaires themselves. In the Gordon-Helmer study, the questionnaires consisted mostly of lists of statements expressing possible future developments in particular subject areas. The panel members were asked to check one of several boxes expressing their opinion as to the effectiveness of the development in question in achieving a specified goal (the categories offered ranged from nil to high). They were then asked to express their opinion as to the probability that the specified development would occur by checking "never", "maybe", or "certain". Where appropriate, similar categories were offered relating to the expected time that the development in question was deemed likely to be achieved, its overall desirability, and the like.[35] Although the composition of these items reflects the careful work of experts in the fields of concern, the desire to minimize the use of the panel experts' time, and the interest in facilitating the construction of averages and similar summaries were met at the expense of open-ended questions which might have permitted qualified responses. One problem with exclusive reliance upon box-checking or multiple-choice questionnaire techniques is that they invite rapid, off-the-cuff responses which may lack the careful reflection which is actually called for. A further problem which the authors recognize is that both the questionnaire items and the response alternatives provided are too brief and categorical to permit serious qualification. Hence there exists considerable ambiguity and imprecision in both the stimuli and the responses they elicit.

Finally, it should be recognized that the conventional committee setting for which the Delphi technique is a substitute offers a number of advantages as well as disadvantages. The interplay of judgments and influences that occur in face-to-face association can be a valuable adjunct in the development of reasoned views on a given subject. Only through such contact can each member of the group perceive the nuances of meaning and intensity of convictions which are of vital importance in assessing the true significance of expressed ideas. Much of this contextual matter is lost when we resort to written communication. Nevertheless, in a world whose experts are always in short supply and subject to heavy demands, the Delphi technique is

[35] Gordon and Helmer, *Ibid.*, appendix.

certainly a worthwhile approach to consider in eliciting some systematic expression of considered views on the future prospects in any area of interest.[36]

In concluding this discussion of techniques of projection, it may be helpful to return to a brief consideration of first principles. It is axiomatic that no projection can be more reliable than the data on which it is based. Much of the criticism and controversy surrounding long-range projections involves differences whose magnitude is smaller than that of the band of error surrounding the underlying data. If, for example, the estimated current population of working ages reflects an undercount of, say, 3 percent, the derived labor force, ceteris paribus, will have a similar error. It follows that a projection of the population and labor force will reflect that much error as a minimum, so that alternative projections which differ by that amount cannot be differentiated meaningfully. In other words, even our point predictions should be regarded as interval predictions wherein a range of values are equally consistent with a given set of assumptions.[37]

The problem or errors in the original observations is far more extensive than merely the undercount of the population in a given census. All observations of social and economic data are afflicted with uncertainty due to errors of measurement and classification. Morgenstern's criticism of economic research activities in general and economic forecasts in particular is fundamentally a criticism of the widespread tendency to ignore or underestimate these errors in arriving at results which are spuriously precise. His concluding recommendations are especially incisive: First, good data are needed if economic (or any other) theory is to enjoy any testable relevance to the real world. Second, if the errors found in the data are attributable to inherent difficulties of observation and measurement, they can be reduced only by a gradual improvement of the pertinent methods; but if they are due to intentional falsification, faulty memory, or the inability to obtain truly random samples for observation, the data in question must be discarded or interpreted in the light of a statistical theory (yet to be developed) which could estimate and correct for these distortions. Third, far greater effort must be expended by data-producing agencies to estimate and publicize the errors associated with the statistics they issue. Finally, economists (and, by implication, other social scientists) must devise more flexible techniques for handling data with a wider range of accuracy, rather than proceeding on the assumption that their data are sufficiently accurate to warrant the analytical techniques that happen to be currently in favor.[38]

For the practical forecaster, Morgenstern's critique contains little that is new or particularly shocking. As noted previously, the forecaster might usefully apply the estimated band of error surrounding his basic data to the forecast values themselves, thus obtaining a kind of minimal interval estimate instead of a point prediction. He might also draw certain comfort from the realization that the researcher cannot escape uncertainty merely by confining himself to the past and present.

In the next chapter, we turn to problems related specifically to long-range projections.

[36] More recently, Helmer (Helmer-Hirschberg) has linked his proposed use of the Delphi technique to a far more sweeping proposal for the establishment of an institute for social planning, similar to that proposed (but in softer terms) by Bertrand de Jouvenel (*op. cit.*, chapter 20). See Olaf Helmer-Hirschberg (*89*). The initial ideas for this technique and its application were developed by Helmer and Rescher (*88*). The findings of an earlier study on the use of experts in developing forecasts are informative in this regard. See A. Kaplan et al. (*97*). These authors found that group discussion and the requirement that specific predictions be justified or explained by the predictor led to improved predictions.

[37] A classic analysis of errors in decennial census coverage is Ansley J. Coale (*27*). A careful analysis of the coverage of the 1960 census is that of Jacob S. Siegel and Melvin Zelnik (*172*). The recommendations of Joseph Steinberg following the Siegel-Zelnik paper, p. 86ff, are noteworthy for their implications regarding demographic and labor force projections. For a more detailed discussion of the policy implications of census undercoverage of particular population groups, see Jacob S. Siegel (*173*).

[38] Oskar Morgenstern (*135*), p. 302ff. For a brief summary of this work and its severe recommendations, see Morgenstern (*136*). Morgenstern's criticism of data-producing agencies, particularly those of the Federal Government, for their failure to concern themselves with errors in the data they publish met with a sharp rebuttal by Raymond T. Bowman (*16*). Bowman's most telling point is his long but partial listing of government publications relating specifically to errors in the data produced by the particular agencies.

II. MAJOR PROBLEMS IN LONG RANGE PROJECTION

The development of long-range projections gives rise to a variety of problems, some of which are primarily technical while others involve broad philosophical issues and questions of policy. Furthermore, these problems are commonly interrelated, so that solutions which are technically feasible may be problematic for reasons of policy, or on philosophical grounds.

In considering, first, the technical problems, it may be helpful to distinguish four major problem-areas: (1) The statement of the assumptions; (2) the selection and evaluation of the data; (3) the treatment of the data; and (4) the determination of the number, range, and time span of the projections themselves.

A. *Statement of the assumptions*

The most common plea of those who seek to understand how a projection was developed in order to use it intelligently is that the underlying assumptions should be made more explicit. Since technicians are usually greatly concerned to do likewise, it is surprising that they seldom meet the demands of their critics in this respect. One difficulty may rest in the meaning of the term "explicit." Its ambiguity can be illustrated by means of the conventional statement of assumptions to be found in many publications of long-range projections of the labor force.[39] The statement typically asserts the assumption that "there will be no major war, prolonged depression, or other profound disturbance during the period covered by the projection." Such a statement is formally explicit in the usual sense of the term, but when the user or critic demands explicitness, he is asking for more than the use of clear language and an open statement of the ceteris paribus assumptions. Ideally, he wants to know the precise effect of the adoption of the assumption in question on the resultant projection. In other words, to use Baumol's distinction once more, he wants the assumptions to be transparent as well as explicit. This ideal can only be met when an assumption can be expressed as a quantitative element in an equation, so that the effect of retaining, removing, or altering the given assumption can be readily measured. Short of this ideal, the expression of assumptions can only be improved by the exercise of greater care in their formulation. One can argue that the kind of statement referred to above is essentially a way of saying that a long-range projection is usually prepared on the assumption of underlying structural stability. If so, it could only be made more explicit by means of a detailed description of the specific structural elements whose continued stability is assumed, together with some estimate of the probable impact upon the projection as a whole of a failure of any of these elements to remain stable over the projection period.

When we consider long-range projections of labor force in this context, the demand for greater explicitness of assumptions is, in effect, a demand for a research effort of unprecedented scope. Consider, for example, the projection of labor force participation rates for young adult men and women age 16 to 19 years to, let us say, 1985. In developing such a projection, conventional BLS practice has been to examine past trends in the worker rates of the major subgroups of this population. These subgroups are themselves defined, within the limits of available data, in terms of major social, demographic, and economic characteristics which have been associated in past experience with important differences in the propensity to enter the labor force. The more obvious of these characteristics are age, sex, school enrollment, and, for women, marital status and the presence of young children. According to the usual BLS procedure, the projected labor force for this age group in 1985 would be prepared by the following procedure:

Step 1. Since, in 1967, not all of the individuals who will be 16 to 19 years old in 1985 have yet been born, it is necessary to review existing population projections for this group in the light of available evidence concerning current trends in fertility, in order to select the most plausible population series to be used as a base in the projection.

Step 2. The group as a whole is then classified into the several subgroups defined by the major differentiating characteristics noted above. This categorization is limited, of course, by the available data. It is important to observe, at this point, that the pertinent characteristics of this age group (its school enrollment, marital status, etc.) relate to 1985, not to the present. This implies that separate projections must be made of each of these characteristics to the target date, or existing projections must be utilized. Common BLS practice has been to utilize the available projections of

[39] See, for example, Sophia Cooper and Stuart Garfinkle (29), p. 24 and the National Planning Association (142), p. 2. Grunberg's caustic remark on this type of general caveat so favored by forecasters is simply that "an economic forecast made under the condition that neither a depression nor an inflation shall occur is of little predictive power." See Emile Grunberg (76). More recently, the Bureau of Labor Statistics has attempted to provide more detailed statements of the assumed conditions underlying some of its projections. See, for example, Howard V. Stambler (181).

the Bureau of the Census in this work so far as possible.

Step 3. Past and current trends in the rates of labor force participation of each subgroup are then obtained and extrapolated to the target date.

Step 4. Past and current trends in the distribution of the total group among the several categories, as projected to the target date in Step 2, are then combined with the projected worker rates obtained in Step 3 to obtain a projected labor force for each of the components of the total group.

Step 5. The projected labor force of each subgroup is then combined to yield a projected labor force for the total age-group as of the target date.

When the demand for a more explicit statement of assumptions is considered in the light of the above account of conventional procedures, it is clear that the pursuit of greater explicitness can only be terminated arbitrarily, or by the limits of available time, patience, and resources. In the above illustration, one might state that it is assumed that men and women in this age group will continue to pursue formal education through the equivalent of 4 years of high school for the overwhelming majority and on into college for a growing minority. Further, it is assumed that a significant minority of young women in the age group will continue to marry and begin childbearing. It might well be added that the projection assumes no radical change in the structure of financial assistance offered in support of educational activity, so that substantial proportions of students in this group will continue to seek and find paid employment while going to school. It is also assumed that there will be no radical change in either the desire for marriage, the desire for children, or the ability to avoid childbearing among these young people. This implies an assumption of stability in values and motivations regarding marriage and family growth. Further, it is assumed that prevailing conditions of work, study, leisure, prevailing definitions of these activities, and prevailing opportunities for choosing among them will remain substantially unchanged. Finally, it is assumed that the prevailing definitions of the labor force as it relates to this age group will remain in force.

At this point, one could take up each of these assumptions so as to reveal the further assumptions which they entail, in an effort to justify their adoption in preference to possible alternatives, and so on ad nauseum. The point to be understood here is that one must consider the very real limitations of time, resources, and publication space available to those who prepare projections, and the equally real limit to the amount of detail of this kind which can usefully be absorbed by the users of a projection. One virtue of this kind of detailed recital is that it alerts the user of the projection to the network of assumptions which is all too easily overlooked when he quickly passes over the glib assertion that the given projection assumes that no radical structural change will take place. However, no matter how detailed a verbal account of assumptions may be, it cannot provide the user with the kind of information he needs in order to assess the real significance of the projection. For this purpose, he would need to know, in quantitative terms, the effect of different parameters relating to each of the assumed characteristics upon the resultant projection, together with the probabilities relating to the likelihood of continued stability of the factors to which each of the assumptions relates.[40]

The above considerations bring us to the second point in regard to the demand for more explicit assumptions—namely, this is in effect a demand for more quantitative procedures. As such, it can only be met to the extent that factors which are believed to be relevant to a given projection can be expressed quantitatively. In seeking to accomplish such quantification, the technician runs the very real risk of imposing an artificial structure upon the data. If, on the other hand, he eschews consideration of non-quantifiable factors in order to avoid the charge of subjective bias or imprecision, he runs the still greater risk of ignoring significant factors which affect the phenomenon he seeks to project. The heroic solution to this dilemma is of course familiar to every graduate student in the behavioral sciences; if a factor of alleged importance is in fact significant, its impact must be measurable. Hence only those factors whose impact is measurable should be regarded as significant. Unfortunately, where the available data are of limited scope and quality, it

[40] A further complication, particularly in the case of long-range projections, is the reversability of causal sequences among associated factors. For example, conventional projections of labor force supply assume a given population projection as a starting point. However, fertility, mortality, and, a fortiori, migration are all affected by the economic situation, which therefore influences the labor force directly, via job opportunities, and indirectly, via longrun demographic changes. We are thus confronted by a causal network rather than a causal chain. The two-way relationship between population growth and economic development has of course been recognized in a large number of studies. See, for example, Irma Adelman (*2*), Robert M. Dinkel (*45*), and Stephen Enke (*50*). An excellent summary of the development of theory relating to demographic-economic interrelations is A. L. Levine (*108*). For a recent analysis reflecting some of these interrelations, see Peter E. Haase (*79*). An early exposition of population change as a factor in labor force growth is the classic study by Seymour L. Wolfbein and A. J. Jaffe (*204*).

is necessary to rely upon a priori theory, experience, and even intuition in order to determine the significance of factors whose operation is hidden from view. It is sometimes possible to incorporate nonnumerical elements in the form of "dummy" variables and thus include, in some sense, factors which would otherwise have been excluded because they could not be expressed quantitatively, but this parctice is not always adequate or feasible.[41]

A third point concerning the demand for more explicit assumptions relates to the criteria whereby clarity is to be determined. Here, we encounter the dilemma discussed previously—a carefully worded statement of assumptions such as those described in regard to a projection of the labor force of 16- to 19-year-olds may be perfectly clear and explicit to the practical user, while remaining full of ambiguity to the technician. Conversely, a statement which is adequately explicit to the technician will commonly mystify anyone lacking the requisite training in quantitative methods and terminology. In short, it is necessary to recognize that projections are used and must be understood by non-technicians whose demands for explicit statements of assumptions cannot be satisfied by a list of equations.

B. *Selection and Evaluation of Data*

As noted previously, reliance upon simple extrapolative techniques in developing projections offers one important advantage—such a projection can be obtained on the basis of past trends in the phenomenon being projected, without resorting to an analysis of its underlying mechanisms or causal processes. However, the ultimate interests of neither the user nor the technician can be met in this manner, since it does not provide an understanding of the factors which determine the temporal movement of the phenomenon in question. Thus attention is turned to the discovery of factors associated with the phenonemon of primary interest, and to the measurement of their impact. At this point, the need for theory to guide the analyst in the selection of relevant variables is paramount. In the absence of adequate theory, and given the availability of automatic data processing equipment of growing versatility, the analyst can now feed all his data into the hopper and passively await the computer printouts which will tell him which variables are significantly associated with his dependent variable. The only difficulty with this approach (apart from the very real possibility that the analyst can be overwhelmed by the computer output) is that the occurrence of associations cannot be understood or explained without resort to theory. Furthermore, analysts who seek to derive conclusions on the basis of associations discovered in this manner tend to engage in the dubious practice which Professor Mosteller used to deride as "capitalizing on chance." That is, they seek to attribute significance to meaningless associations that are bound to appear eventually, given a sufficient mass of input, particularly when the observations relating to the several variables are not entirely independent of one another, as is common in economics.

Unfortunately, theory relating to the factors influencing the entry or withdrawal of persons from the labor force is not entirely adequate for the needs of long-range forecasting. Furthermore, the kind of information that is available itself reflects the paucity of relevant behavioral theory, so that the analyst is forced to work with economic data primarily, even if he suspects, as does the present writer, that the major influences on the propensity of persons to enter or leave the labor force are social and psychological as much as economic in nature.[42] In short, the development of a more adequate basis for labor force projections is hampered by the relative absence of relevant noneconomic data.

In this situation, two courses of action may be recommended: First, it is necessary to work with what is available in the knowledge that both our failures and successes will prove instructive. Second, it is necessary to develop a more adequate behavioral theory relating to labor force activity, on the basis of which we can specify the kinds of information needed. This implies a need for far greater collaboration between economists, who have dominated the field largely by default, and other social scientists. We shall elaborate on this point in the concluding chapter.

C. *Treatment of the Data*

In making projections by extrapolative techniques, one encounters all of the problems associated with time series, sooner or later. The first of these is the problem of changing definitions and changing coverage which impair the comparability of data through time. In working with population estimates for the United States

[41] This is not always a happy solution, since most qualitative factors, by their very nature, are not readily classifiable as merely present or absent in a given instance.

[42] Despite the limitations of available theory and the even more serious limitations of available data, a number of analysts have made significant inroads toward explaining the factors affecting the labor force activity of particular subgroups of the population. See, for example, Glen Cain (*23, 24*), and Jacob Mincer (*129, 130*). Professor Mincer is continuing his research into the relation between labor force participation and unemployment. Among the others who have made important contributions in this area are Clarence D. Long (*113*), Alfred Tella (*191, 192*), and W. Lee Hansen (*82*).

over the postwar period, for example, a number of elementary but easily overlooked examples come to mind. Do the estimates include or exclude Alaska and Hawaii throughout the period of interest? Are the worker rates based on the total population, including Armed Forces abroad, or are they based on the total resident population, or the total civilian population, or the civilian noninstitutional population? Similarly, the recent change in the definition of the population of working age from 14 and over to 16 and over necessitates caution in comparing statistical aggregates through time.

A second problem relates to the treatment of observations that are out of line or atypical in a given time series. In developing an extrapolation, the technician is confronted with a decision here, and one which requires the exercise of considerable judgment. He can simply retain all observations as given (thus avoiding a judgment that may be both complicated and hard to defend). However, he thereby runs the risk of seriously distorting his results. Alternatively, he can examine the historical circumstances surrounding the deviant observation to determine whether it can be explained by actual social or economic conditions or must instead be attributed to chance fluctuation or a peculiar combination of distorting factors. Whatever the outcome of this examination, the technician must then decide whether the observation in question should be accepted and given its full weight, assigned a reduced weight, smoothed into line, or left out of consideration entirely.

A familiar example can be seen in the labor force participation rates of men 20 to 24 years old in the immediate postwar period (1947–52). These rates rose very rapidly from 84.9 percent in 1947 to 92.1 percent in 1952—an increase of 7 percentage points in 5 years. In extrapolating these rates (which have declined slowly and irregularly since 1953) the technician must decide whether the rise in the 1947–52 period should be allowed to influence the extrapolation proportionately or whether it should be left out of consideration entirely. If he chooses the former course, he extrapolates on the basis of a trend line through the observed rates for the 1947–66 period, obtaining a constant or slightly increasing trend into the future. If, on the other hand, he leaves out the 1947–52 period, his extrapolation, derived from the trend for the 1953–66 period, will yield a gradually declining rate of labor force participation for this group.

In the above example, the analyst can readily surmise that the postwar enrollment of veterans under the GI bill held down their worker rates, and hence accounted for the low 1947–48 rates of the 20-to 24-age group as a whole. He would therefore be likely to discount the increase from 1947 to 1952 as reflecting an unusual condition which he would not want to incorporate as one of the assumptions in his projection. Unfortunately, not all of the quirks in a statistical time series are so readily explained or dismissed, so that the role of expert judgment is crucial. This implies, first, that the technician must either possess a thorough knowledge of the subject matter with which he deals in preparing a projection, or be able to work closely with subject-matter experts. Second, the judgments which are exercised in making projections must be recognized as their most essential component. Except for mechanical errors or trivial differences in procedure, any projection can only be criticized in terms of its underlying assumptions and the judgments that have been made at critical stages in its development.

A third problem regarding the treatment of long statistical time series is the relative weight to be given to earlier and more recent observations. Here again, the simplest and most easily defended procedure, that of equal weighting throughout, may not always be the most judicious. It is possible, for example, that the more recent observations are obtained through improved procedures or from a better sample, and thus provide a more accurate picture of reality than the earlier observations. The analyst must also consider the possibility that one or more periods within the entire time series may reflect underlying structural changes which are inconsistent with the assumptions of his projection. An obvious example is the projection of labor force participation rates for a population group whose worker rates are sensitive to the economic situation. In such a case, a projection derived from rates observed during a recession period would be inconsistent with an underlying assumption of high employment levels. In developing such a projection, it would be necessary to discount or adjust the group's worker rates during the recession period. Once more, the crucial role of judgment is evident.[43]

The above problems are multiplied when simple extrapolative techniques are abandoned in favor of more complicated analytical methods. In fact, a fundamental reason for the hesitation of experienced

[43] This problem, and the development of techniques to overcome it has been a central concern of Thomas Dernberg and associates. See Thomas Dernberg and Kenneth Strand (*41*) and Thomas Dernberg, Kenneth Strand and Judith Dukler (*43*). For a brief summary of their argument, see Thomas Dernberg and Kenneth Strand (*42*). Concern with the possibility of hidden unemployment during economic downturns is not recent. For an earlier analysis of this phenomenon, see Harold Wool (*205*).

econometricians to prepare projections beyond short-range periods (3 to 18 months ahead) is their knowledge that such projections entail separate projections of most of the variables in the econometric equation system or model. In such a case, the judgments employed by the econometrician are likely to be even more numerous and complex than those of the judgmental forecaster using simple extrapolative techniques.[44]

D. *The Number, Range, and Timespan of the Projections*

Decisions relating to the number of alternative projections to be made, their range (i.e., the extent to which the alternative series differ from one another) and their timespan (how long into the future they extend) can only be reached upon a careful consideration of the data and resources available to the technician and the purposes which the projections are intended to serve. In its most general sense, a projection is some function of the available data, so that a consideration of past trends and variations in the relevant data provides a good initial notion of the number and range of projections which are needed to capture the likely variations in the future. For example, the trends and fluctuations in the labor force participation rates of men and women 35 to 44 years old over the 1947–66 period reveals a range of 0.9 percentage points with a high of 98.2 percent for the men. For the women, the range is 10.6 percentage points, with a high of 46.9 percent. Obviously, projections relating to the women in this age group should be greater in number and/or far wider in range than those for the men in the corresponding age group.

The question of the number of projections to be prepared is problematic. In preparing population projections, the experience of the Bureau of the Census has been that two alternative sets (or series) of projections tend inevitably to be labeled "high" and "low" or even "optimistic" and "pessimistic", with the obvious pejorative connotation of the latter term affecting the decision to adopt the series so labeled. Three alternative sets or series are little better, since the users then feel an overwhelming pressure to adopt the middle one. More recently, therefore, census projections of population have appeared in four series, labeled with the greatest possible neutrality "A", "B", "C", and "D."[45] This has afforded the several government agencies who use the projections the great satisfaction of expressing a conservative optimism by adopting series B, the second highest of the four.

An alternative approach to this question is offered by the National Planning Association, which has prepared economic projections in three series—a target or goal projection, a present policy projection (derived by an extrapolation from past trends), and an intermediate judgmental projection, reflecting the attainments which are deemed the most likely outcomes of the compromises to be expected among the inertia of custom, the pressures of conflicting goals, and aspirations toward higher achievement.[46] This approach is far more useful to the user of the projections, particularly in the formulation of policies, since it affords a plausible description of a goal to aim for together with some notion of the probable "gap" between the goal and what might be expected to ensue in the absence of a directed effort. This gap is, in a sense, a measure of both the need for a particular program and an estimate of its maximum potential effectiveness. As such, it can be useful to the policymaker as an expression of the magnitude of a given task and the stakes involved.

For the technician, the instruction that he is to prepare two, three, or four projections is hardly a solution to his problems. He must still select from among a very large number of assumptions those few combinations which best reflect the alternative conditions which are deemed worthy of separate projection. The nature of this task can be readily appreciated by considering long-range population projections. The population of an area is of course a function of only three variables: fertility, mortality, and net migration. In view of observed past variations in these variables, one might decide to work with, say, four sets of fertility assumptions, three sets of mortality assumptions, and three sets of migration assumptions. In their several combinations, these assumptions would yield 36 sets of projections, a number which is obviously excessive for any practical purposes. Of course, it is likely that many of these combinations would resemble each other closely, but the point to recognize is that the task of the technician is not limited to spinning out the impli-

[44] It is only fair to add that experienced econometricians are unlikely to take long-range projections seriously for the same reasons that they are reluctant to try to make them—the necessary assumptions must be determined judgmentally and are difficult to quantify or defend. On this point, see John R. Meyer and Robert R. Glauber (*128*), pp. 240–257. For a defense of the role of judgment by an economist whose qualifications as scientist and econometrician are above reproach, see Nicholas Georgescu-Roegen (*58*), pp. 274f.

[45] Jacob S. Siegel, et al. (*op. cit.*, 1964).

[46] National Planning Association, *op. cit.*, p. 2. For an excellent account of the rationale behind the NPA's alternative sets of projections, see Sidney Sonenblum and Louis H. Stern (*178*).

cations of a large number of alternative assumptions. He must at some stage act decisively in reducing their number to manageable size, and in doing so, must exercise judgment as to which sets of assumptions best illustrate the plausible range of expectations regarding possible futures. In fact, he may be expected to push his selective process to the limit, inasmuch as he will often be asked to identify the single most likely projection—in other words, he will be asked to make a forecast. It is at this point that his technical expertise is of little help, and his general familiarity with the subject-matter, viewed in its broadest social and historical context, becomes an essential asset. One implication of the foregoing discussion stands out: No matter how many alternative series may be projected, the practical needs of the users will usually require the selection of a single series for use in the formulation of policies and programs. That selection requires the closest cooperation and mutual understanding between the technician and the policymaker.

Two considerations are paramount in determining the timespan of a projection. The first is the purpose which the projection is designed to serve and the second is the degree of uncertainty that is acceptable. Here again, some kind of trade-off must be achieved between a relatively confident projection that extends over too short a period to allow for practical use and a less precise projection covering a longer timespan. When we consider the rapid expansion of the band of uncertainty surrounding any projection as it is carried farther into the future, it is indeed questionable that any long-range projection is worth developing. However, their continued development is assured by the growing demand for long-range planning in all sectors of our social existence.[47] Furthermore, such plans are typically developed with sufficient flexibility and generality that a considerable range of error in the projected magnitudes can be tolerated.

In the manpower field, the timespan of projections is dictated by the leadtime required to plan and establish the educational and training facilities needed to meet tomorrow's requirements for highly qualified personnel. In the anticipating future needs for highly trained manpower, such as scientists, engineers, and physicians, "tomorrow" means 10 to 15 years in the future. Even greater leadtime is required in planning for financial and institutional support of such programs as the Social Security system and other private and public pension and retirement systems.

The broader philosophical issues which arise in regard to long-range projections relate to their purposes, their uses, and their social consequences. Since these considerations are interrelated with questions of public policy, they must be viewed in the context of social and political action designed to maintain the society and guide its progress in a changing world. Perhaps the most fundamental issue is that of the basic model to be used as a guide in developing long-range projections. The central argument of this essay is that long-range projections cannot adequately fulfill their most important functions if they are prepared solely in accordance with the dictates of scientific method, with the ultimate goal of achieving predictive significance. This is not an easy argument to defend, particularly in an age which equates truth and rationality with science. However, this argument is not an exhortation to abandon scientific procedures. It is rather a claim that the act of projection must have an extra-scientific aim. The chief value of a projection is its delineation of possible futures, not its conditional prediction of more-or-less likely outcomes. Projections should serve to open up possibilities and explore their consequences, not reduce them to a single, seemingly inevitable path. The use of scientific methods is indispensable in assessing the range of possibilities and determining the probable consequences of alternative courses of action. But insofar as science seeks to discover either deterministic or probabilistic laws on which predictions can be based, its ultimate purpose differs radically from that of projection. This difference and its significance have been aptly summarized by Bertrand de Jouvenel, when he points out that the ability to predict the future implies its inevitability, which in turn removes the need for projection. For this reason, he argues, our interest rests with uncertain phenomena, since only in the realm of the uncertain can we act to influence our destiny.[48]

The advocates of greater scientific rigor in the development of projections encounter a further difficulty when they consider the impact of public projections upon that public. That is, they encounter the dilemma with which social scientists are familiar—the public (i.e., social) expression of the predicted behavior of

[47] On the need to recognize this demand as a necessary and legitimate function of government, see Jan Tinbergen (*197*), pp. 74f. See also the remarks of Henri Theil (*op. cit.*, 1960), Alfred Sauvy (*158*, *159*), and Richard A. Lester (*107*).

The advantages of medium-range manpower projections (extending about 5 years into the future) are well described by T. R. Crossley (*32*). For employment forecasts in particular, Crossley argues that the 5-year forecasting period affords sufficient time for effective implementation of specific policies together with sufficient precision for meaningful manpower planning.

[48] Bertrand de Jouvenel, *op. cit.*, p. 52.

the members of a society becomes an added element in the determination of that behavior, which then causes some change in the behavior from what it would otherwise have been. In short, we are confronted with the problem of the self-fulfilling and self-denying prophecies. In considering this problem, it is important to bear in mind that it is only problematic for those who so identify projections with scientific procedures and goals that they uphold the scientific aim of successful projection. Obviously, a projection which is either self-fulfilling or self-denying cannot meet this criterion. However, when projections are viewed as statements of social goals or as descriptions of possible futures which may be regarded as harmful or threatening, their self-fulfilling or self-denying character becomes potentially useful. If a projection serves to raise aspiration levels so that the attainment of desired goals is realized, or if it provides advance warning of impending difficulties so that plans are set in motion to avoid these difficulties, it has fulfilled its most important function—one that bears little relation to the predictive aims of science.

Those who espouse a rigidly scientific conception of projections are led into a consideration of the above problem in terms of highly convoluted speculations regarding a potentially endless chain of stimulus and response, in an effort to incorporate the probable response to a public projection into the projection before it is made public. One consequence of such speculations is the assertion that long-range projections of manpower should be discouraged or at least regarded with great suspicion, since their consequences cannot be foretold, and may be harmful.[49] Other partisans of scientific rigor have arrived at the optimistic conclusion that it is possible, at least in principle, for the technician to incorporate in his projection the probable response of the public to his announcement of the projection, thus avoiding the dilemma of self-fulfilling or self-denying statements.[50]

On the basis of our argument as to the proper task of projection, the above view is subject to two criticisms. First, predictive accuracy is neither a basic goal nor the sole criterion of the worth of a long-range projection. Hence, the attempt to incorporate into a projection a chain of anticipated reactions to it implies the sacrifice of a major objective for the achievement of a minor one.[51] A second and more serious objection is that any public projection loses its effectiveness either as forecast, target, or warning insofar as it comes to be regarded as an effort at manipulation or propagandizing. The best way to avoid the suspicion of manipulative intent is to avoid all attempts to manipulate. This means that target or goal projections must be clearly announced as such. There purpose is to inform the public of the possibilities inherent in the society. A projection is manipulative only if it is designed to create an artificial atmosphere of progress and prosperity, or to exhort the public to adopt certain goals by concealing or downgrading other alternatives. The attempt by any government agency to alter its projections by incorporating the anticipated reactions of the public to its announcement might, in theory, improve its predictive accuracy, but it would hardly contribute toward easing the credibility gap.

Even if we grant that the procedures followed in conventional scientific investigations do not afford an adequate paradigm for the construction of projections, the fullest possible use of scientific methods remains the best protection we have against the tendency to reduce all projections to exercises in politically-inspired wish fulfillment. Indeed, many of the functions of projections call for no more and no less than the utilization of appropriate scientific methods.[52] For example, projections may be developed as a quasi-experimental substitute for the kind of hypothesis testing which occurs in a laboratory setting. The parameters of the independent variables of a given equation may be set in accordance with a given hypothesis and then changed to reflect a number of alternative hypo-

[49] W. Lee Hansen (83), pp. 13ff. Hansen's comments are followed by an excellent rejoinder by Sol Swerdloff, pp. 25ff. It is an unfortunate truism that all knowledge may be misused, including the uncertain knowledge which takes the form of projections. On the other hand, the concealment of knowledge is almost certain to have harmful consequences.

[50] Emile Grunberg and Franco Modigliani (77). Compare the apt remarks of George C Smith (177). In sociology, the problem of the self-fulfilling prophecy and of reflexive predictions generally has long been recognized. See, for example, Robert K. Merton (127).

[51] Roger C. Buck (22). In their reply to this article, Grunberg and Modigliani point out that their theorem expressing the possibility of incorporating the anticipated public reaction to a prediction in the prediction itself does not imply an endless regression of action and reaction, nor does it require that such a regression be convergent. All that is required, they assert, is that the public reaction is a known and continuous function (termed the "reaction function") of the stated prediction, and that the predicion itself has finite and real upper and lower bounds. The authors conclude that the possibility of correct public prediction exists whenever correct private predictions can be made, and that the very real difficulties in the latter sphere are "unrelated to their potential reflexivity." See Emile Grunberg and Franco Modigliani (78).

[52] On the dangers of allowing projections to be reduced to mere rationalizations of current policies, see the comments of Gerald Colm (28), pp. 38f.

theses. The more plausible of the resultant outcomes might then be regarded as a confirmation of the hypothesis expressed in the underlying equation. Similarly, any projection is, in its purest sense, merely an elaboration of the consequences entailed by a specified set of assumptions. As such, its development must not be influenced by non-scientific considerations. In short, the proper but crucial role of value considerations in the development of projections is not too different from its role in scientific undertakings. Initial value considerations dictate the selection of assumptions to be expressed in the projection, just as they dictate the selection of problems to be investigated in science. Then, after the technical work has been carried out, values determine how alternative projections are to be identified, used, and interpreted, just as the findings of a scientific investigation find their chief practical usefulness only in terms of the values they help to advance. Thus, the proper role of values in the development of projections can best be understood by recognizing that the most important purpose of long-range projections is to express our values within a framework of environmental constraints so that their attainability can be assessed and realistic priorities established. However, the projective techniques must themselves be "value-free" in that they must utilize scientific procedures to the fullest possible extent.[53]

A final issue in regard to long-range projections is primarily a matter of policy, though it carries significant philosophical implications. The issue relates to the question of public versus private projections. Once again, the fact that the issue arises in the first place is telling evidence that projections cannot be regarded solely as scientific or academic exercises, since one of the hallmarks of science is the free dissemination of findings. But a projection is always in some sense a model of a possible or probable future. Its publication, especially by a government agency, inevitably carries with it the authority of an official pronouncement, with at least some connotation of administrative approval. It is therefore tempting to differentiate between projections designed for internal use only and public forecasts to be circulated at large. One advantage of such a differentiation is that it permits the technician the free exercise of his talents in working out the implications of a wide range of alternative assumptions without the onus of having to explain or defend the government's interest in these alternatives. Under such a policy, the only published reports would be forecasts, rather than projections. They would reflect some kind of plausible compromise between the goals a given administration might hope to attain and the most likely outcome in the absence of governmentally-inspired efforts. As such, they would resemble the judgmental forecasts of the National Planning Association, but without their alternative target and present policy projections.

Our objection to such a policy rests first upon our basic argument that projections should provide a public awareness of possible choices or alternatives and their probable consequences. A single forecast, whatever its ultimate accuracy, tends to create an atmosphere of false determinism which discourages critical reflection. One should note, in passing, that this effect is as likely to be found, and as dangerous in its implications, within the government itself as well as among the general public.

A second objection relates to the needs of the policymaker himself. A single forecast is of course required by him for use in the formulation of plans and programs. However, the policymaker must also estimate the effort required to attain particular goals, and he must consider alternative paths toward these goals. For instance, he needs to know not only that the national economy should be prepared to offer, say, 80 million jobs to its workers by 1970, but, in addition, whether that goal can be reached by means of a continuation, relaxation, or intensification of current growth rates. In addition, the methodological requirements of the newer Planning-Programming-Budgeting systems with their cost-benefit accounting schemes demand a systematic comparison of program targets and their achievement costs with probable outcomes and related costs in the absence of such programs. This requirement can only be met by the development of alternative projections and a publication policy which gives these alternatives equal publicity.[54]

[53] This position in regard to the role of values in behavioral science is a familiar one. For a brief exposition, see Richard Rudner (*156*). In economics, the need to supplement "value-free" techniques with value-inspired definitions of goals is well set forth by Adolph Lowe (*114*). Also see Lowe's more recent work (*115*), pp. 160 and 313. For an excellent critical discussion of Lowe's thought, see Robert L. Heilbroner (87). Philipp Frank's insightful discussion of the "extra-scientific" purposes which can be served by scientific theories is highly pertinent to this issue. See Philipp G. Frank (*56*).

[54] The need for alternative projections in carrying out cost-benefit analyses is brought out in three articles by Abraham S. Levine (*109*, *110*, *111*). The dangers of an exclusive reliance upon cost-benefit analyses in determining the feasability of proposed policies in the manpower field are well stated by Robert Lekachman (*106*). For a balanced statement of the uses of cost-benefit accounting in evaluating manpower policies and programs, see Richard A. Lester, *op. cit.*, chapter 7.

Ultimately, the argument favoring the free publication of all projections deemed worthy of preparation in the first place rests on the notion that an informed public is, in the long run, our best safeguard against the pursuit of unwise objectives or the adoption of inappropriate means toward desired ends. Particularly in regard to the future, the public needs to recognize that the growth of science and technology does not imply an increase in determinism but rather an increase in the range and scope of the alternatives before us. If projections are freely developed and published in all their diversity, they will perhaps serve their most vital function in educating the public to a conscious awareness of its own inescapable freedom—the freedom to "invent the future."[55]

[55] This tempting phrase is the title of a provocative essay by Dennis Gabor (*57*). Using Jouvenel's apt terminology, Daniel Bell states the issue very sharply as a choice between decision-making by seraglio or forum. See Daniel Bell (*11*). Compare the remarks of Alfred Sauvy (*op. cit.*, 1962). For a challenging discussion of the potential dangers in the achievement of reliable prediction in economics, see Gregor Sebba (*163*). In his modest and temperate commentary on Sebba's article, Carl F. Christ suggests that the danger is not on the immediate horizon, a least. For a profoundly pessimistic analysis of the universal trend toward greater rationalization and control over the future, see Roderick Seidenberg (*164*). Seidenberg, like Herbert Spencer a century earlier, foretells a world which has achieved total predictability at the cost of surrendering the last vestiges of meaningful individual choice.

III. THE EVALUATION OF PROJECTIONS

Both the improvement of projections and the determination of their status as forecasts or policy instruments depend upon the criteria whereby they are evaluated. Such evaluation cannot be confined to the simple question of their accuracy because, in the first place, projections often serve other functions than that of prediction, and secondly, the evaluation of a projection cannot always await the actual occurrence of the events it seeks to foretell. The following discussion distinguishes four areas on which evaluation of projections may be focussed: Their assumptions, their methodology, their outcome or content, and their usefulness, given their intended purposes.

A. *The Evaluation of Assumptions*

As previously noted, the most glaring deficiency of most projections is the insufficient specificity of their stated assumptions. This suggests the primary criterion to be employed in evaluating the assumptions: Are they sufficiently explicit to permit an adequate understanding of their implications? This is of course a matter of degree. Insofar as any projection reflects human judgment, it is bound to incorporate a complex interrelation of factors which cannot be fully expressed or weighted. Even in the case of the more obvious judgments, such as the selection or rejection of a particular variable for a particular equation, a detailed exposition of the reasons for the decision might well exceed the capacity of both the technician's memory and the reader's powers of absorption.

Furthermore, the practical value of a full explanation of the assumptions underlying a given projection can be questioned. In theory, such full disclosure would permit a faithful replication of a projection. What then? Does the ability to duplicate a projection affect its worth? Replication is of course useful in disclosing mechanical errors, but it does not affect the plausibility of the underlying assumptions. Unfortunately, it is precisely the more subtle features of an expert's judgment which are most difficult to express or duplicate, so that the demand for totally explicit assumptions amounts to a demand that experts should utilize only their most pedestrian, readily quantifiable talents in making projections.

The assumptions which underly a projection can also be evaluated in terms of their internal consistency. For example, a labor force projection which postulates a continuation of military expenditures as a constant proportion of total output would be inconsistent with the assumption of a drastic decline in the size of the Armed Forces, unless accompanied by the further stipulation of increasing automation in military technology. Obviously, this evaluation for consistency requires a certain specificity in the statement of the assumptions. "Global" statements relating to the assumed absence of worldwide conflict or depression are of little value, since they permit of a wide range of plausible interpretation.

A third criterion for the evaluation of assumptions is that of plausibility. This criterion must be applied with caution, because with projections in particular, what is deemed plausible tends to be greatly influenced by what is regarded as desirable. Furthermore, the expressed purpose of a projection may be to spell out the implications or consequences of a highly improbable set of circumstances. Such an exercise may be valuable heuristically, and should not be criticized because the stated assumptions are unlikely to occur in fact. Nevertheless, given the limited resources generally available for this work, the bulk of projections must be designed to provide a reasonable picture of potential realities, so that the plausibility of their assumptions remains an important consideration in their evaluation. Since we are concerned here with making judgments about other judgments, it is difficult to establish definite limits beyond which a projection may be rejected as implausible. However, the attempt is worthwhile, if only to force the technician to defend the adoption of assumptions which appear to fall outside the range of plausibility.[56]

B. *The Evaluation of Methodology*

The evaluation of a projection's methodoloby does not differ from that of any scientific procedure. Such evaluation should consider, first, the technical accuracy of the results obtained; second, the appropriateness of the analytical methods employed; third, the adequacy of the data and their treatment; and finally, the validity of the results and of their interpretation.

Considerations of technical accuracy comprise both the mechanical or arithmetical accuracy of the projection itself and the correctness of the definitions and categories of data that were utilized. While these matters are obviously the responsibility of the technicians themselves, independent review by potential users may often disclose discrepancies or errors which might otherwise escape detection. A frequent problem, particularly where time series are being used extensively, is that of definitional changes or changes in coverage which impair the comparability of major categories of data through time. A related problem is the need to fully explain the adjustments to the data which may have been deemed necessary Such adjustments are frequently made in haste and inadequately recorded, so that analysts working with long historical series of data are commonly unable to determine the nature and magnitude of the adjustments that may have been made at the time the data were being originally processed.

With respect to the adequacy of the methods used, it is necessary that they be considered not only in comparison with possible alternative methods, but also in terms of the time and resources available to the technician. The ease with which the inclusion of additional variables or the use of alternative methods may be suggested is not matched by the ease with which these elaborations can be carried out. Where government agencies have presented a single forecast rather than an elaborate set of projections, they have commonly done so because of limited resources, and not because of a deliberate policy of restricting publication to a single series. Given these inevitable constraints, however, the technician always faces a choice between better or worse methods, and a review of his methodology is therefore appropriate in any case.

Two further considerations must be stressed in the evaluation of the methods employed in projections:. The appropriateness of the methods given the quality of the available data, and their suitability in terms of the purposes the projections are intended to serve. It is not necessarily incorrect to employ elaborate methods of analysis with limited data or data of poor quality. In fact, such data may require the kind of careful tailoring which such methods can provide. However, a common effect of elaborate methodology is to convey a sense of greater precision than exists in fact, and to conceal the deficiencies of the data beneath an impressive scaffolding of quantitative analysis. There are no simple safeguards against the misuse of statistical methods other than the slow improvement in the methodological sophistication of technicians and users alike. One helpful device is the routine application of simple measures relating to the accuracy of the data (where such measures can be devised). It is now standard practice to show such measures alongside the pertinent statistics in all statistical publications. The publication of equivalent measures in connection with projected values would alert both the technician and the user to the limitations of the data and might warn them against drawing unwarranted inferences from the given results.[57]

C. *The Evaluation of Content*

The actual results of a projection—the levels and distributions which are shown for future dates—must themselves be evaluated directly, apart from considerations of assumptions and methodology. This evaluation may be carried out in three directions. First, there is the question of the consistency of the outcome with the stated assumptions; this is a separate issue from that relating to the plausibility of the assumptions as such. If, for example, a labor force projection assumes a continuation of early marriage and childbearing practices, a major rise in the projected labor force participation rates of young adult women should at least be questioned. A second direction of inquiry concerns the ability of the projection to satisfy the purposes for which it was developed. If its main purpose is to provide a public prediction or forecast, the publica-

[56] See, for example, Thomas Dernberg and associates (*op. cit.*, 1966) and the critical review by Sophia Cooper and Denis F. Johnston in the same issue, pp. 69-78.

[57] As noted previously, this is a primary recommendation of Oskar Morgenstern, *On the Accuracy of Economic Observations*, p. 302ff.

tion of a range of values reflecting alternative sets of assumptions will not suffice, unless one of the series is clearly identified as the most likely or judgmental selection. On the other hand, if a projection is designed to provide estimates of the probable impact of alternative policies, it should indicate the different effects of these policies by means of separate projections reflecting each of the alternatives.

A more difficult problem in evaluation of content is to determine the amount of detail and accuracy that is both sufficient and necessary. The needs of the user may often be met by a single number—for example, he may require a projection of the population of the country in the year 2000. However, such a figure can best be obtained by working with individual age-sex cohorts, and aggregating the results as a final step. Thus, the amount of detail required in developing a projection is more a function of the methodology employed than the purpose of the projection itself. The real question is therefore whether the results of a given projection reflect an optimum compromise between what the available data can offer and the detail required to serve the purpose of the projection.[58]

A third direction of evaluation of content relates to the recognition given to the band of uncertainty or error which surrounds any projection. Above all, projections should avoid giving the impression of greater precision or foreknowledge than is warranted. This is only partly a question of semantics. One can argue that a projection is not a prediction, and that a reading of the text will invariably disclose a convenient escape clause in the form of the usual "ceteris paribus" proviso, and that, in any event, "let the user beware." However, in the practical world, forecasts are used as given, and the attendant qualifications tend to drop out of consideration. This danger can only be minimized by giving equal prominence to the estimated band of uncertainty surrounding a given projection as to the results themselves.[59]

D. *Evaluating the Usefulness of Projections*

Because projections are typically developed for practical purposes, their evaluation must include some consideration of their usefulness in meeting these purposes.[60] Since many projections serve a variety of uses, such evaluation requires some notion of priorities. When projections are designed primarily to serve the needs of the policymaker, their evaluation must recognize the constraints imposed upon the technician by the pressing demands of policy formulation. The basic function of the policymaker is to map a course of action into an uncertain future. He is not merely called upon to make prognostications from time to time; he is immersed in the future all the time, and must constantly formulate and express his outlook on the future in relation to specific objectives and policies. He must therefore be provided with a variety of statements having a future reference, but related to current trends and policies. These statements must often be prepared on very short notice, and may commonly be altered quickly, with little chance for more than the most superficial research. Hence the evaluation of such statements or forecasts cannot be carried out in terms of the thoroughness of the methodology and underlying research, but must consider instead their timeliness and over all consistency.[61]

The simplicity of a projection technique may itself be an important asset to the policymaker, since he is often required to convey some notion of the assumptions underlying his forecast briefly and in nontechnical language. Projections designed to meet such purposes should not be evaluated in terms of criteria appropriate to the analysis of the more recondite research efforts in the field.

In contrast, projections designed to provide the policymaker with detailed guidelines, measures of progress toward a specific goal, or elaborate delineations of the possible consequences of alternative courses of action must be evaluated by considering their assumptions, methodology, and overall consistency in the fullest detail. Such projections should open up options; their assumptions must be sufficiently explicit

[58] Considerations of accuracy give rise to the question of the relative cost of an overestimate versus an underestimate. For purposes of manpower planning, an underestimate may prove more costly. On this point, see the remarks of Sidney Sonenblum (*179*).

[59] Morgenstern (*op. cit.*), p. 64, cites a classic example of misplaced accuracy: that of the man who estimated the age of a river as 3 million and 21 years, because 21 years earlier, he had seen its age given as 3 million years. Unfortunately, routine arithmetical computations require the use of specific estimates, despite the band of error which may surround them. The real danger is always that arithmetic precision will be confused with empirical accuracy.

[60] As noted previously, the principle purposes of governmental projections are admirably summarized by James W. Knowles, *op. cit.* (99). Also see James W. Knowles (*100*), p. 4f. For an evaluation of a group of recent short term economic forecasts, see Victor Zarnowitz (*207*). A more detailed exposition of the methods of evaluation of economic forecasts in general is forthcoming. See Jacob Mincer and Victor Zarnowitz (*131*).

[61] The preparation of a quick forecast does not necessarily imply the use of simple or intuitive methods. If a forecasting model is truly operational, it can, in principle, be used to generate new projections under a wide range of specified assumptions on very short order. Of course, many of the quick forecasts that are demanded are too unique and poorly specified to be readily fitted into any complex model.

to permit a clear distinction among the alternative series. Their evaluation should therefore focus on the explicitness of the assumptions, the clarity with which their likely consequences are described, and, most important, their variety and imaginative composition.

Projections which best fulfill the above purposes must actually serve in two capacities, and should therefore be evaluated accordingly. Their overt function has already been emphasized—to alert the policymaker to the scope and range of the alternatives before him and offer added insight into the attainability of desired goals, or the desirability of attainable ones. Their hidden function is, however, just as important—to provide an awareness of the limitations to action which exists in a given situation, the constraints imposed by limited resources, political exigencies, the weight of habit and custom, the occurrence of unpredictable events, and the like.[62]

As was discussed in the second chapter, projections serve a number of purposes additional to those mentioned above. Following the sequence of purposes summarized there, appropriate criteria for their evaluation can be suggested in each case. First, the ability to provide advance warning of impending strains or imbalances given a continuation of current trends affords a stringent criterion for evaluation. The evaluation of a projection on the basis of its success in this respect necessitates an ex post facto analysis, since it presupposes knowledge of what did or did not occur over the projection period. Even with such knowledge, this kind of evaluation is difficult. It is always possible to detect, with the advantage of hindsight, signs of impending events which were not at all obvious prior to their occurrence. Furthermore, forecasts which turn out, by the merest chance, to be remarkably accurate are likely to convey to both the forecaster and his methods an undeserved reputation of forecasting ability. The reputations of many traditional soothsayers and oracles were largely built up in this manner. Since their prognostications were commonly expressed in sufficiently ambiguous terms as to be consistent with a wide variety of outcomes, their forecasting ability is not surprising. Today's quantitative forecasts do not usually allow such generous bounds for forecasting error.

In evaluating projections on the basis of their ability to provide advance warnings of coming change, it is necessary to recall their "self-denying" possibilities. A projection of an impending problem which sets in motion a course of action which overcomes the problem will have served a vital function although it turns out to be highly inaccurate.

Evaluating the ability of projections to elucidate the probable consequences of alternative policies or programs requires some decision as to the appropriate limits of the technician's professional competence. For example, current labor force projections show substantial increases in the labor force participation rates of young adult women. Should such projections be accompanied by a discussion of the possible unintended consequence of such a trend, such as a possible rise in juvenile delinquency or other strains on the structure of the family?[63] The fact that such speculations cut across conventional disciplinary boundaries does not affect their potential usefulness to the policymaker. In the longrun, the noneconomic consequences of particular manpower policies may turn out to be even more significant than their economic impact.

A far more stringent criterion for the evaluation of projections is the consideration of their success in assigning probabilities to the possible outcomes of specified assumptions or alternative policies. Only strictly quantitative projections offer even the possibility of meeting this requirement with any precision. However, it should be possible for technicians who develop alternative series of projections to accompany them with some kind of rough probability ranking, arrived at judgmentally, on a scale from highly probable to highly improbable. The use of such a scale would also be helpful in selecting a single most likely series for forecasting purposes.[64]

Where projections are designed simply to extrapolate trends from a specified period in the past, their evaluation is more straightforward. Here, the relevant criteria are the adequacy of the statement of assumptions, the appropriateness of the technique selected to represent the central tendency of the observed data, and the accuracy of the derived extrapolation itself. Similar criteria are appropriate in evaluating projections developed to prescribe alternative paths from a fixed base line to a predetermined objective or target level. The adequacy of such projections rests ultimately on their consistency with the specified assumptions.

[62] The thoughtful remarks of Simon Kuznets provide a clear sense of the difficulties of evaluating projections in the context of changing social and political realities. See Simon Kuznets (*101*). The same problem, seen from the viewpoint of the business forecaster, is usefully discussed in Edward J. Chambers (25), ch. 12.

[63] This is intended to be purely illustrative, and presupposes the existence of evidence showing a positive association between the percentage of working mothers and the incidence of juvenile delinquency. We are begging two questions here—does this association exist? and, if so, does it imply what we are assuming it to imply?

[64] An interesting example of this kind of scaling is to be found in T. J. Gordon and Olaf Helmer (*op. cit.*)

The increasing use of projections as elements in a larger model or in a chain of related projections suggests further criteria for their evaluation. An obvious consideration is the mutual consistency of the assumptions which underly each of the projections which are linked together. In preparing any projection, some attention should be paid to the more obvious usages it will be given in association with related projections. For example, a population projection is more useful as a base for labor force projections if it provides greater age detail for persons age 16 to 24 and 55 and over, since the worker rates of persons in these ages change rapidly. By the same token, the usefulness of population projections would be greatly enhanced if separate projections could be shown of the civilian, noninstitutional population, for use with projected civilian labor force. Similarly, projections of total and civilian labor force should be made available to meet different user needs. Although the tailoring of a projection to meet special requirements of this sort may add considerably to its cost, the ad hoc adjustments which must otherwise be made by the user himself are likely to be both costlier and less accurate.

Most difficult to evaluate are the projections which offer the greatest potential challenge—those deliberate inventions or scenarios of the future which seek to explore the consequences of a set of hypothetical circumstances. Perhaps the greatest danger in this kind of imaginative exercise is the non sequitur. However, we are dealing here with possibilities, not with necessities, so that a weaker criterion of evaluation than strict implication is called for. Perhaps we are again left with plausibility as the only criterion which affords a basis for criticism while recognizing that any conceivable future is possible in a nondeterministic universe. The problem with plausibility is that it, too, demands an operationally meaningful definition; otherwise, it reflects little more than one's cultural background, values, and predilections. Since, to the ordinary critic, no future is plausible unless it reflects an idealization of the present, the criticism of heuristic projections of the grounds of their plausibility would introduce a powerful conservative bias into their evaluation and thus greatly weaken their potential contribution. A better basis for evaluation may be found in considering the thoughtfulness and ingenuity of their development and the overall adequacy of the structure of their hypothetical determinants and consequences.[illegible]

The evaluation of projections in terms of their ability to fulfill a public information function brings into consideration their potential audience. As previously noted, the requirements of technically trained users differ markedly from those of either the policymakers or the general public. Both multiple projections and single forecasts are apt to mislead the general public; the former because of their indefinite quality and the latter because of their apparent determinism. In addition, a forecast, when published, carries the authority of the publishing agency behind it and tends to be interpreted, in some sense, as agency policy. For these reasons, it is necessary to evaluate both projections and forecasts in terms of the balance they maintain between the theoretically possible and the historically probable. While both projections and singular forecasts are rooted in historical trends, the former can only be given an appropriate range by emphasizing theoretical possibilities representing departures from past trends. The forecast, on the other hand, must reflect a selection from among these possibilities with emphasis upon the historical trend. Unlike projections, forecasts should not be expected to depart radically from past trends unless the departure can be supported by strong evidence. The burden of proof in a forecast (again, unlike a projection) should rest on the assumption of a major change in underlying structure rather than on a continuation of past trends.

The primary needs of the general public in regard to projections and forecasts are for clarity of exposition, careful labeling of results, and an understanding of the vital educational role to be played by any pronouncements about the future. In evaluating projections as public documents therefore, primary consideration should be given to their expression in nontechnical language, with only a necessary minimum of expository detail regarding methodology and assumptions and greater emphasis on purposes and uses. This requirement cannot easily be met when projections are the product of technically complex econometric procedures, but even here, a nontechnical summary of their assumptions and procedures, followed by a careful statement of their intended uses is essential if the projections are to be used intelligently by those who lack the technical training of the econometrician.[illegible]

To the informed public, forecasts are commonly the object of amused scepticism, reflecting the commonsense view that a forecast is only good if it is accurate, and this quality can never be determined beforehand. Furthermore, forecasts are seldom the object of wide spread public interest at the time they are issued; only

[illegible] See the trenchant observations of Bertram Gross (72). For a brief summary of his challenging views, see Bertram Gross (71).

[illegible] The recent remarks of Secretary Wirtz are pertinent in this regard. See Willard W. Wirtz (203).

when, with the passage of time, they turn out to be glaringly wide of the mark, or, more rarely, surprisingly close to reality, do they receive great publicity. In the latter instances, the aura of certitude which comes to surround both the successful technician and the successful technique may be more harmful than complete scepticism, since it promotes slavish imitation rather than innovation. For these reasons, the educational purpose to be served by the publication of projections rather than forecasts is of great importance. Only when informed individuals understand the differences among projections, forecasts, and predictions can they be expected to examine projections in the light of more relevant criteria than merely their predictive accuracy. When projections are recognized as accounts of possible futures or "futuribles" (to use de Jouvenel's term), they can properly be viewed as possible extensions of human values and goals (i.e., as target projections) or as extrapolations of trends which may threaten these values and goals. Only then can the expression of public reaction to given projections serve to inform the policymaker of the public will in regard to the essential task of guiding the future growth of those aspects of our society which are deemed appropriate objects of national planning.

The above considerations lead us to conclude that projections cannot be adequately evaluated on technical grounds alone, and a fortiori, they should not be evaluated only by technical experts. Their ultimate significance can only be perceived within a broader frame of reference, comprising the society's goals and values, and, to use Shackle's term, the current "rig" of the society—i.e., the set of tensions or pressures arising from gaps which exist at a given time between the actual situation and our values and aspirations.[68] The construction of such a frame of reference is coming to be recognized as a crucial task for the behavioral scientists during the remaining third of the present century. Such a frame of reference would require the development of a system of social statistics supplementary to the economic statistics on which so many current estimates of the society's condition must rely. Given such a system, selected measures could be used, separately or in combination, as social indicators—i.e., readings of the current status and evident trends of the society in regard to specified goals or program objectives.[69]

The primary significance of this development for projections of labor force is that it affords both a challenge and an opportunity to bring the problems of labor force growth and manpower development out of the constricting framework of a conventional supply-demand calculus into a broader perspective of social goals and aspirations. Only within such a perspective can the potential costs and benefits of alternative manpower policies be estimated in social and psychological, as well as economic terms.[70]

[67] The dangers of self-serving governmental secrecy in this vital area are well summarized by Alfred Sauvy (*op. cit.*, 1962, p. 125f). As one of France's leading demographers, Sauvy has had extensive experience in the application of demographic and economic techniques in the development of economic policies. For a brief exposition of his thinking on demographic-economic projections, see Alfred Sauvy (*op. cit.*, 1959).

[68] George L. S. Shackle (*166*), pp. 112-118. This essay is a perceptive commentary on three lectures by Bertrand de Jouvenel on "models of conjecture."

[69] A general outline of such a system is developed by Bertram Gross (*73*).

[70] A challenging account of the plurality of rationalities which must be considered in the development of social policies is provided by Paul Diesing (*44*). The need to incorporate noneconomic factors in the solution of economic problems is also well stated by Allan G. Gruchy (*75*). Also see Melvin L. Greenhut (*69*) and Alfred L. Thimm (*196*). A clear statement of the transeconomic nature of manpower problems is given in Henry David (*38*). Also see the discussion of this issue by Harold L. Sheppard (*167*).

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Mr. IRWIN. The smart bird at the Bureau of Census pushing all those buttons is smart enough not to kill the dumb bird; keep making projections we want to use. I don't know if that dumb bird is quite that dumb. He is liable to get the idea he can make the projection and do without the Bureau of Census. I am personally sympathetic with the idea of a forecast figure, a preference choice. I think the tenor of my remarks indicated we would if such a statement is made. This is an old argument between monitors making projections and the people who want to use them, but the basic assumptions put into the population projections are these ones of completed family size: 3.4 for the high (A) and 3.1 for (B), 2.7 for (C), 2.4 for (4). These are strictly judgment calls although I would add a parenthetical comment: We represent B and C as more reasonable and probable and A and D as extremes, which is a minor point. I should have elaborated on the expectations and stated we are basing the choice on this national survey which has been carried out three times: in 1955, 1960, 1965; asking women how many children they expect to have in 5 years. This survey goes no further than 5 years and the data has remained constant at three. Knowledgeable people in the field working on the expectations data pointed out we were talking only to young women of childbearing age, we don't interview anyone 10 to 14 years of age. Within 5 years these girls produce a number of babies. If we get a range of probability around one figure, I'm sure we would do it. I feel we ought to try.

When Denis was showing the problems which beset him with the female participation rate 25 to 29, I would take the official bureau position, the one he has presented. He presented a series of assumptions. We say they are bald assumptions: If they come true the population will be thus and so, but since our assumption becomes his starting point, with the mental torment we put him through regarding the number of babies to be produced, which series to select, I wonder if this is a useful exercise. Our position is, "Look, folks, we don't know exactly what is going to happen; these are your alternatives. You have certain situations; use this the way you want." This is what we did and why, which is quite involved. I feel sympathetic because before I came to the Bureau I was irritated to find they were giving me four, not three. You can't even pick the middle one and I had to read this long detailed explanation. It is tough and I am not really presenting a position so much as asking a question.

Dr. JOHNSTON. I ended by saying whatever else projections do they educate us and I don't say it as professional training.

Dr. SLAFKOSKY. What do you suggest as the proper alternative criteria for projection. You are saying you can't use the fact they were either correct or wrong in some sense. The proper criterion should be internal as I understood it. You stated they were educational. Kindly elaborate as to the kind of education you are referring to.

Dr. JOHNSTON. I don't know. I get to the outer edge of my own ability to comprehend the area in which I earn my living. We are groping for a sense of social goals, social indicators or progress toward social goals. There was a book published last year, "Social Indicators." As my old professor at Harvard used to say, "The only thing that gives this book unity is the cover," but in the book is a very good collection of work relating to the problem of the social sciences—how do we get even measures to our goal, much less the goals we all agree on. We want healthy and peaceable people all over the world. We would like people to be educated, so they can fulfill their creative potentials. All these goals I state, may sound clear enough but they are not transparently clear. You can have something that sounds clear when you read it but it's ambiguous in its implications and its connotations. All of our social goals are ambiguous in a sense. What do we mean by health, education, peacefulness, and social order. We want growth. This contradicts the notion of order. Maybe you can't get growth without disorder. We need to know when we have a goal to agree on and how we measure our progress toward it. "Social Indicators" by Al Biderman has a chapter relating to the Uniform Crime Reports of the FBI, and tries to show how misleading the statistics are. You get the idea they do more harm than good in informing the public about the phenomena from this article. I'll try one measure of the effectiveness of a projection to my way of thinking: Has it given more fruitful directions to search for measures of attainment, progress or advance? Does it give us insights? Does it open possibilities of seeing relationships we would otherwise overlook? Does it open possible alternatives we would not have considered? The idea of giving every potential Viet Cong a Ph. D. is a typical far-out stupid irrelevant remark, but 20 years ago this might not have been unfeasible. When political relationships were sufficient we could take some of the leaders of these underdeveloped countries and educate them. It might have been feasible in China 20 years ago when they were still friends. These are far-out alternatives; projections may serve to open possibilities. I'm sure concerned about the ones that end with a prediction. One thing about a prediction: You can be sure it will be wrong.

Dr. SLAFKOSKY. Let's talk about your projections. If you are interested in the insight you theoretically get, you internally are doing projections and you may get some. It seems the value of these insights are only as good as your projections. It is only afterwards they receive any value. If you agree then one of the values in this procedure is looking at how good your projections and forecasts were. Only by this reflectivity can you profit internally and, as matter of fact, see the implications of various variables or interrelationships that might exist in the area you are discussing.

Dr. JOHNSTON. You prompt a reply that makes me sound a little schizophrenic. I agreed with Dick Irwin when he said if you had a choice between a wild projection and an accurate one, he would work on the accurate one. This schizophrenic element is here. The future is something we are inventing. It is a function of the past but also a function of decisions we need to make and the question is what do we want to be and what do we want to do? There is a book by Dennis Gabor and I quote the title: "Inventing the Future," which can sell the book. I read it and I'm not as satisfied with the inside as I am with the title. We are involved in the business of invention and not just technological invention. Our best inventions in the next 50 years are going to be in the social area. We have seen that today to know this is true.

Major MARTINO. How does an internal criterion of information measure the usefulness of a forecast, other than how accurate it was as viewed years afterwards? Another problem—you choose what you consider to be the most likely set of assumptions and then make a single projection which becomes a forecast. I have had experience with problems where one set of assumptions had a likelihood of 10 percent. This was the most likely set of assumptions. But there were others quite widely spaced that had a likelihood of 9 percent. So I had one most likely set of assumptions but it wasn't much more likely than something radically different. The point is, I'm changing roles for a moment and speaking not as a forecaster but as a consumer of forecasts, the decisionmaker, the advisor to the decisionmaker who has to use the forecast. The statement, this is the single most likely set of assumptions, is not nearly as useful as telling me: Here are three, or whatever the number, about equally likely things that could happen. Al, how do I judge the utility of the forecast? I judge it in terms of how well it helps me plan for the disasters that might happen, and do this within my budget. It may be that the disasters never happen. If I have optimally deployed my resources, I have not wasted them on an unlikely disaster. But I have deployed them in defense against reasonably likely disaster; then if this is not the case and they don't happen, the projection and the forecast was inaccurate but, nevertheless, it was useful to me.

Dr. JOHNSTON. I think you made the point better than I could. Sometimes you make a projection you hope will be self-denying and sometimes you make one you hope will be self-fulfilling. The government in its infinite wisdom is always doing this. When a projection about a labor force is made we are, in effect, trying to announce: "Look, folks, we are going to have a hundred and five million people that are going to be working or looking for work. With more than 5 percent of them looking for work, we will have troubles. We've got to get a hundred million jobs." We hope it is self-fulfilling in the sense that we don't know of any viable alternative to being able to make your way in the world-of-work. Someday everybody may be educated to leisure but that would be an alternative. This self-denying prophecy is also useful—the one you look at 10 years later and say, "Gee, that was all wet; we didn't have an atomic war." It was useful at the time because it led to policies and programs which made war less likely and caused it not to happen. Certainly a great deal of your work is in the area—I hope all of your work forever will be self-denying in that sense, if you prevent what everybody hopes won't happen.

Mr. CETRON. I was glad to hear you mention the war and the ones who will understand the work in the future. Daddario, in our Congress now, in a briefing a few weeks ago said he was pushing to have a group set up reporting directly to Congress, indicating what the possible future world will look like. What is technologically feasible, socially acceptable and the second group, not this same in Congress saying, "Now, not 1984 we will do this and this, but how do we pull together and coordinate various people going in different directions to make sure the better goals for this country will be achieved." Part of the rationality brought out was Daddario said, "You are talking about 1984; you brought it back over here; you are going to direct us; we're going to have a ruling group—not an aristocracy—telling us what the future will be," and I think he handled it well by saying, "No, I'm not. I'm trying to say what would make the world a better place to live in, especially the United States, what we can do by coordinating the technology, the social implications, to make sure this will be available and, how is this going to put us in a better position to take care of the country as a whole." I believe this is the point we are trying to bring out. Your output, the labor force, the input on the census, these things are inputs to them because they've get to use this as part of their rationale. The better your figures are, or the poorer, take your choice, the better their projections will be with the social implications, which, is where you start your projections of technological forecasts. There is a major turning-around on the outside, a ferris wheel in a sense, and in the center is your future environment. Things are happening and we are trying to clear the focus on this crystal ball. We are on target but every year we adjust and I think that's what you are trying to do.

Dr. JOHNSTON. It should be a continuing process.

Mr. FRIEDMAN. Those of you who do not live in or around Washington, D.C., may not be aware the local tabloids devote considerable space to the Federal employees of the nation. You also may not be aware of a recent article which suggested we extend the work day one-half hour by extending the lunch period from a half hour to an hour, and the Federal Government would save a billion dollars. What does this suggest to your projected labor force figure? How does this affect your thinking?

Dr. JOHNSTON. One of the biggest weaknesses in our labor force projections is we treat each person as a unit and a person could be working for pay or profit 1 hour a week or more and he is in the labor force. The biggest unknown, and this may be an advantage, it gives some leeway in planning for jobs because if you are getting people who would be working part time and, some won't work under any other conditions, you may talk about a hundred and five million people in the labor force, but in terms of hours of paid employment, that would have to be provided—you might have a plus or minus factor of 10 or 15 percent depending on the willingness or readiness of the people to work shorter or longer hours. We do need to refine our projections by introducing still another variable, which has to be projected, namely, an average of hours worked. We think of two thousand hours as a convenient work year pattern: 50 weeks, 40 hours a week, give or take a week or so. But it is not 2,000 hours because more people in the labor force are coming down to 1,800 and for others it is half of that or a third. We need to have that variable and we don't.

Mr. FRIEDMAN. If the purpose was to extend the work day a half hour, eliminating the need to hire ten thousand additional people, thereby saving a billion dollars. That was the implication. If you do not hire ten thousand people what does this mean to your unemployment force?

Dr. JOHNSTON. Much of our work is at cross purposes because what you are saying is: You are going to reduce the demand and demand being reduced in Government is still a small component of the total economy. Labor is a very expensive component. You have everybody reducing demand then what are we going to do? One possibility is this leisure world that David Reisman has been talking about. I daresay it is going to take more education to handle leisure than it ever has taken to handle work—education at knowing ourselves and what we are like, becoming acquainted with ourselves. Most of us can't find out about ourselves because we are too busy earning a living. Maybe that's good. It looks like WPA, disguised under different initials. The Government has been suggested as an employer of last resort, to which the cynics comment: It already is.

Dr. CHACKO. I think you have information which would take you out of that. I would like to point out two things. You have been avoiding your responsibility—not you personally—by saying the projections will be checked against numbers. We could care less about that and should be more responsible to our obligations to the social affairs. If you were to say what errors, in one projection, are going to have the worst effect. For instance, if you were 10 percent off in projecting the labor force in the age group 50 to 60, this would be less of a calamity than the same kind of error in predicting the labor force of people between 16 and 24, the reason being the older age group have the wherewithal to find a place better than kids just entering the labor force and the damage that would be done by stretching the rubber band is far more acute in the lower age level. I suggest if we consciously said, "This is one position, the error will be more damaging than something else." This is what you are doing by default, simply saying everyone is equal, but this will be introducing a design having a format. There is one which could work for projection purposes, which is the completed families. I know you have had this analysis, but I don't know what you are doing now. If a round figure of three children is taken as the standard, it is clear the family has two children and is likely to have one more rather than two. This again you can differentiate. If the woman had two children before the age of 25, the probability of having an additional child is higher than if the woman is age 30 when she has two children. We can narrow the area with this information and check the actual data of family completion as we go along. By watching this variable we can improve our projections. You spoke of the econometric projections and later you talked about the insensitivity of the labor force to economic factors and birth rates. The only way to reconcile the two statements is to have an equation of the form:

$$a_1x_1^{b_1}+a_2x_2^{b_2}+\dots+a_nx_n^{b_n}$$

Dr. JOHNSTON. Yes, I did it in a paper for a course in econometrics. The result was a little beyond my capacity to analyze but the professor, Martin Abel, a wonderful teacher, was rather disappointed. Everything was directly contrary to what we would be able to explain on the basis of what we understand about human motivations. Maybe we are wrong. We are approaching the results of an equation with notions as to how people respond. Everything increases through time, we are dealing with many serial correlations. You get beautiful correlations and you may get pretty tight coefficients without too much variance. A standard deviation term might be accepted but you are left with an independent variable, which you have to make assumptions about. I don't want to disagree with your criticism, that you don't mean personally, but I take them personally because we are an underdeveloped world. This is a world of soft science technology and I am in a position whereby I can act on what I learn. There are only four of us working on this and we can do something about is this discriminate analysis, but focus on the groups that are important and you are exactly right. From 16 to 24 years is a problem. Beyond this age people have settled into a routine or have the wherewithal to cope. The young entrants are the crucial point. I am making an educational projection, tied to the Census Bureau's projections, for the population as a whole. Education is one variable that is subject to policy manipulation to a certain extent which we can introduce at a faster or lower rate. I don't know about lowering it, but we can increase the educational growth to an extent. You are right, our projection is deficient because we have over-quantified. A person in the labor force who is going to work 40 years, 40 hours a week, 52 weeks a year, is one; and a person drifting in and out at random, working a few hours at a time is also one, which is misleading.

MAJOR SWETT. A quick billiard with Dr. Slaf-sky's question and Major Martino's response. We are addressing the problem of validity of forecasting and what we have is this: The nature of all forecasts and projections is essentially deductive. The sophistication of technique can increase the validity of the product up to but not beyond that of its entering assumption. The weakest link in the chain of social sciences is the entering assumption. In trying to establish criteria to evaluate forecasts, we find assumptions have to be verified against data. Data varies by credibility with time and we have none for the future. This is one reason you can't check a forecast against the future. The other is the feedback effect. If anybody knows about your forecasts to that degree it is a steering signal that changes the future condition and has a self-defeating or self-fulfilling aspect. The future is no good to evaluate forecasts, even when you get there. As far as checking against present data is concerned, you don't have it and it is not instantly available. There is an optimum point in the past but you can't go back for data to verify your assumptions because data becomes exponentially uncertain as you go backwards. The farther you go back the less is known about specifics to check your theory against. There is an optimum point in the not distant past where data is most valid for the verification of assumptions or premises. In the area of international relations, which is my field, data is best at 5 to 8 years past. The criterion we are using to evaluate forecasts is the degree to which entering assumptions have been empirically verified. The interesting thing is that some basic assumptions in my discipline have been empirically denied, but continue to be used as input to scenario generators and forecasting systems. The way out would be to increase the efforts to verify or deny the assumptions on which these projections are based.

DR. JOHNSTON. We think of this as the ego problem. You have to reach in and out.

MR. HACKE. The reported unemployment rate underestimates actual unemployment and is self-limiting because—particularly to certain structural elements of population—no one can receive jobs after a certain point. Would you comment on this?

DR. JOHNSTON. Yes, the claim is valid. You can always suggest alternative measures but the measure we use on unemployment makes it impossible for this to be valid. You were talking in terms of the real world and we are talking in terms of how we make a measure. We measure unemployment by surveying a sample of households about one out of every 1,100 households a month. We cover around 52,000 households, the best sample of any survey in this country—and I want to compliment my census colleagues on the wonderful work they do in that area. They do a survey each month. We measure unemployment by responses to questions: What were you doing last week? Was he looking for work, or unemployed? We miss the people we undercounted initially, maybe 5 million people. Are those people more likely to be unemployed? We can't jump to that conclusion. It has been tried by one conference. Your question addresses itself more sensitively and our Secretary of Labor is putting it in these terms: How many of our people are underemployed? You can be underemployed if you are not using your education. Then you can easily slip from that to the psychologist's point: We are underemployed if we are not using ourselves to the fullest capacity. I, personally, don't like to be utilized to the fullest capacity because it makes me feel tired all the time. There is such a thing as overemployment and I am overemployed but we don't know how to measure it. Some people are discouraged. In the survey of 50,000, under age 65, we found a small number of these. Many people are discouraged because they are getting one day older every day. This is discouraging for all of us. We can attribute this discouragement to something else, but it is discouraging to get older and we don't know how to measure this very well.

Mr. Hacke describes a study he has recently completed, that required him to investigate several forecasting techniques which also involved trying to forecast "what might have been", if history had been different. His cautionary words about how difficult it is to find out what actually happened in the past, let alone make forecasts for the future, should be taken to heart by all forecasters.

—Editor

ANTICIPATING SOCIOECONOMIC CONSEQUENCES OF A MAJOR TECHNOLOGICAL INNOVATION

James E. Hacke, Jr., Stanford Research Institute

INTRODUCTION

This is a report on two related research projects. One was to produce a manual on technological forecasting methods for the long Range Planning Service (LRPS) of Stanford Research Institute. This project has produced two volumes, now in galley proofs, entitled "Forecasting Technological Change: Executive Overview and Forecasting Technological Change: Manual for Analysts."

The other project was for the National Science Foundation to assess the feasibility of anticipating economic and social consequences of a major technological innovation. A paper that I gave last year at this symposium embodied preliminary results of this study. Its title was "A Methodological Preface to Technological Forecasting", and it sought to prove the thesis that technological forecasting has the same epistemological warrants as such other applications of scientific knowledge as engineering and medicine.

The final report on this NSF project has just been released for publication.[1] In broad outline, the research plan was to assemble, analyze, evaluate, and organize existing methods for forecasting technological change; to assemble, analyze, evaluate, and organize existing methods for assessing socioeconomic impacts of past technological innovations; to combine the two sets of methods into methods for anticipating socioeconomic impacts of future technological innovations; and to try the methods out on a past innovation to see how they work.

This paper therefore falls naturally into four main parts, following this introduction: Technological forecasting, assessing socioeconomic impacts, testing the combined methods. and conclusions. First, methods of forecasting technological change.

[1]James E. Hacke, Jr., "The Feasibility of Anticipating Economic and Social Consequences of a Major Technological Innovation" (SRI, May 1967).

TECHNOLOGICAL FORECASTING

In his monumental volume,[2] Erich Jantsch described more than 100 techniques that he calls methods of technological forecasting. But, in the first place, he includes all human activity, from basic science to government, in the subject matter of his forecasting; second, he includes diffusion (among disciplines, industries, nations. and cultures), along with innovation, as something to forecast; and, third, he advocates what he calls normative forecasting, or goal formulation and planning, among the forecasting tasks.

Both for methodological and for practical reasons, the scope of the forecasting task here studied is much more limited. We differentiate technology from science, economics, sociology, and politics; study innovation and not diffusion and leave goal formulation and planning to others.

Technological change is not a process that goes like clockwork, with every tick highly predictable from the sequence of past ticks. Individual technological advances are almost completely unpredictable. So, if you will, technological change is like human birth. Each birth is the result of highly unpredictable activities; but the birth rate is not completely unpredictable.

We cannot forecast technological change with complete confidence and accuracy, any more than we can forecast the birth rate with complete confidence and accuracy. At most we can forecast that technological change will probably fall within a certain range. The real question is whether such "iffy" forecasts are useful.

Technological change does not go on in a vacuum. It

[2]"Technological Forecasting in Perspective" (OECD, Paris, 1967).

is largely the result of technological effort, and that effort is motivated largely by economic and social concerns. Technological innovations in turn have economic and social consequences: think of the Bomb, the Pill, and LSD. In my opinion, nevertheless, technological development is much more dependent on socioeconomic developments than vice versa. This is a basic contention underlying the whole study.

In the light of this contention, the rational approach to technological forecasting is to start with forecasts of economic and social conditions, goals, values, beliefs, and concerns. These forecasts can assist in judging whether specific technological efforts are likely to receive more, less, or the same support in the future as in the past. These estimates are the principal bases for estimating whether future technological trends are likely to follow historical patterns or to diverge up or down.

Having made a technological forecast, the analyst should investigate possible economic and social consequences of the forecast technological changes. If necessary, he should modify the socioeconomic forecasts that he started with, and repeat the process until the socioeconomic and the technological forecast are consistent.

The analysis, evaluation, and classification of forecasting methods in this study resulted in 10 generic classes of forecasting methods, five qualitative and five quantitative. The qualitative methods yield a picture of the probable future pattern of technological development. Quantitative methods yield the probable future change in parameters descriptive of the state of the art in a technological discipline. A parameter is meaningful only in the context of a known technological pattern; and, when a parameter reaches a critical value, a change in pattern may become possible or necessary. So you can start either with patterns or with parameters. Either way, you must continue investigating the probable effects of each on the other until you have a forecast that is qualitatively and quantitatively consistent.

QUALITATIVE METHODS

The five qualitative methods are those that forecast the probable pattern of future development. These methods can give insight into such aspects of the future as the layout and organization of the metropolitan area; the nature of future transportation and communication systems; and relations between men and machines in the future business organization.

The first of these five qualitative methods is delineating the boundaries of the possible. These boundaries are set by the definition of terms, budgetary identities, the laws of science, the present technological situation, and the nature of human utility. Budgetary identities apply not only to money but to manpower and material resources. Any future development of the U.S. railroad system must start with rails that are now 4 feet, 8½ inches apart. Telephony between the United States and India will be of limited utility as long as they sleep while we work and vice versa.

The second qualitative method is historical analogy. We use it whenever we use metaphorical labels like "iron horse" and "industrial revolution". But the only systematic attempt to apply historical analogy to technological forecasting of which I am aware is in a book edited by Bruce Mazlich.[3]

By comparison and contrast with another great social invention, the transcontinental railroads, Mazlich and his authors try to draw conclusions about the space effort and about the general nature of social invention and technological effort. One of their conclusions: Innovations do not move from one industry to another primarily through literature, but in the minds of trained employees.

The third kind of qualitative forecasting method is perhaps the most widely used: expert opinion. At first, it seems reasonable to go to an electronics expert and say, "Give me a forecast of the future of electronics"; but this procedure is not so wise as it seems. First, he is not necessarily an expert in forecasting. He will almost certainly invent his own forecasting methods, without being aware of what has been learned about it. In the second place, he is likely to be ignorant of developments in other fields that are going to have a profound effect on his own. In 1957, when I forecast the future of global telecommunication, I did not take space communication into account.

It is therefore usually better to rely on the technical expert only for the present conditions in his discipline —including the present rate of development and the nature and severity of presently visible barriers to future development. On the basis is these data, the forecaster can erect a projection of probable future developments. He will want to check this forecast with the expert, to assure that he has made no technical blunder; but the responsibility for it is his.

[3]"The Railroad and the Space Program: An Exploration in Historical Analogy" (MIT Press, 1965).

By contrast, the fourth qualitative forecasting method, using consensus, makes deliberate use of the intuitive forecasts of a variety of experts. The assumption here is that the experts can bring a lot of knowledge, remembered and unremembered, to bear on the forecasting task. If the group includes a wide variety of disciplines and economic and social viewpoints, they can help correct for one another's prejudices and blindnesses.

Many ways of arriving at a consensus are open to the objection that the experts can influence one another psychologically as well as logically. The Delphi technique developed by Olaf Helmer and others at Rand helps to obviate this limitation.

Consensus does not necessarily mean unanimity. The experts may agree to disagree. This spread of expert opinion can serve as an uncertainty measure for the forecast.

These first four qualitative forecasting methods are useful as far as we can reason into the future from the present and the past. This time span is adequate for almost all planning activities. But on some occasions it would be advantageous to have some idea of the future as far as 50 years hence. Young people in school today, for instance, may still be working in A.D. 2020. Educators would appreciate an inkling of what skills known today will still be useful then.

The last method of qualitative forecasting is useful for discerning what is discernible about such a remote future. It has been called inventing alternative futures, and involves writing as many different, plausible, internally consistent future histories as possible: of the world, of this Nation, and of relevant technology. From these alternatives emerges an envelope enclosing the long-range technological changes that may be expected to occur.

As a matter of experience, only about a score of really different alternative world futures have so far been invented. That there are so few may reflect the limits of human imagination rather than of the possible. But they surely embrace more alternatives than a less extensive and systematic effort would produce, and thus yield the best available basis for preliminary long-range planning. And there will be plenty of time to change plans to deal with the unexpected.

QUANTITATIVE METHODS

Before discussing quantitative forecasting methods in detail, consider for a minute how to select and collect data on quantities to forecast.

A quantitative measure of a technology should indicate something about how well we are likely to be doing something tomorrow, as compared with today. The chosen quantity should therefore actually represent the technology in question. Speed records, for instance, do not represent how fast our transportation systems actually move goods and people.

Unfortunately, most technological data were not gathered for forecasting, but for accounting and control. They are not always particularly appropriate as a base for forecasting.

Early in the development of a technology, there is considerable uncertainty about what data are suitable for any purpose, and there is little or no systematic effort at collecting data. The forecaster must then be satisfied with very approximate methods, until systematically collected and organized data become available.

Quantitative forecasting methods also fall into five generic categories.

When you have collected and organized your data, you can hardly help plotting them as a function of time and seeing what the trend looks like. Forecasting by extending past trends into the future is the first quantitative method: inductive trend determination and extrapolation. It is inductive because it requires no explanation of why the trend is as it is. It just is, and that is enough for the forecaster.

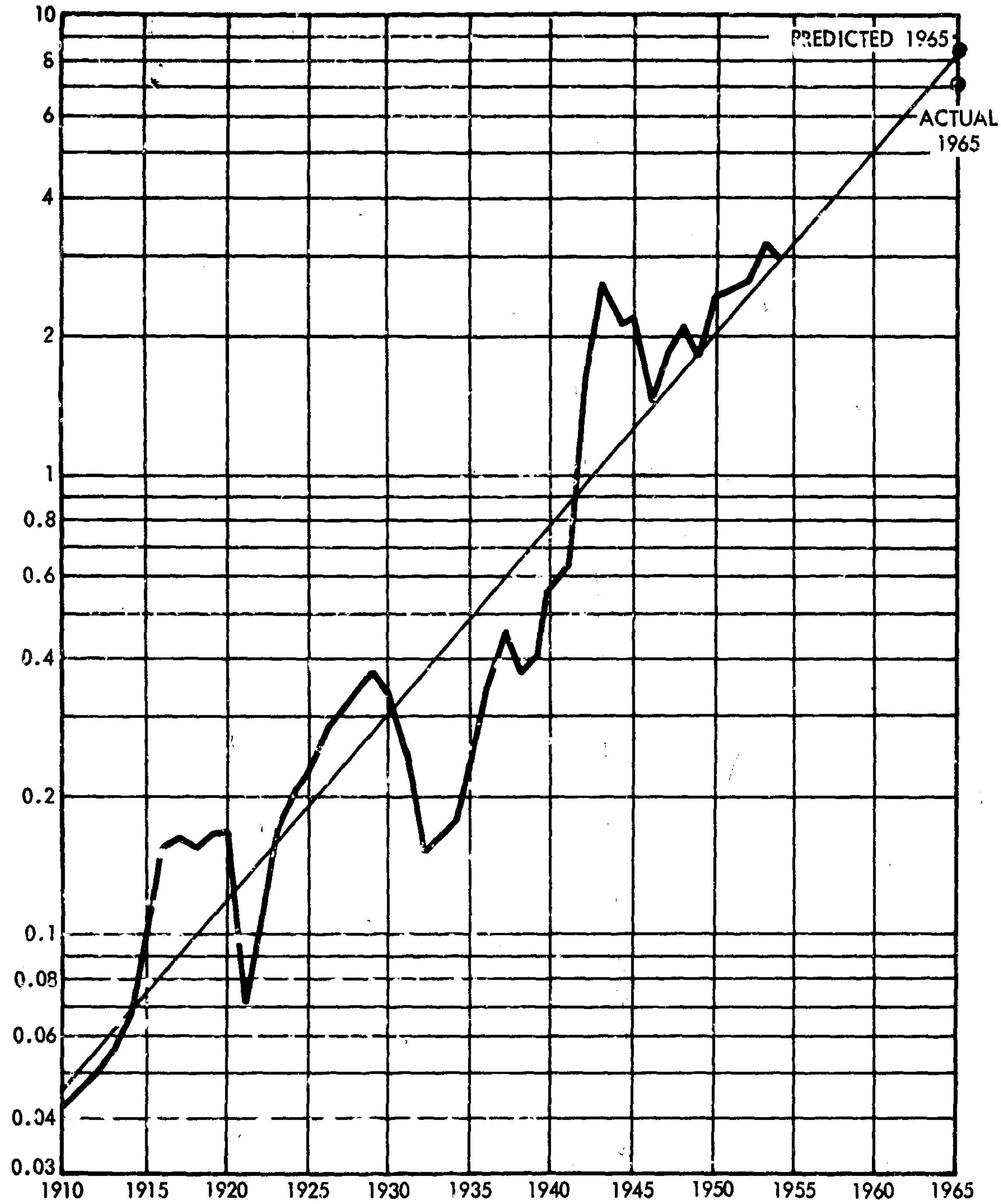
In 1957, James E. Rosenzweig [4] plotted aluminum consumption data for the United States for the years 1919–54 inclusive (see fig. VII–1). He extrapolated the observed trend to a forecast demand for 8.59 billion pounds in 1965.

Deviations from a straight-line course are probably easier to see than deviations from anything else, so the analyst is happiest if he can manipulate his data to show a straight line trend. Rosenzweig used a logarithmic scale for aluminum demand. Other transformations are possible.

Then trend line can be drawn and extended by eye; but almost always the analyst will use statistical methods. There are two pitfalls to watch for in such use of statistical method.

The first is to take the trend line as the forecast, without realizing that future values may vary from the trend by as much as past values have. A main reason for my using Rosenzweig's aluminum forecast as an

[4] In his "The Demand for Aluminum: A Case Study in Long-Range Forecasting" (University of Illinois "Bulletin," 54, 63, April 1957).



Source: Rosenzweig.

FIGURE VII-1.—Calculated aluminum consumption in the United States, 1910–65.

example is his calculating and supplying the standard error of his forecast. His forecast of demand for aluminum in 1965 was one-tenth of a standard deviation off from the actual demand.

But the second pitfall to avoid is to take statistical measures of variance too seriously. These measures were derived on the assumption that deviations from the trend are independent of one another; and historical time series rarely show random deviations. Rosenzweig's aluminum data show a coefficient of serial correlation of +0.80. Each point is like a moving average over a period significantly longer than a year, and so the actual fluctuation in demand for aluminum is much greater than the data reveal.

So don't take a trend line as representing what is going to happen, without allowing for variance; and don't take statistical measures of variance too seriously if serial correlation is present. Taken together, these considerations may make a good-looking trend line rather useless. A 95-percent confidence interval that covers a 100-to-1 range may be of limited utility.

The second kind of quantitative forecast is even more subject to pitfalls than inductive trend determination and extrapolation. It is the attempt to find and display periodic fluctuations in the value of a parameter.

In two circumstances, searching for periodicity makes sense. One is when some reflexive mechanism behind the phenomenon under study makes it fluctuate periodically. For instance, some diseases may at one time have traveled around the world, returning to the site of a former epidemic when the population there had lost most of its immunity. This may have caused periodically recurring epidemics. But technological change has largely destroyed this reflexive mechanism, if it ever existed. Diseases, as well as people, now travel faster; but immunization and antibiotics offer new controls of epidemics.

The other circumstance warranting search for periodicity is control by a naturally recurring phenomenon, such as time of day and time of year. For example, the average daily production of electricity in the United States shows a regular secular trend; but it also shows a 5-percent semiannual fluctuation about that trend, peaking in summer and in winter (see fig. VII–2 and VII–3).

Cyclic analysis has become highly sophisticated, for example, in analysis of communication traffic demand. A somewhat different technique, harmonic analysis, has been borrowed from communication theory by C. W. J. Granger to forecast the sunspot cycle.[5] But in all of these instances the physical basis of the periodic fluctuation is evident. Human beings are so prone to see cycles where none exist, and have wasted so much time trying to forecast the stock market from sunspot cycles, that the warning is probably justified: Don't use periodic analysis unless you know how the periodicity comes about and why.

These first two quantitative methods are largely inductive and heuristic. By contrast, the third is deductive and theoretical. It consists in applying to the data growth curves derived from theoretical assumptions about the mechanism by which the parameters change. In a paper at the Lake Placid Club last spring, for example, Ralph Lenz applied to military aircraft speeds a curve derived under the assumption that the speeds of which aircraft are capable change annually by a factor proportional to currently achieved speed, times a factor that goes to zero as achieved speed approaches orbital speed (see fig. VII–4.)

Forecasting by use of theoretically derived laws of change is subject to the same statistical limitations as trend extrapolation. Slight changes in the assumed mechanism of change can yield large differences in the forecast. But testing a variety of plausible assumptions against the data may yield some insight into what is going on to cause the observed change.

The three preceding methods seek to express the observed and predicted changes directly as a function of time. The fourth quantitative method, trend correlation, expresses the measured quantity as a function of other, intervening variables, themselves considered functions of time.

At first, this might seem like exchanging one forecasting task for many, with no clear advantage. But if the intervening variables chosen are actually important influences on the parameter under study (or conversely), the residual variance can be significantly reduced. After his inductive trend determination and extrapolation of aluminum demand, Rosenzweig investigated the effect of several intervening variables and chose two as significant: GNP and aluminum/steel price ratio. A multiple correlation with these two intervening variables reduced his standard error of estimate, for 1965, to 20 percent.

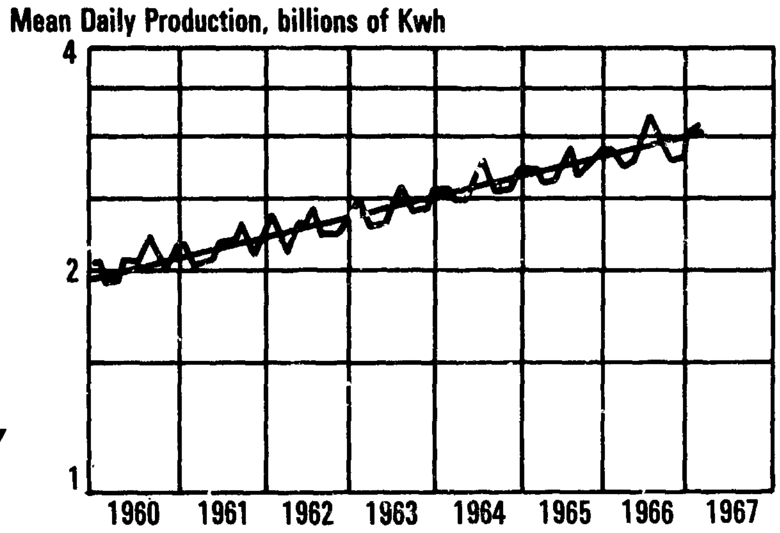
The same caveats about variance and serial correlation apply to multiple correlation as to those methods that study phenomena directly as functions of time. In addition, the form of the relationships among the dependent and the intervening variables is uncertain and is not often investigated in any great depth.

The final method of quantitative forecasting consists in making a complete model of the process under study. The model may be analytical, digital, or stochastic. Any attempt to be rigorous and precise in such a model leads to many equations and many variables, and therefore is best treated with a computer.

The demographic projection methods described in a preceding paper by Richard Irwin were complete modeling techniques, of the particular variety that

[5] In "Spectral Analysis of Economic Time Series" (Princeton University Press, 1960).

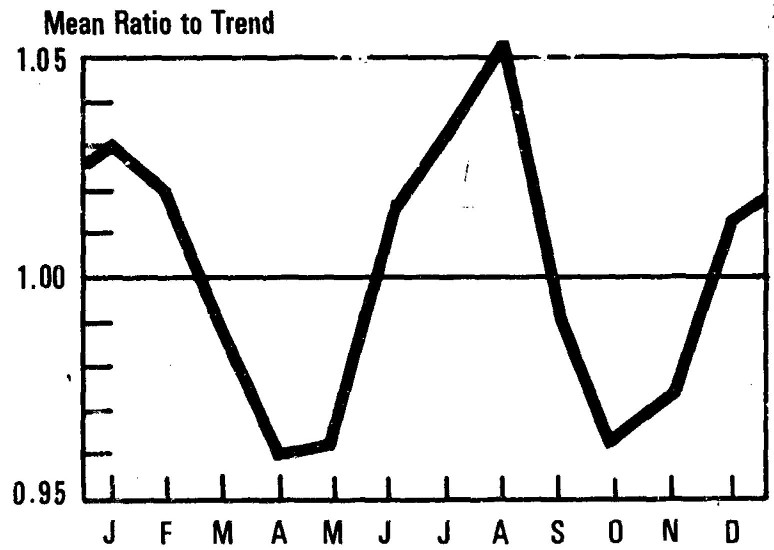
LONG-TERM TREND IN U.S. PRODUCTION OF ELECTRICITY



Source: Federal Power Commission

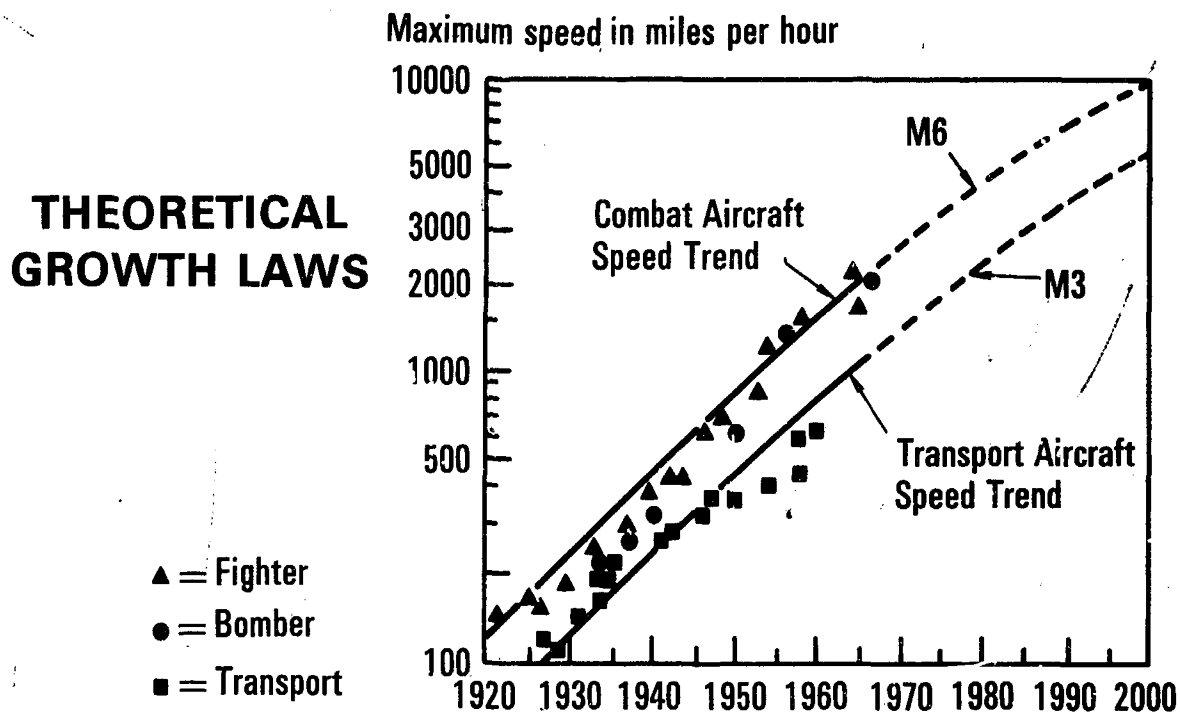
FIGURE VII-2.

SEASONAL VARIATION IN U.S. ELECTRICITY PRODUCTION



Source: Federal Power Commission

FIGURE VII-3.



Source: Lenz

FIGURE VII-4.

uses difference equations. As his discussion illustrated, the attempt to erect a complete model leads you almost inevitably back from considering quantitative to considering qualitative changes.

IMPACTS OF INNOVATIONS

So much on methods of technological forecasting. More detail, and more justification of some of my statements, is available in my NSF and LRPS reports.

As you remember, the basic idea was to combine methods of technological forecasting with methods of assessing economic and social impacts of past innovations. I thought that assessing past innovations had been well studied, so I tackled forecasting first. Then I turned to methods of assessing socioeconomic impact.

To my surprise, all of the studies I could find were guilty of the post hoc, ergo propter hoc fallacy. If they were studies of hybrid corn, they blamed all the changes in American agriculture on hybrid corn. If they were studies of farm mechanization, they blamed all the changes on the tractor. And so on. I got an economic historian to make a more exhaustive literature search and analysis for me, and he came to the same conclusion.

In this impasse, I arrived at three propositions:

1. There are no technological forecasting methods. There are just forecasting methods. Most of the methods I have outlined were in fact developed first for biological, economic, or social forecasting. The method appropriate in a given set of circumstances does not depend so much on whether it is technological, economic, social, or demographic phenomena so much as on the morphology of the data.

2. A method of assessing socioeconomic consequences of an innovation that escapes at least partly from the post hoc, ergo propter hoc fallacy is as follows:

Describe relevant developments following introduction of the innovation; describe relevant following developments were the innovation not introduced; and take the difference between the two sets of developments. This is the impact of the innovation.

3. It makes little difference whether it is past or future impact you are studying. When it is past, you have one extrapolation into the unknown to make: What would have happened had the innovation not

been introduced? When the impact is in the future, you have two: What will happen if the innovation is introduced, and what will happen if it is not?

This, then, is the method developed for anticipating socioeconomic consequences of a major technological innovation:

Using the forecasting methods analyzed and organized, describe what might happen, or might have happened, were the innovation not introduced; describe what happened or might happen after its introduction; take the difference. The remaining task was to test the method, by applying it to a post innovation.

Example: The Transistor.—After consultation with the National Science Foundation and a fair amount of analysis as to what would constitute a suitable innovation for study, we chose the transistor. The transistor could be fairly unambiguously defined; I could understand it; and we felt that plenty of data would be available. At the same time, it was not so minor as to constitute no test of the method.

The idea was to go back in imagination to some point in past time and use only data available at that time, and see if we could make a forecast using that data extending to some point in the more recent past. Comparison of the forecast with actuality would constitute the test of the method.

Our first intention was to cut off at the end of 1956, thus allowing a forecast over the decade 1956–66. But the available information through 1956, both qualitative and quantitative, was just not extensive enough for meaningful forecasting. So we moved the cutoff to the end of 1957. This gave us two 9-year periods: from the announcement of the transistor in December 1948 through 1957; and 1958–66 inclusive.

I began by describing economic and social conditions in 1957, giving evidence that would justify a forecaster's thinking that major social and economic trends would continue for a decade as they had in the recent past. Among this evidence was a forecast made for Tempo in 1957.[*]

Between 1941 and 1957, the electronics industry had grown about 11 percent per year; but there were signs of stagnation or maturity in 1957. Production increase between 1956 and 1957 was only 4.6 percent, and many indexes were off. But three technological developments often associated with the transistor were already in being, and had arisen independent of the transistor: miniaturization, printed circuits, and computers.

Historical quantitative data on the early history of the transistor proved unexpectedly difficult to come by. JETEC—later EIA—had accumulated and circulated a lot of data, but they were proprietary and therefore not available for this study. It proved necessary to tabulate what data we could from the trade press and from advertisements.

In this way we were able to develop four time series that gave some basis for extrapolation: Total transistor production (fig. VII–5) transistor/tube price ratio, number of transistor types, and performance parameter (fig. VII–6). This parameter, $p_d f_c^2$, where p_d is device power dissipation and f_c is maximum operating frequency, is a quite generally appropriate figure of merit for active electronic devices originally developed for microwave tubes. In addition, we had some data on foreign transistor production, but not enough for meaningful time-series analysis.

Analysis of and extrapolation from these data led to the following quantitative forecast of the state of the transistor art:

> By 1965, U.S. transistor production will have reached at least 6 billion units, and may possibly reach as many as 600 billion. They will have become thoroughly competitive in price with vacuum tubes: the ratio of transistor to tube prices will average somewhere between a third and three times as much per unit. In these circumstances, performance characteristics will be fully as important as price in dictating which device is chosen. Transistors with a dissipation rating of 1 watt will have cutoff frequencies as high as 50 Gc/s. There will be at least 2,650 registered transistor types, and the number may possibly run into the hundreds of thousands, although it is difficult to see how so many types could be registered, kept track of, and advantageously used. Other free world countries will compete effectively with the United States in producing and marketing transistors. Of these, Japan, already producing a fifth as many as the United States, is in a particularly strong posiion.

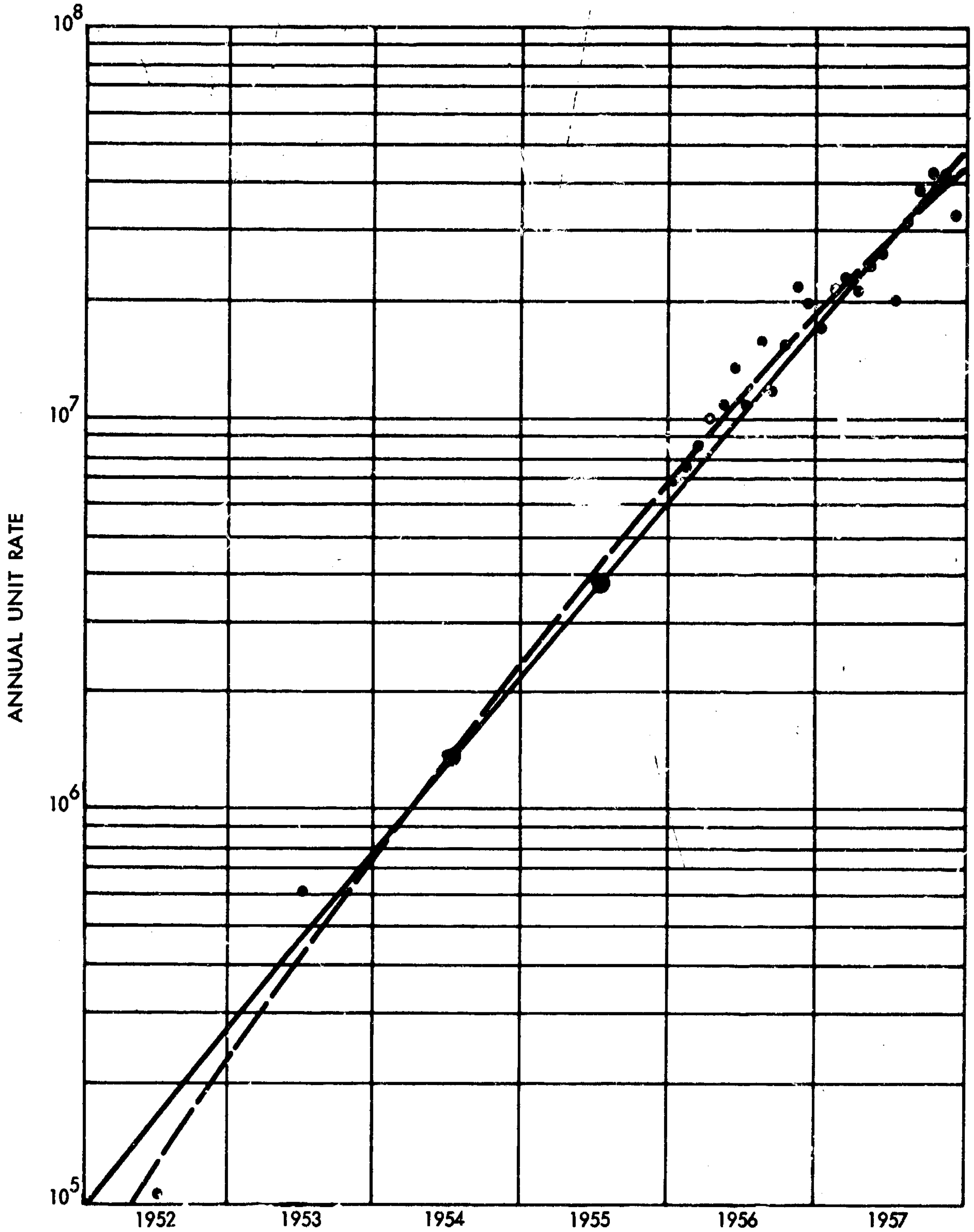
Two of the quantitative forecasts proved greatly overoptimistic. Transistor production in 1965 was only one-seventh of the minimum prediction, and the power-frequency capabilities of current transistors are orders of magnitude less than forecast. These predictions therefore were not quantitatively fulfilled. They might still have served as useful indicators for the planner.

The other two extrapolations—number of transistor types, and transistor/tube price ratios—fell comfortably within predicted confidence limits. Japan's 1966 transistor production was 72.4 percent of that in the United States.

The application of several different methods of forecasting qualitative technological changes led to this forecast:

> The wealth of possible transistor materials and configurations that knowledgeable experts could have enumerated in 1957 as worthy of exploration would have given a forecaster grounds for expecting that transistor technology would continue to advance over the ensuing decade at a rate comparable to that prevailing

[*]"1970: Challenge to Planners" (General Electric Co., Santa Barbara).



Source: S.R.I.

FIGURE VII-5.—*U.S. Transistor production, 1952–57.*

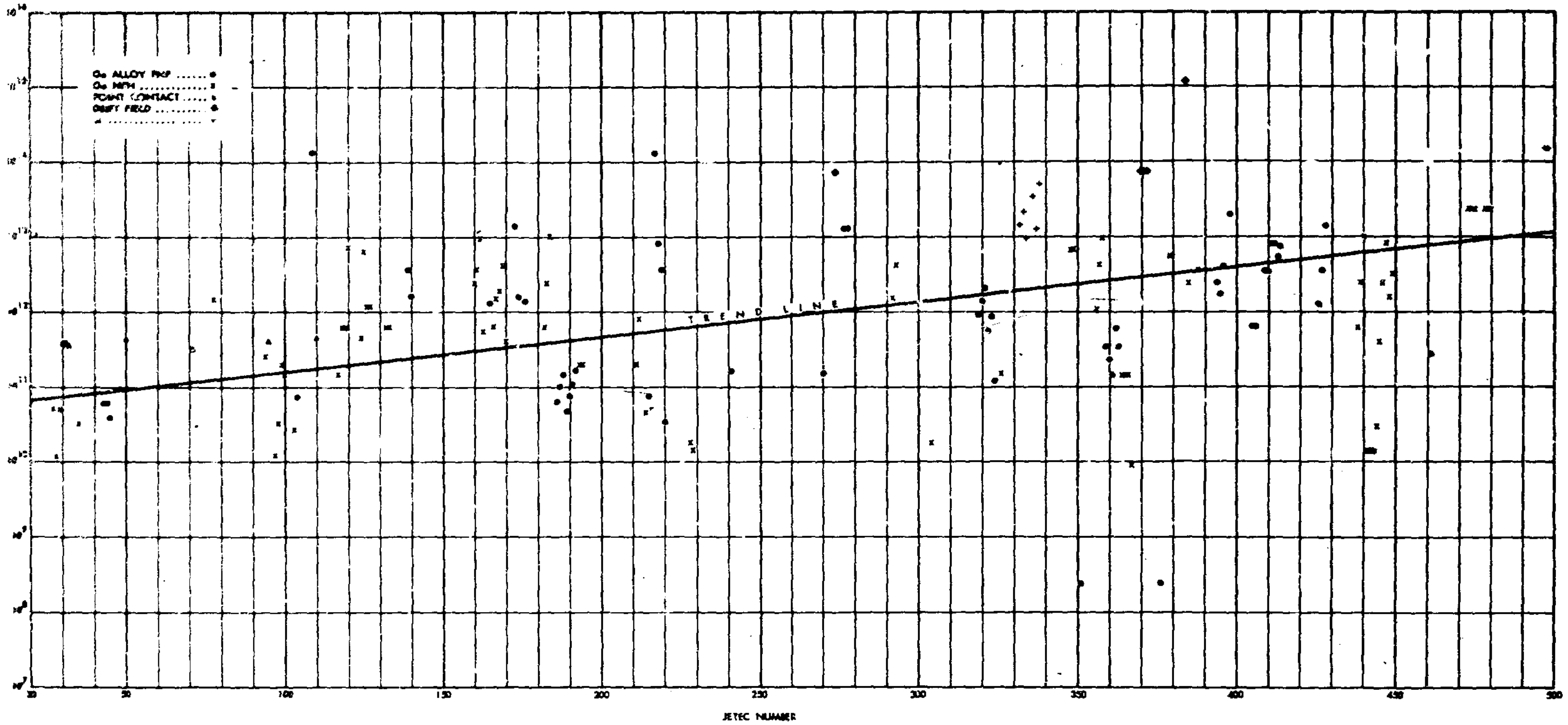


FIGURE VII-6.—*Transistor performance as a function of JETEC number.*

through the preceding one. Shockley's expectation, first published in 1950, that semiconductor electronics would compare in magnitude and complexity with all of the rest of electronics, seemed well on its way to fulfillment in 1957. The pattern of applicability, already highly complex in 1957..., could be expected to continue to increase through additional applications. However, in none of the applications... had the transistor become dominant. At the point for each of several important applications—hearing aids, computers, and domestic radio receivers, for example—when transistor prices and capabilities made them better than competitive with vacuum tubes and when designers had acquired the skill necessary for designing with them, design effort would switch from tubes to transistors.

These forecasts were borne out by developments through 1966:

Every one of 40 hearing aids evaluated in *Consumer Reports*, January, 1966, was transistorized. In the 1957 Spring and Summer [Sears] Catalog..., every console phonograph and radio-phonograph and every portable radio, radio-phonograph, and phonograph listed is transistorized. All but 4 of 19 table radios are transistorized.... Only in the television set has the transistor not taken over from the tube in consumer electronics.

The partial success of these forecasts of transistor technology lends some credence to the assessment of the impact of the transistor on the electronics industry:

...the U.S. electronics industry continued to grow at its historical rate of 11 percent per year, instead of "slowing down," as it might have had it not been for the transistor. Consumer electronics products continued to grow in sales volume. Production in the United States of transistorized radio receivers increased in volume by 9 percent per year, but declined somewhat in total value. This relatively slow rate of growth of volume and decline in total value of receiver production is undoubtedly due in large part to Japanese competition....

The expectation that by 1965 there would be a half-dozen or less major suppliers of transistors in the United States has only partially been borne out. Some 41 companies produce some transistors. But not more than a dozen of these are major producers, and not more than six account for a majority of the transistors produced in the United States.

Although quantitative data are not available, there is qualitative confirmation of the "forecasts"... of the revival of standard-band, amplitude modulation broadcasting and of the shift in consumer repair services almost entirely from radio to television....

The major impact of the transistor on industry in general was through its influence on the cost and therefore the extent and distribution of automation. It was easy to see (and was foreseen) in 1957 that one major impact of automation would be on the size of the civil-service force and in the distribution of demands for skills in the labor force. Interestingly enough, there were forecasts about a decade ago that automation would produce large-scale unemployment as its first effect; but these forecasts were not made by people in the electronics industry. In electronics journals, the forecast was that automation would sustain employment, at least at first, through its effect on productivity; but that it would cause increased difficulty with structural unemployment. These forecasts, which I embodied in mine, have been fulfilled.

This was about as far as I could go with anticipating the social impact of the transistor. The reason had to do with the fact that this was a feigned forecast, not a real one. I could speculate about a wide range of impacts, from the effect of the personal portable radio on teenage culture to the effect of Japan's economic growth, partly due to the transistor, on Far East stability; but I could not demonstrate that I was not just reading history back into my forecast. So I could not go as far in the feigned forecast as would be possible in an actual one.

CONCLUSION

In conclusion, the feasibility of anticipating economic and social consequences of a major technological innovation has in my opinion been demonstrated. The anticipated consequences are not exhaustive or absolutely accurate, but sufficiently full and precise to be of significant value to the planner.

The process of demonstrating this feasibility has yielded the following methodological results:

1. Forecasting is an application of the results of empirical science on a par with engineering and medicine.

2. There are 10 generic types of forecasting methods, five qualitative and five quantitative, and they embrace all methods described in the literature that are strictly forecasting methods—as over against planning methods.

3. These methods are not peculiar to technological forecasting, but are applicable to economic and social forecasting as well.

4. They are also applicable to answering the question, What will happen—or would have happened—had a given innovation not come along?

5. Answering these questions is necessary in order to avoid the post hoc, ergo propter hoc fallacy in assessing the socioeconomic impact of an innovation.

Major MARTINO. I found your comments on the technological fields quite interesting. I presume you include in this demographic forecasting, which is a little different from the others?

Mr. HACKE. Yes, by implication I answered that query. The demographic forecasting system is one model.

Dr. JOHNSTON. I think you probably covered this technique as well, but I wish I had been able to anticipate some of the things about your paper. You mentioned the qualitative techniques—I guess it was this trend correlation technique. Have you tried the lead-lag indicators for shortrun purposes? That is, if you have a technological factor which is a close relative of one and the other is going ahead. Have you had any luck with this?

Mr. HACKE. I haven't used it; just analyzed it. I would include this lead-lag as a form of trend correlation.

Mr. IRWIN. I have a question of detail regarding trends correlation. This is with respect to using regression analysis, either simple or multiple regression and concerns the case of extreme values. The ones I refer to are related to employment and population change by State. In all the data I have, 1940–1960, are three States that drove very fast: California, Arizona, and Florida, and a couple of States are losing like crazy: West Virginia and Pennsylvania. Usually the other 45 States or so, are in the center and very often are not correlated. It looks like Denis Johnston's chart. But with those five States that spread in a line like the moons floating around Jupiter you get very good correlation value when you put all the numbers in, like 0.98, 0.97. Take them out and it goes down to about 0.6. Is there a recommended technique for dealing with this? What would you suggest?

Mr. HACKE. What are you correlating with?

Mr. IRWIN. I usually have migration as my dependent variable. For example, take regional shift in the share of each industry by State in the forties and correlate it with net migration in the forties and you get very fine correlation if you leave all the States in, but California is always there, Arizona and Florida way out, everything going up, and on the other side, everything going down.

Mr. HACKE. I think what you are saying is these five States are obvious in their advance, but if you include all the other States in you get a higher correlation than if you include them out. Is that right?

Mr. IRWIN. I didn't think of doing that. I said, "Look, I've got to see this value about these States, so I pulled them out."

Mr. HACKE. You pulled these five States out and ran the correlation again? What happened?

Mr. IRWIN. It dropped way down. The point is: Supposing I go to the next step and use my multiple regression equation or a simple regression equation as a predicter. I can say for sure I am going to predict these five dynamic States pretty well, assuming the future will be like the past, but what about those in the middle? Granted they are not changing as much and I can't make as bad a mistake because you are not actually forecasting change, but it puts the use of regression equations in question and this is the question I am raising.

Mr. HACKE. No, I don't think it does. You are saying essentially is you don't have enough data on the migration of these in-the-middle States to make a medium for prediction. We're going to have inmigration or outmigration. That's about all I can say, however you have to do a more detailed analysis. It is not the form of the regression that's at fault. It is just that you need more data.

Mr. IRWIN. We took a survey for the Office of Economic Opportunity. Collected data and found one datum missing. We have an imputation procedure whereby we impute characteristics to a person on the basis of six or eight other characteristics. However, the imputation procedure did a bad thing on professional policemen. They used what they called "a hot deck". People are coming and going in the imputation, then when characteristics are missing, you look in the hot deck to see who was the last one in that followed these characteristics and the implication is a geographic connection, which happened to be haywire, so this full-time professional policeman of fairly high talent received a salary of $100 a year—the consultants working with the data were extremely unhappy and said it was going to ruin their regression equation. Should we use a tool where a crazy value will ruin it, especially when you start getting complex models that use stepwise multiple regression with say 50 variables, and makes a choice on an F-test of every variable when you don't know what goes into it?

Mr. HACKE. Nothing comes out of a thing better than what goes in.

Mr. IRWIN. There is nothing I can do about the input.

Mr. HACKE. A committee is a group of people who individually do nothing but when they meet, they decide nothing can be done. Let me say this: I think one of the major advantages of tech forecasting is how much you know about it.

Mr. SMITH. In answer to Mr. Johnston, we have done some forecasting using trend correlation and components, I guess you'd call it, component trend correlation and I have a report that was published before. It is an old report but shows that using this component trend correlation that in the past year and a half since we accomplished work, our forecasts seem to be quite valid with this particular method. I've given the report to Mr. Hacke and I am sure he would be glad to have you look at some of these curves.

Major SWETT. I have a definitional question. You were describing extrapolation as inductive. Would it be correct to say the fitting of a curve by regression technique is the inductive process whereby you would have a hypothetical function, but then the extrapolation itself is a deduction based on the hypothesis?

Mr. HACKE. I've been around this semantic merry-go-round a couple of times and don't know a better way to say what I want. Of course, drawing the line or making a particular forecast is a deduction from the mathematical expression you generate, but generating the extrapolation itself, the mathematical curve itself is an inductive process. Looking at the data, you say, "Gee, it looks like this. I assume the generality is going to be as follows."

Major SWETT. What occurs is a hypothesis bases on the data points.

Mr. HACKE. Yes.

Dr. LINSTONE. The sociological consequences of major technological innovation brings to mind something we experienced during the Mirage studies. The first one in particular where, you might say, military technology invaded the military area and its impact. An analysis of the rapid deployment with a Lanchester-type analysis has shown the implication was quite evident that the faster you go into a situation and nip it in the bud, the less force is needed. As I say, this was kind of in our innocence. I'm more and more skeptical that such a result is meaningful, because this is a socio-implication or psychological implication. It doesn't always work the way theory predicts. All that happens is you tend to go into more places. Nothing apparently reduces the deployment of troops.

Mr. HACKE. You go into more places with less and less until you end up with nothing.

Mr. LINSTONE. You have the desire and the capability to use more and never therefore economize with less. I don't know if it's true but it is kind of a feeling nowadays. This beautiful argument for more airplanes, etc.

Major MARTINO. I think Mr. Linstone's question is the same regarding social implication. He is suggesting the naive prediction, from a better military capability, is that you do the job with less people and a more sophisticated implication is that you will contract to do more jobs and still use the same number of people. Would you care to comment on either of these predictions?

Mr. HACKE. I think there has to be recognition and all of us in forecasting are much too fond of linearity. If one man can build a house in 30 days can 30 men build a house in one day? There is an optimum beyond which this total force would increase. I don't know whether this is what you are suggesting, but there is an area of compensatory magnitude in many situations that operates to reduce the impact of the socioeconomics of many technological innovations. If we put the effort on miniaturization and ruggedization in vacuum tubes that we have p[illegible] in transistors, I don't think our electronics technology would be greatly different from that which we have today.

Dr. LINSTONE. There is something that affects forecasting. We rarely see any document or discussion of negative needs. What do I mean by negative needs? We all talk about what new thing will be added. I don't know what pyschological factors are operating but I think there is a real problem. People are not going to give up anything. I think that's more true than people recognize as the negative forecasts.

Mr. HACKE. One example which illustrates what I think you mean is—there was an ingenious new type of vacuum tube developed about 8 years ago with a 4-year lifespan and that was a low-anode-voltage vacuum tube. It worked fine as an RF amplifier in conjunction with transistors in the audio section. It was used for a while and then suddenly transistor technology passed it by.

Dr. LINSTONE. It isn't precisely like that. I'm not talking about a new item that might not be the best. For example, in the Delphi there was forecasting of breakthroughs. These are new advances the positive side of the ledger, so to speak. I would like to see a corresponding list of things to discard or things we won't have.

Mr. HACKE. Are you talking about things that have a disfunction, a disutility?

Dr. LINSTONE. Yes.

Major SWETT. This discussion belongs in a class of problems involving waste, and accumulation generated by the existing system.

Dr. LINSTONE. When you have supply over demand — you have more of something around, then you have a disutility function and it is the same problem with eliminating atomic wastes, smog, and bacteria; and our own waste that you can't get rid of, this is an area that has been practically unaddressed so far. This problem came up at one of the Howze boards. One of the obvious questions Mr. McNamara wanted answered was: What don't we need and what can we get rid of?—this caused more agony than the whole new concept. It was decided a narrow gage locomotive was something you don't need.

Major MARTINO. This is something about vested interest and all that implies.

Dr. CHACKO. I would like you to comment on something which is related to both questions and problems that have been raised recently. As you know, Congressman Daddario introduced a bill for technology assessment. He told me Congress bought off on the idea of supersonic transport without much critical review. Then SRI showed there will be a sonic boom when the supersonic transport travels over landmasses as opposed to water surface. This was not practical, the question arises: From your study of this transistor radio or allied work, is there any methodology we could see that will address a more general question of technology assessment, or fallouts, of any innovation? Do you have any words of wisdom to offer?

Mr. HACKE. In the first place, we were trying to demonstrate a method or develop a demonstrated method, rather than produce a study that may have some words of wisdom to come from this kind of question. But from other work that has been conducted at SRI on educational policies, research centers, I am speaking for myself as a participant in the study and not for the study, this series of questions relates to the question of values. When you get into the area of human or humane values it is easy for things to become superficial. There has been so much superficial writing and speaking on this topic. A second point regarding Daddario's proposal was for a technology evaluation adjunct to Congress. We were saying that undoubtedly the time has long since passed when we needed a staff in Congress to give them a way of acquiring, at least as a planner or decisionmaker, some insight into the technological process and a means for evaluating the approach goals and objectives through the use of technology. The new scientists have an idea of setting up what amounts to a technocracy to formulate goals and plans but it is ideologically dangerous. At least in a democracy the feedback process from goals to plans should include the populace. I think Congress has the responsibility and it cannot escape. They can have a staff but its line function is goal formulation and plan adoption.

Dr. DALKEY. You partly answered my question but I would like to get it on the record. There is a rather amusing paradox particularly in the use of growth curves to forecast technological developments and at the same time assess the impact of those technological developments. You said, you thought you could make an as-if kind of assessment of what would have happened if the transistor had not been put in the technological stream. If we take this notion that lies behind the use of the growth curve, which is that a form of technology will arise and keep that curve going, if the transistor hadn't been invented, then presumably something else would have kept the growth curve going; in which case you might say the impact of the transistor is to the first order of approximation null, because if it hadn't been invented something else would have taken its place.

Mr. HACKE. I would not make exactly that assumption but go ahead.

Dr. DALKEY. There is a slightly amusing paradox because if you go back in the past and ask the question: What would have happened if such and such hadn't been invented? You would be in an interesting position of having to invent what would have been.

Mr. HACKE. You are precisely in the same predicament as the forecaster who cannot imagine the inventions or innovations that are actually going to be made. The railroad system and the way it has been developed, for example, might well have been an invention that never would have been needed because of the horseless carriage. I don't know, I'm just a forecaster.

Dr. DALKEY. If you are trying to evaluate the impact of a named item you can say the impact of any given named item is essentially zero because if that named item hadn't arisen, something would have taken its place.

Mr. HACKE. Let me use the transistor as an example and incidentally, the choice of the transistor came as a result of a study of a fairly large number of nominated innovations because it was completely defined. It was fairly unambiguous. A border has to be drawn between other solid state devices and the transistor, but otherwise you have a straightforward job. There are many innovations that are hard to define. Specifically with the transistor which is noted in the report, I think you can say: had the transistor not been developed, true technology would have continued to develop at approximately the rate it did before the transistor came along, therefore, some compensatory mechanism would have operated with penalties in terms of weight, cost, and power consumption. The forecast of the transistor is not nil. You say something else would have been invented. I can't answer that; I can only carry it as far as I can investigate the alternative.

Dr. DALKEY. I don't want to push it too far but if you are going to apply things like growth curves in the future, wouldn't you be willing to apply them to an uncertain past?

Mr. HACKE. Yes.

Dr. DALKEY. It seems this would be rather an amusing exercise.

Mr. HACKE. I think methodologically also you are on the same ground trying to forecast what will happen.

Colonel VAUGHN. You discussed 10 different methods but I imagine none of them are used in their pure and sole state; that there's always some mixture.

Mr. HACKE. There are a couple that are used alone, or in a fairly pure state. One is the use of experts, inductive trend determination, induction and extrapolation. These occur to everybody to try first. I think I can anticipate what you are going to suggest but you should use as many different methods as possible as a check.

Colonel VAUGHN. I was wondering if that is good policy? It appears to be good technique for a person to use one against the other and hopefully use several to check against.

Mr. HACKE. Yes, you can.

Colonel VAUGHN. We sponsored a study at Syracuse University, which was basically an expert report bound together and more or less correlated. At least they all used the same language; but I can imagine that each person in his own disciplinary area probably used all 10 and found more.

Mr. HACKE. My experience from use of experts and I have been an expert in a forecast and have seen these things—many of the books you read, from H. G. Wells on, are expert opinion books—is that the expert generally operates by using historical analogy and inductive trend determination. The expert uses the method and his methods are subject to analysis as are anybody's.

Mr. SMITH. Colonel Vaughn, on using several different methods, from the experience we had—we used growth curves, trend correlation and experts and put together a tech forecast about a year ago. We found almost complete disagreement between the experts, growth curves and trend correlation, almost 100 percent in every forecast. We finally resolved it by taking the growth curves and trend correlation, because we could document it against many opinions, and found experts quoting historical analogy to contradict our curves. You can't mix the three, not in our experience anyway.

Colonel VAUGHN. It might be interesting to try the Delphi technique.

Mr. HACKE. I am unhappy about the title growth curves. If you discuss growth curves you also have to talk about decay curves, but if you discuss decay curves, everybody gets unhappy. Will you assume a mechanism of change and investigate mathematically what that method means? These have been derived by analogy, from bacteria, and I'm not happy about bacteria.

Colonel DAVIS. I took the opportunity after your lecture, Dr. Dalkey, to show where it seemed feasible to fit a growth curve or logistic curve to the outcome of a prediction by a group of Delphi experts, and since all of the Delphi curves have this characteristic, one might think it possible to do that with others. Consequently, the results of Delphi people may lead to a phenomena which can be described or a description of a phenomena that could be characterized by growth curves. I wanted to remind you so you won't be too unhappy with growth curves in the event they would be useful to characterize mathematically some of Delphi results.

Dr. DALKEY. I hope I didn't give the impression I was unhappy with growth curves. On the contrary, growth curves seem to have a great deal of validity and have a life of their own. This is what results in the paradox.

Dr. CHACKO. You asked for another word for "growth." I have used "life cycle curves."

Major MARTINO. That is a good terminological suggestion.

Dr. CHACKO. One of the major technological innovations is probably a better nuclear detection device and when we sign a test ban treaty, we will know who is cheating. The technical means to detect this should be available. Should this come about and lead to a deescalation of the current armaments race, one of the major problems is that of economic impact. I am sure you have studied the report that was published about 2 or 3 years ago showing the probable impact on several firms largely dependent on defense contracts. I suggest this will be a major technological innovation and will have serious major socioeconomic consequences. Would you comment on how you look at this problem?

Mr. HACKE. Although this is not a quarrel with the problem you pose, I don't think the question is a technological innovation. However, an economic and political innovation is there. We have adequate test devices for all practical purposes. One of the things I think you are asking is: that this would be, as all forecasts have to be, a conditional forecast based on the decisions made by significant actors in the situation. The United States will undoubtedly adopt some programs of one kind or another to ameliorate the employment impact and the like of partial or total deescalation. It will depend—the forecaster should give the planner in the light of the innovation what the probable consequences of the visible alternatives would be so that he could make a selection, and could exercise judgment to decide which and what strategy to use.

Major SWETT. I'm not sure correlation would be of interest. The Delphi pattern of convergence through reiteration shows a split logistics curve normally.

Mr. HACKE. What do you mean by split curve?

Major SWETT. A split curve on both sides. If you are looking at the convergent cone, each half of it looks like a logistics curve normally. The technological development curve oftentimes also shows a logistics shape. Psychologists use the same term and found it repeatedly in their learning phenomena. They called it a learning curve. It seems both pyschological and technological progress show a characteristic of learning.

Mr. HACKE. I wrote a section of this report which attempted to demonstrate this. The normal curve of error can be deduced from very general mathematical assumption. Therefore, it has a wide applicability in its description and I think the same general assumptions can be made by the nature of technological progress, and will yield the same kind of curve. It is not accidental that the estimates people make of an imperfectly known quantity in the learning curve and many growth curves are all the same shape.

Dr. JOHNSTON. I remember in the courses where we have anything normally distributed we accumulate the normal distribution by logistics and this was discovered in the 1920's again and found to apply to a vast range of phenomena and it is dangerous to forecast from it because it is difficult, on a time scale, locating yourself on that curve.

Mr. HACKE. Yes, until you get to the inflection point, you don't have enough information as to where you are going, and then it is too late.

Major MARTINO. The same is true of Zipf's Law. Because of some fundamental mathematical things it turns out to have wide applicability. But you run into the same kind of a trap. You never know whether this is one of those cases.

Mr. HACKE. Until it is too late.

Major MARTINO. You mentioned Terman's forecast made in 1932 regarding frequency modulation. The reason Terman was wrong in this case was in the 1920's people first started looking at frequency modulation and not understanding the situation. They believed one could transmit the same amount of information over a narrower bandwidth which would be wonderful. Finally, they understood this was not so. that, in fact, FM would take more bandwidth or as much. All the experts soured on FM. They were completely against it because it would not give the advantage originally hoped for. The advantage of FM lies in another field. It allows you to get rid of noise and the problem is that Terman was answering the wrong question.

Mr. HACKE. Yes.

Major MARTINO. You have to be aware of this problem in dealing with an expert. Make sure he is answering the right question.

Mr. HACKE. I agree.

One of the most widely used forecasts currently being prepared by the U.S. Government is that of the weather While weather forecasting has been practiced by farmers, sailors, and other for centuries, the use of scientific tools for forecasting is a comparatively recent innovation. The problem of the weather forecaster, since the introduction of scientific methods, has been that he can have access to more data than he can possibly use, in the time he is allowed to make his forec[illegible] The advent of the digital computer is already changing this situation, however. Commander Hamilton's paper, on numerical weather forecasting, presents some of the problems of a forecaster with a plethora of data, but whose theory about the meaning of the data is not always adequate to the task of preparing reliable forecasts.

—Editor

NUMERICAL WEATHER FORECASTING TECHNIQUES

Comdr. G. D. Hamilton, U.S. Navy, Fleet Numerical Weather Facility, Monterey, Calif.

The development of numerical methods of weather analysis and forecasting is one of the most significant and spectacular advances in the field of meteorology in its long history.

The numerical prediction process attempts to forecast atmospheric motions by formulating them in mathematical terms. To accomplish this, Sir Isaac Newton's laws of motion, which were developed for bodies or particles subject to impressed forces, had first to be applied to a fluid medium. This was done in 1755 by the German mathematician, L. Euler; however, it was not until the latter half of the 19th century that Euler's hydrodynamical equations were applied to the study of the atmosphere. A treatise, published in 1888 by H. von Helmholtz, a scientific genius of his period, applied the hydrodynamic equations to the atmosphere.

It might be wondered, therefore, why our interest in solving these equations is so belated.

The answer is probably twofold. In the first place, the equations are simply so difficult to solve. In mathematical terms, the difficulty is one of solving a general boundary—and initial—value problem for a simultaneous system of six nonlinear partial-differential equations in three dimensions. Even today, there are no known methods by which the solutions of such equations can be related explicitly to general boundary and initial conditions.

Exact analytic methods failing, the most satisfactory course would have been to solve the equations by purely numerical methods. These, however, were not fully developed until early in the 20th century and, in any case, would have required an enormous volume of computation.

The second and probably more important obstacle to the early development of dynamical weather prediction was the sparse meteorological data. The dynamical meteorologist of that day did not really know what kind of phenomenon he had to explain, and could not fully test any theory that he might propose to account for the behavior of the large-scale weather disturbances.

The realization that the general hydrodynamical equations could be solved in principle, and if necessary, by purely numerical methods and sheer brute force, appears to have first occurred to L. F. Richardson, a highly original British meteorologist-mathematician-economist-statistician who also had a lively interest in the new finite-difference methods. During World War I, between ambulance trips to the front, he designed and carried through a finite-difference scheme for solving the nonlinear hydrodynamical equations for meteorological purposes—the first genuine attempt at dynamical weather prediction. Anticlimatically, Richardson's results disappeared in the general confusion of the war, together with the computations he carried out laboriously by hand over many months. They were later found under a coal heap and published in 1922.

Richardson's experiment was not an unqualified success, since his calculations predicted that large-scale weather disturbances would travel at about the speed of sound and in the wrong direction. In addition, he estimated that it would take 64,000 human computers operating simultaneously just to predict weather as fast as it happened. Numerical prediction could not, therefore, become a practical reality until the development of high-speed electronic computing machines.

Another very important factor in the development of numerical prediction was the establishment of an international network, of upper air observing stations. Our present day network, while still sparse over ocean and tropical areas, is fairly dense in many land areas. Furthermore, the observations are not confined to the earth's surface but data throughout most of the vertical extent of the atmosphere's mass is obtained by balloon soundings. Thus, a three-dimensional picture of atmospheric motion is obtained.

By the end of World War II, the stage was set for rapid progress in numerical prediction. The design of the necessary high-speed computers was still in progress when a numerical prediction group was formed in 1946 under the leadership of J. von Neumann at the Institute for Advanced Study at Princeton. Sponsored by the Office of Naval Research, this group simplified the hydrodynamical equations under certain limiting conditions, so that they could be solved with the aid of computers. Several solution schemes or models were developed, tested and successfully applied to predicting large-scale atmospheric motions.

There were two serious defects in Richardson's work. First, it is a matter of experience that the atmosphere is always very close to a state of mechanical equilibrium; i.e. the horizontal pressure-gradient force is almost exactly in balance with the Coriolis force, and the vertical force of buoyancy is almost exactly balanced by the virtual gravitational force (the resultant of the earth's centrifugal force and pure gravitational forces).

Accelerations of air are dependent upon the small difference in these forces and pressure gradients and winds cannot be measured with the required accuracy. Second, the hydrodynamical equations are general and include high-speed sound and gravity waves as well as the meteorologically important long atmospheric waves. This may lead to computational instability and the solution will blow up unless the time increment is less than the time required for sound waves to travel the grid distance over which the finite differences are taken to approximate derivatives. To provide sufficient resolution of the pressure field, the grid distance must be taken in the neighborhood of 200 km., which therefore requires a time increment of about 10 minutes.

From physical considerations, there is little reason to expect that such high-frequency waves would have much significance in large-scale meteorological phenomena, and it is therefore desirable to filter out these waves. When this filtering has been accomplished, larger time increments (of the order of an hour) are permissible for the individual steps of the forecast.

This is the type of filtered model that was first computed by von Neumann's group in 1950 on the ENIAC computer. The modified hydrodynamical equations describe a relatively simple model atmosphere (barotropic) and it has rather serious shortcomings. It implies that the number and intensity of cyclonic and anticyclonic vorticities cannot change and accordingly cannot predict the formation and growth of new disturbances. Thus, in order to deal with these problems, more general models have been synthesized. Many of these models have been developed, tested and shown that relatively crude methods of dynamical prediction were as effective as subjective techniques in the hands of a skilled forecaster and were accurate enough to justify putting them into practice. Today, computing machines have taken over major segments of the routine weather forecasting job.

With the advent of larger and faster computers, more sophisticated models appeared which could predict the formation and strengthening of new pressure centers. However, they rarely predicted the occurrence at the right times, at the right places or at the correct rate. Discouragingly, the more complicated models have not shown significantly better skill than the barotropic model.

At Fleet Numerical Weather Facility (FNWF) a modified barotropic model is in use as no known prognostic model has proven capable of consistently outperforming the FNWF model. A great deal of the success displayed by this model comes from its having been tuned (i.e. adjusted for maximum accuracy) over a period of several years.

The Weather Bureau has recently instituted a numerical model based on the hydrodynamical equations in their unmodified or primitive form. With the speed of the third generation computers, it is now feasible to integrate the equations in short time steps. However, all models suffer from the lack of observational data. Among existing operational centers, differences in initial analyses are often as great as differences in 24-hour forecasts. Without a correct analysis, any forecast, no matter how good the model, is going to be in error. A plateau has seemed to be reached in numerical forecasting and any substantial increase in accuracy can only be attained with an improvement in initial data.

The greatest increase in forecast accuracy since 1960 was achieved recently when the U.S. Air Force implemented its Automated Weather Network (AWN). Observations are collected from regional teletype weather circuits terminating in computers at High Wycombe, England and Fuchu, Japan. This data is immediately transmitted by high-speed communication links to a collection center at Tinker AFB, Okla. From there, it is relayed together with western hemisphere data at approximately 6,000 teletype error-free words per minute to FNWF with drops also at the Weather Bureau at Suitland, Md., and the Air Force Global Weather Central at Offutt AFB, Neb.

Within a few years, this initial stimulus was followed by the establishment of similar research groups in Sweden, England, Japan, and Germany. In the United States, a landmark was reached in 1956 when an upper-air numerical forecast map was disseminated via the national weather facsimile network to replace a subjectively prepared one.

It is natural to ask what makes the problem of weather prediction so unique and so demanding of human wit and brute machine force.

First, the atmosphere exhibits undulatory and vortex motions on a tremendous variety of length scales. These range from millimeter-size eddies, spinning along a windswept ground surface, to the thousand-mile-size vortices or cyclones associated with major storms. A typical hemispheric weather map is shown in figure VIII–1. All of these motions, small or large in scale, are directly or indirectly essential parts of the weather-producing mechanism. Thus, many observations of variables are required simply to describe the details of the current weather.

Even assuming that the overall effect of very small-scale eddies is determined by events on a larger scale, it is conservatively estimated that the values of at least five variables (pressure, temperature, humidity, wind speed and direction) would be required at 5,000 uniformly spaced points at each of 10 different altitudes to provide a sufficiently accurate description of the state of the atmosphere at a single instant.

Another complicating factor is the solution of the equations. The form of the hydrodynamical equations is such that the instantaneous local time derivative of each variable can be expressed in terms of space derivatives of variables in the same set. Accordingly, if we can observe the initial values of all variables at a network of discrete points filling the entire atmosphere, we can approximate the relevant space derivatives by the method of finite differences, and can compute the initial rate of change of each variable from the hydrodynamical equations. Knowing the initial value and initial rate of change of each variable, we can then extrapolate its value over a very short interval of time at each point in the network. Finally, regard this very short range forecast as a new set of initial data and repeat the process. Thus, it is possible to build up a prediction over any desired period of time as a series of successive forecasts over very short intervals of time.

The real problem, obviously, is to carry out the over 1 billion calculations required for a 24-hour forecast in considerably less time than 24 hours. Herein lies the power of the high-speed computer.

The average number of reports received per operational run is as follows:

Surface pressure (land)	4000
Surface pressure (ship)	550
Upper air soundings	510
Bathythermographs (60 = hour collection period	580
Sea surface temperature (3½ = day collection period)	7800

Figures VIII–2 and VIII–3 illustrate the density of surface weather and upper air reports received for 3 November 1966.

The data is analyzed at Monterey each 12 hours on grids of 4,000 points (Figure VIII–4, soon to be increased to 16,000 points, for the northern hemisphere from a depth of 1,500 feet in the ocean to 100,000 feet in the stratosphere. Hemispheric forecasts are made out to 72 hours for upper-air levels and 48 hours for the surface. These forecasts require about 30 minutes of computer time or on the order of 2 billion computations. The fields are transmitted at 4,000 words per minute to Fleet Weather Centrals in Guam; Pearl Harbor; Norfolk, Va., and Rota, Spain. It is planned in the future to shift to communication satellite channels at up 40,000 words per minute. At the Weather Centrals, the data is tailored for the areas of responsibility and transmitted to fleet users. The Naval Environmental Data Network is illustrated in figure VIII–5. Data tielines (fig. VIII–6.) on the U.S. east and west coasts provide environmental products to Naval Weather Service Command units and other Government agencies.

The growth in computer speeds from the beginning of routine numerical weather prediction is depicted in figure VIII–7. The computational mix for the curve assumes ten adds for every fixed point multiply in typical meteorological work. The figure shows that speeds underwent a steady logarithmic increase during this period.

Although the dollar cost of the computer systems used in meteorological centers has increased in the last 10 years, the time (and consequently, the cost) per operation has shown a remarkable decrease. This may be seen in figure VIII–8 which summarizes the time and cost of five million typical meteorological computations. Figure VIII–9 gives a summary of the Control Data (CDC) computers in operation at FNWF as well as the new STAR computer scheduled for installation in fiscal year 1969. Not indicated is a CDC 8090 communication computer used for data interchange with Tinker AFB in the Automated Weather Network.

With the increase in computer speeds it is now possible to perform numerical integration for long periods of time to determine the atmospheric circulations. Such computations are scientifically important in that they require a formulation of many of the physical processes occurring in the atmosphere. These processes include the effects of orography, moisture, turbulence and radiation. Integrations have been carried out for several hundred days and have given realistic results in correspondence with climatological statistics. However, numerical forecasts still fail to show any realistic skill after 2 to 3 days.

There are many processes in the atmosphere which are poorly understood. They include release of latent heat when water vapor condenses into cloud droplets,

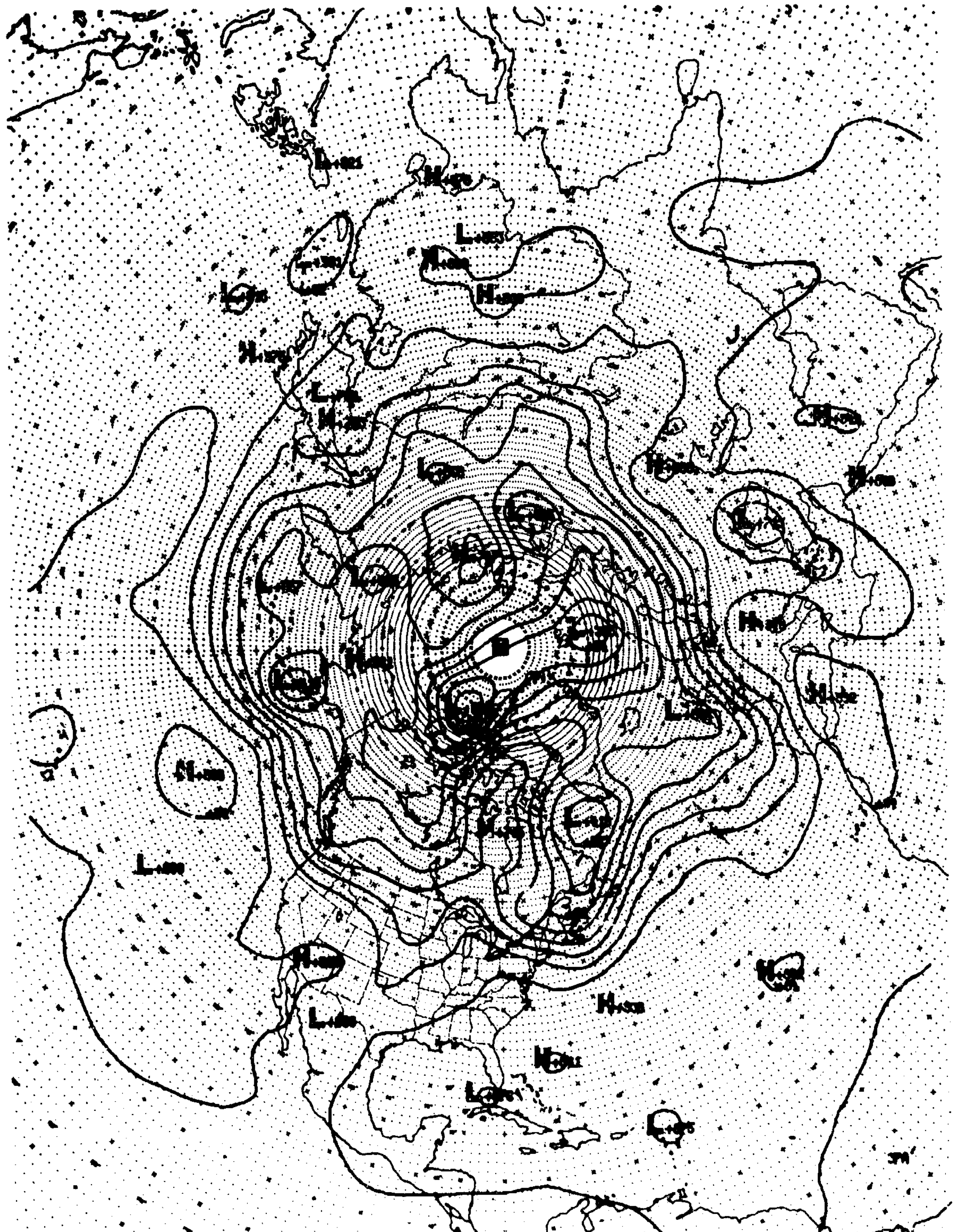


FIGURE VIII-1. *Typical hemispheric weather map.*

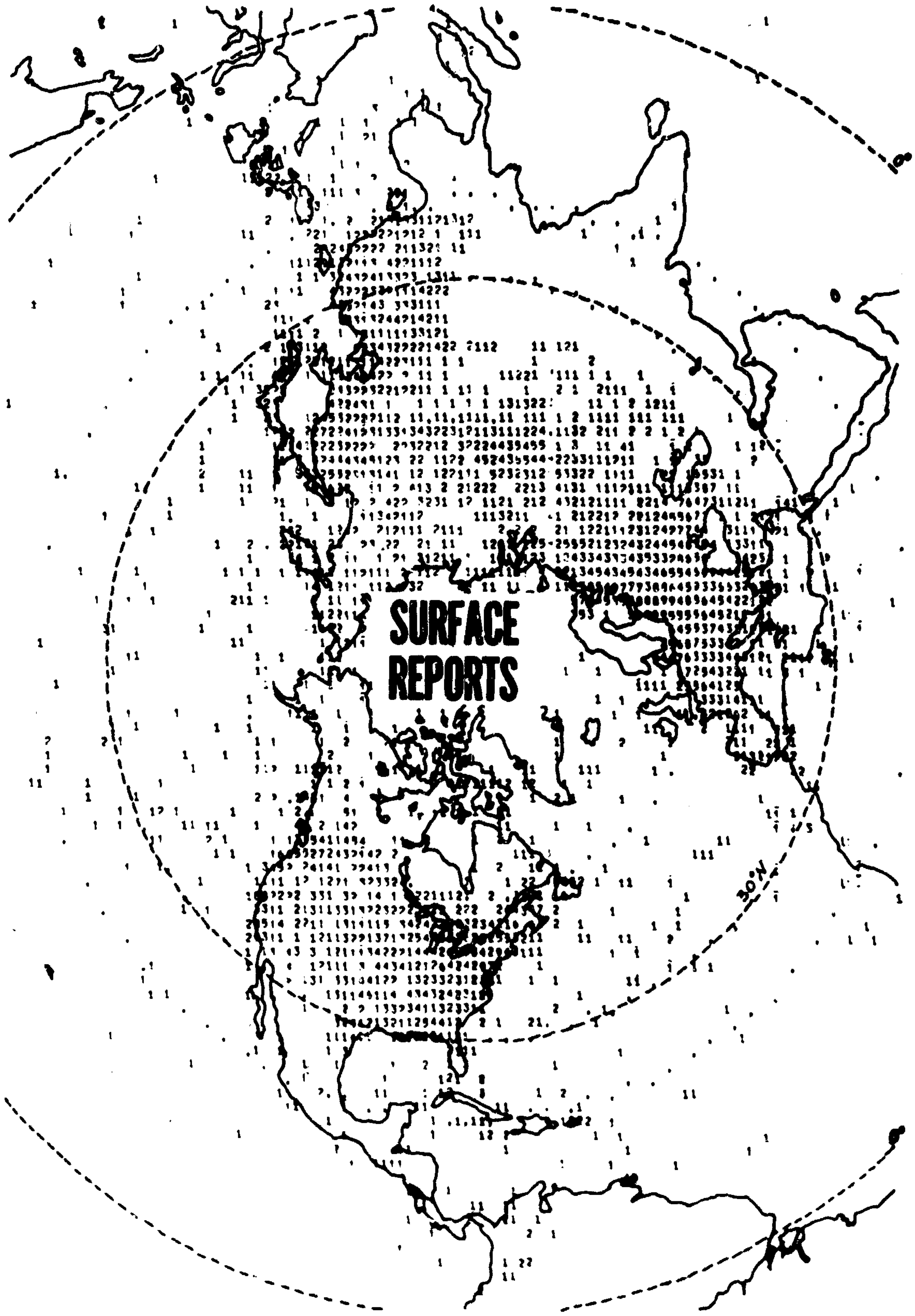


FIGURE VIII-2.—*Density of surface reports.*

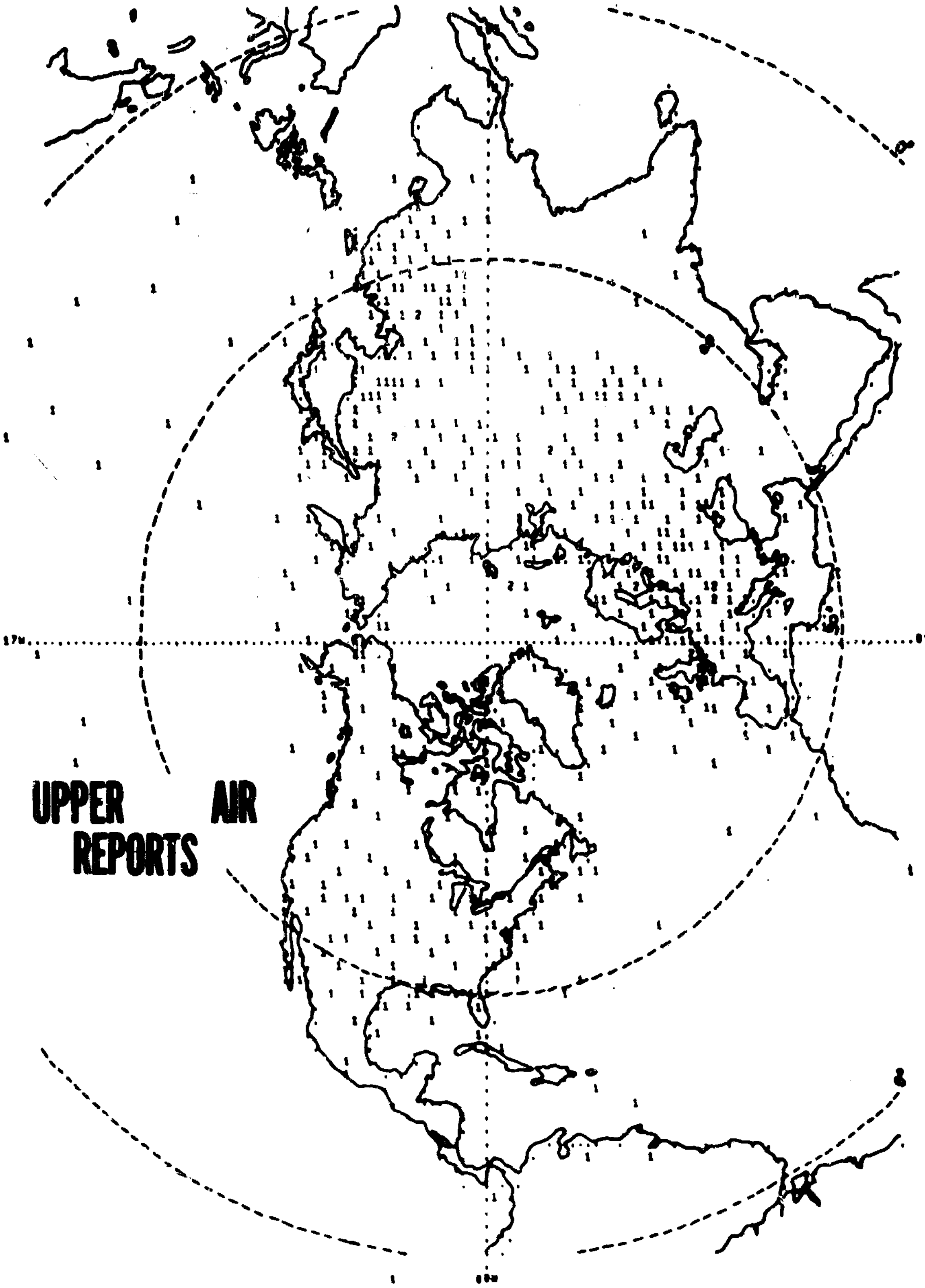


FIGURE VIII-3.—*Density of upper air reports on 00Z 03 November 1966.*

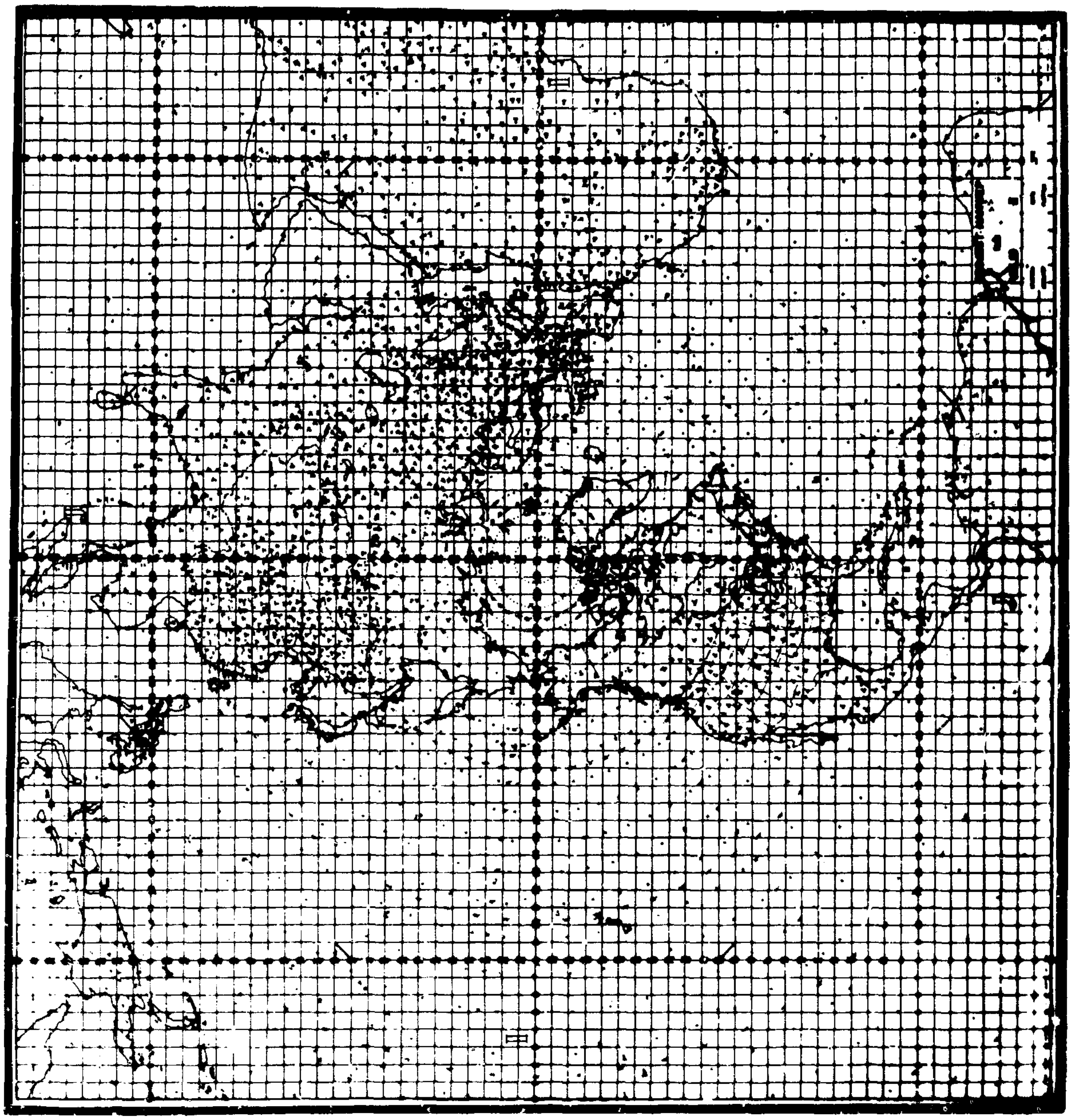


FIGURE VIII-4—*Hemispheric forecasts.*

absorption of the sun's radiant heat energy, loss of heat by radiation and heating by conduction from a warmer land or sea surface. These problems have to be solved before longer range forecasts are meaningful.

What will the decade ahead bring? It is hazardous to forecast the course of science. Clearly, however, it will be required to bring the whole earth's weather under the kind of round-the-world observational scrutiny that is going to be required for operation of the weather forecast models of 1977. A tenfold increase will be required in meteorological coverage by the weather-station network of the late 1970's.

Satellites, free-floating balloons by the thousands, remote, manned and unmanned land and ocean weather stations—these and other tools must be marshaled. Computers, even with the wisest of men behind them, cannot be expected to make accurate 2-week weather predictions, unless they are fed with correct weather information on an hour-to-hour, day-to-day basis.

Perhaps von Neumann was right in the assessment that he made in the mid-1950's—that the weather problem is the greatest challenge for the application of computer science to benefit mankind.

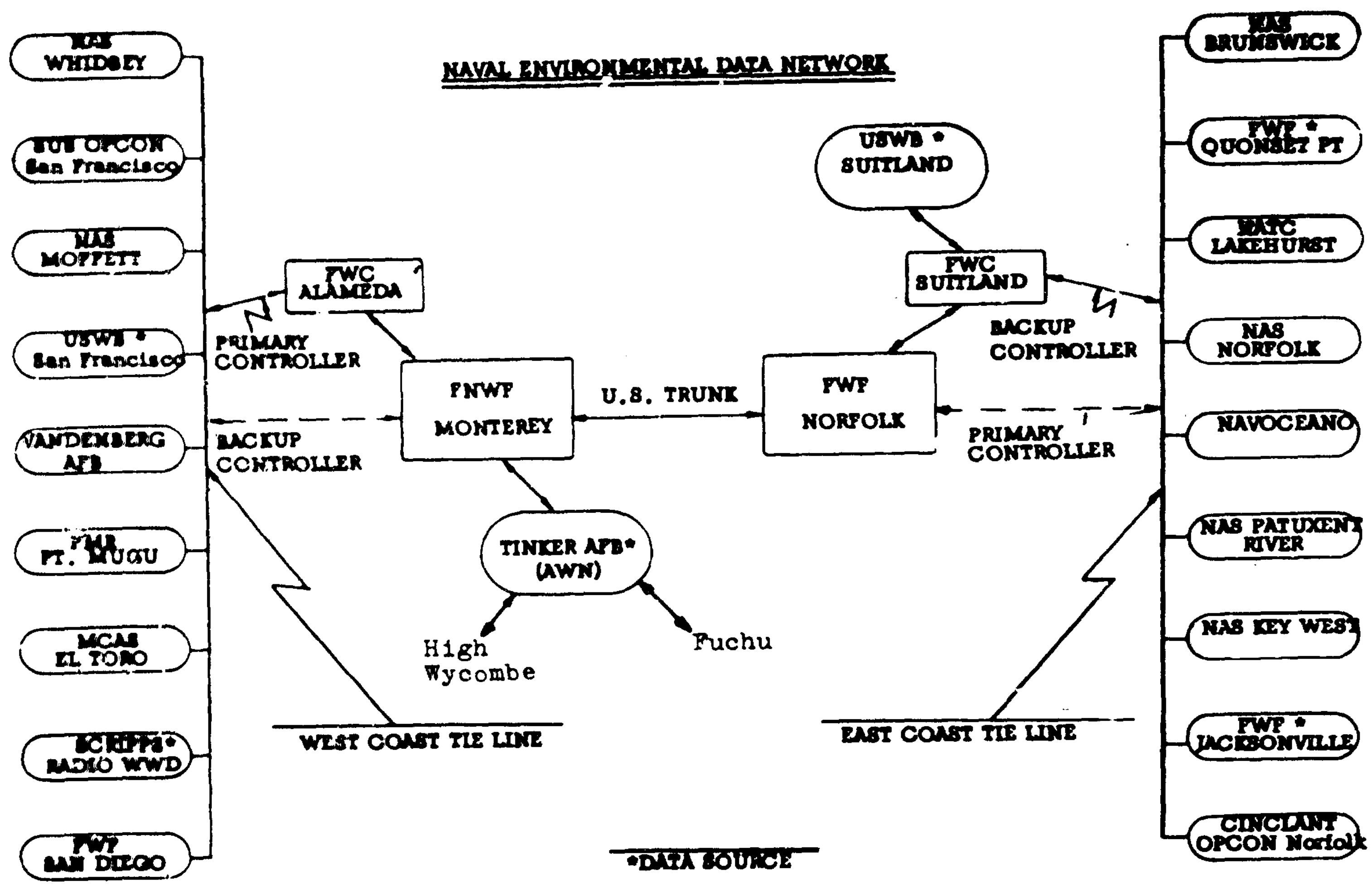


Figure VIII-5.

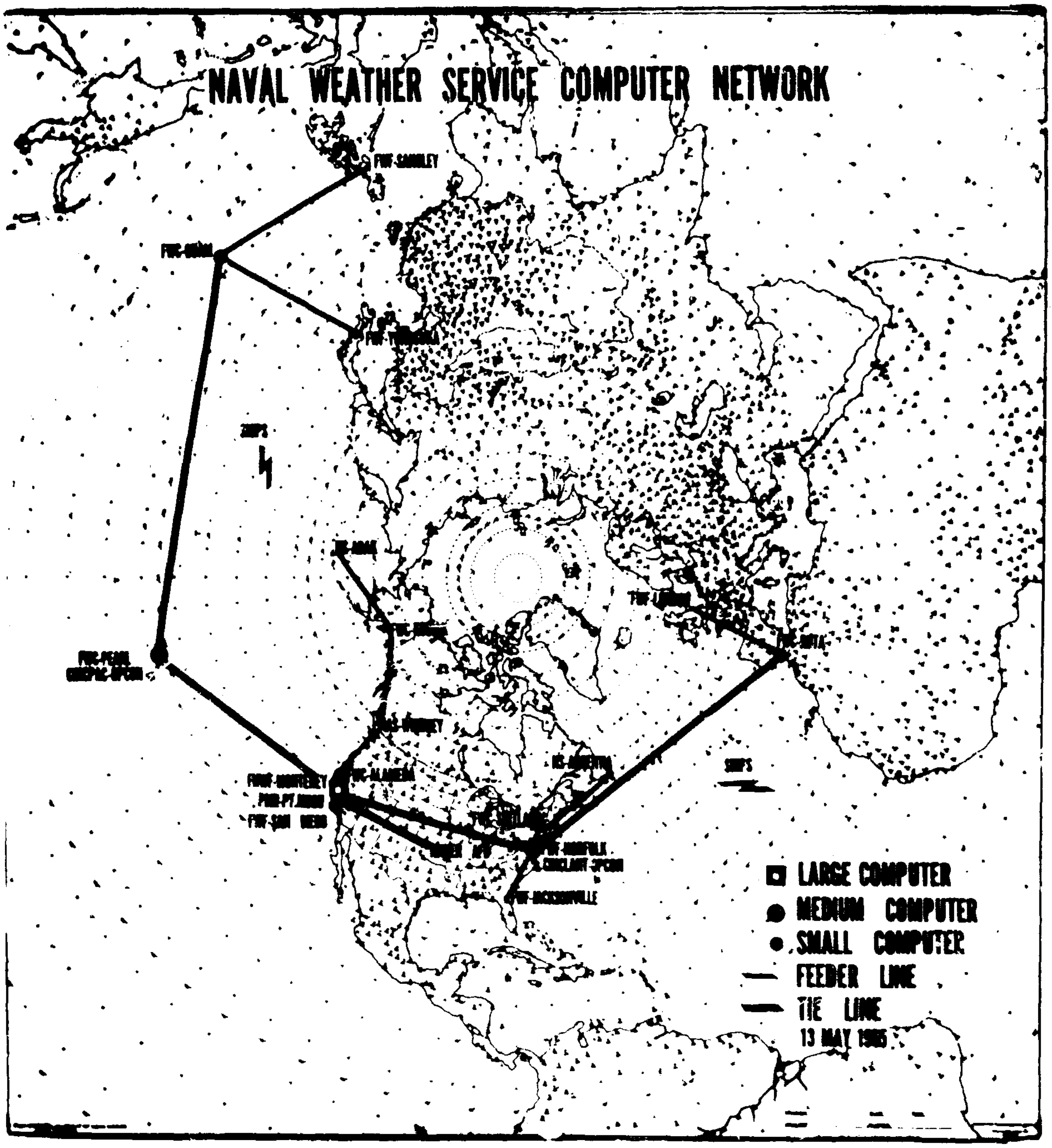


FIGURE VIII-6.

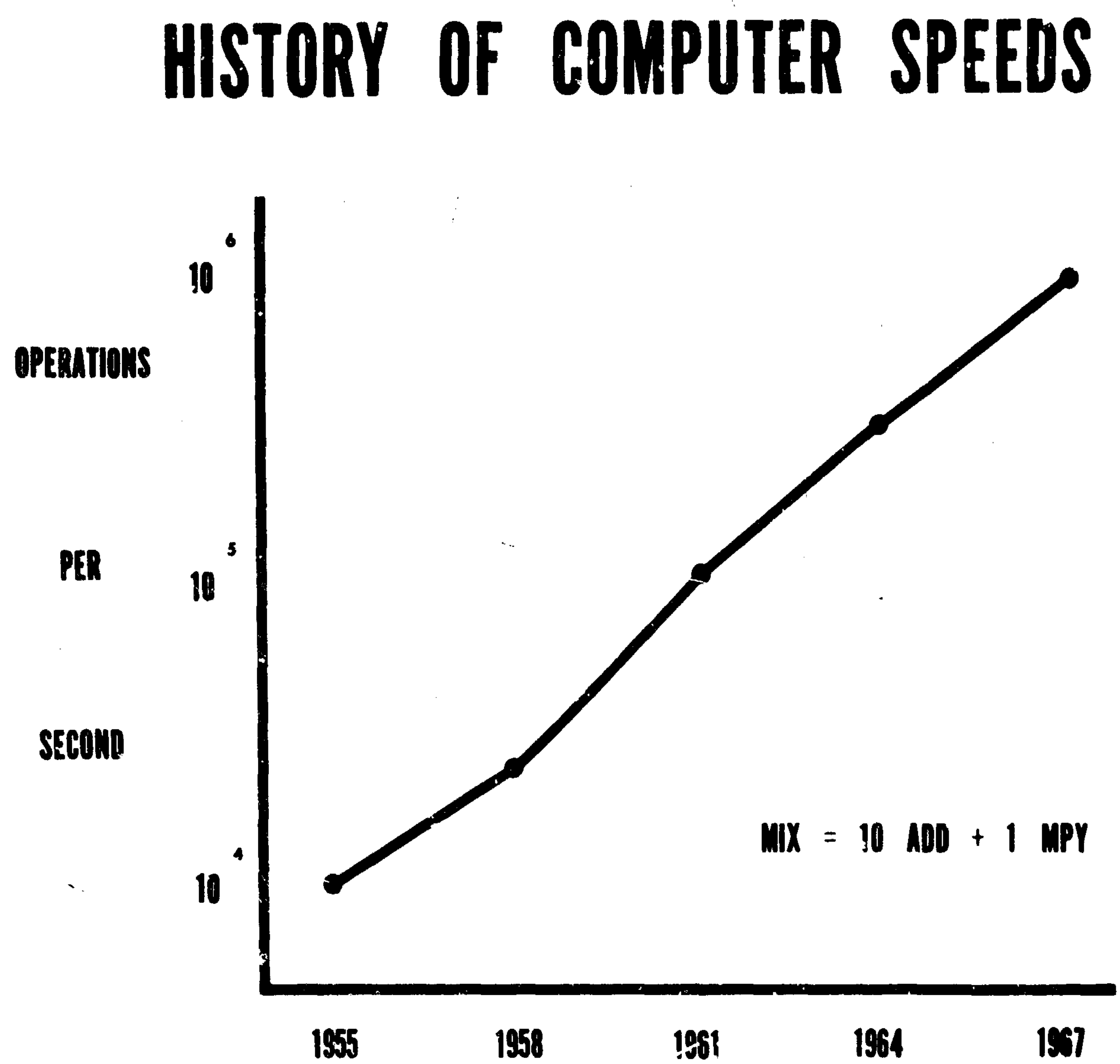


FIGURE VIII-7.

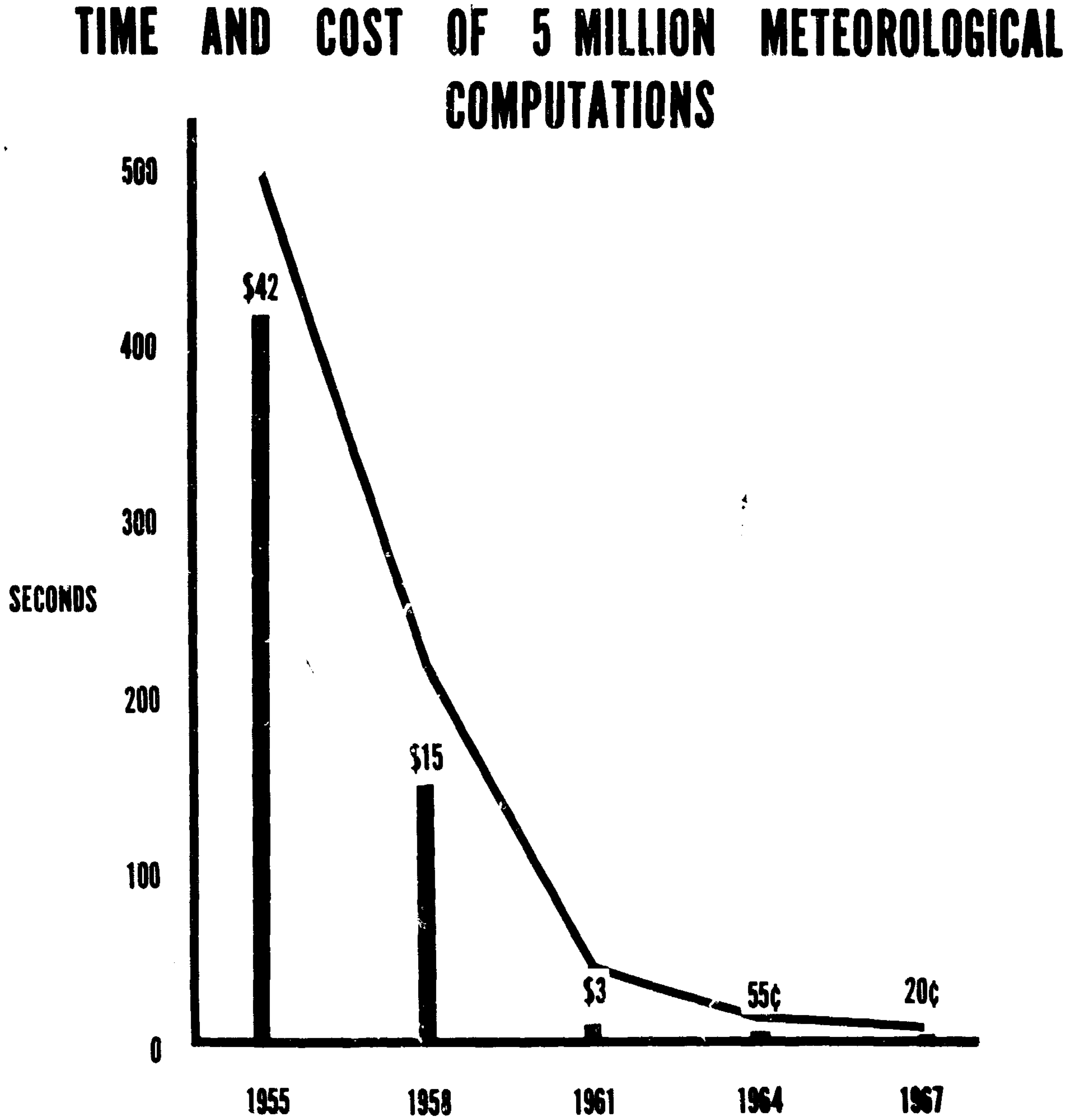


FIGURE VIII–8.

FIGURE VIII–9. –*Fleet Numerical Weather Facility computers.*

Types	*Average operation time (megaseconds)*	*Minor cycle time (megaseconds)*	*System cost (millions of dollars)*	*New*	*Obsolete*
(2) 1604	12	6	1.5	1961	1970
(2) 3200	8	1	1	1964	1973
6500	.25	.1	4	1967	1977
STAR	.01	.025	4	1970	

Mr. Hacke. Are you using LF for communications?

Commander Hamilton. No, they go on facsimile and teletype circuits and so forth.

Mr. Smith. How are you using weather satellites for photography in weather forecasts?

Commander Hamilton. The satellite we feel is basically a research tool. We use it, for example, in areas of no data. This can give you a pretty good idea, for example, that we don't have a storm located quite right—in Washington they digitize this data or focus it, in other words, for the height and we put them into our analyses. This is the only operational use being made of them. It is valuable for plotting storms where data is unavailable. Other than that it is primarily a research tool. The satellite is very interesting and intriguing but operationally it is not much use. Since this is a panel of long range forecasting, I should mention that, my specialty is numerical work, some of the outlook for meteorology in the future, weather control. I am no expert on this—it is a problem similar to that mentioned this morning, of putting oil on the water to stop evaporation. Somebody is going to be hurt—even with hurricanes—first, there is so much about the atmosphere that we don't understand, or even why a hurricane starts. Why is it transformed from a weak disturbance into a full-blown hurricane? A few years ago we thought there had to be ice clouds to act as nuclei to start raindrops then we realized in the tropics this doesn't happen. We don't clearly understand the rain-producing mechanism. Before we can do much about the field of weather control, we have to understand the atmosphere. There is a big push in research in these fields. For example, the rainmakers—one farmer may want rain on his field, but in the city it laps over, and may disturb the businessmen that people are not buying because it is raining. I read, the other day when I was in Stockholm, that the head of the Swedish Weather Bureau was president of the World Meteorological Organization. He was talking about the Russians firing a cannon into the clouds and laughed about it. He thought it was very funny, but the American scientists are taking a very hard look at dispersing hail-producing clouds. This would have a tremendous economic effect on the farmers in Nebraska if they could do this—hail costs them millions and millions of dollars damage a year. This would be profitable to everybody and nobody would be hurt.

Major Martino. There was a specific reason for bringing a paper on weather forecasting in this meeting. This approach fits what Dr. Johnston called predictions. That is, in principal we think we know all the laws that govern the situation and it doesn't fit in the same category as forecasting the future of technology or population, but you have given many things which we suspected. The laws are not fully understood and I think the paper has illustrated the importance and reveals where understanding is inadequate, and, furthermore, reveals where data is insufficient. From one standpoint perhaps those of us in the forecasting business can take heart, even the hard scientists are not much better off than we are and we don't have to hang our heads in shame. Are there any comments or questions regarding this particular paper in relationship with anything else?

Dr. Oberbeck. I feel there are further implications in this kind of paper with respect to the demographic predictions. The motivation for establishing a large data collection and data processing system, is the need for this kind of information. It has many operational uses. In the more complex case of data collection for demographic projection from a census every 10 years, the stimulation for improving forecasting methodology is to find more direct requirement needs for population data. We can conjecture these but I am not clear what the mechanism is to make this kind of data economically useful. I wonder if the people here could make suggestions in the direction of what would be economically useful. I can make conjectures but I would prefer the experts do this.

Mr. Irwin. Are you specifically interested in getting more census data?

Dr. Oberbeck. I am saying for meteorological science, you have an abundance of data and an elaborate mechanism to collect it on a hemispheric scale. The motivation for all of this was someone needed information. It was economically useful and I am sure there must be an analogy for people who need population data. I was conjecturing with Dr. Eber that we have the technology in the United States and in the world today so we could, for example, take census data once a week with satellites. Have population information collectors out continually and let the satellites pick up their information. I wonder if your office has the means for stimulating the use of this data and justifying it economically.

Mr. Irwin. There has been talk of a census every 5 years and I understand the House has passed a law that there will be a census in 1975 and there was considerable debate in public hearings to having it in 1965. Whether there will be an appropriation when the time comes is another story. The feeling in the field is that all people using demographic statistics apparently feel 10 years is too long. It is probably the pickup in the rate of technological process in the recent years and a greater use of demographic data. You may not be aware that every month the Bureau of Census has a sample household survey which collects important data. The design is fundamentally to produce labor force data for Denis's organization, and they provide most of the money to support it—35,000 households——

Dr. JOHNSTON. It's 50,000 now.

Mr. IRWIN. Most of the money comes from the Labor Department in various months of the year—March is a heavy month, also October. Certain questions are put in as riders; in addition to asking the basic labor force data in March, they ask questions about marital status, children they have; and various other things and then we do a collection on special order. The outfit in Census that works up the sample statistics and runs the office is much larger than the population division, and they are located in a couple of large wings. One serious problem is to get more census data. I don't think people will hold still for a population register. I'm not sure a population register is a good idea because this country moves around fast. You've got to generate a tremendous amount of data. There has been a discussion about collecting social security numbers in the next census. However, it has not been decided whether social security information will be collected. In Philadelphia, for example, we were testing procedures for complete coverage of housing population, we're mailed questionnaires, and later we knocked on every door to be sure that all housing units in the area were on our mailing list. About 10 days later we knocked on the door again asking for population information. I observed that the people were extremely reluctant, actively antagonistic: "Go away." There is a certain human equation in getting data from people. I don't know if this is the kind of thing you are interested in.

Dr. OBERBECK. Let me amplify what I had in mind. I want to draw an analogy between collecting demographic data and collecting oceanographic data. In World War II, I was in the submarine service. Just as your householders complained about collecting data from them so the people in the submarine service collecting bathythermic data griped because they didn't know what would be done with it; and it was an interference with their daily routine. An antidote to this kind of reluctance is finding someone who can use the data collected and pay for this valuable information. An improvement for this whole area would be if you could induce the labor force to say, "We really have to have these labor force projections. We will pay for getting the data and get it."

Dr. JOHNSTON. One possibility is if we should get a negative income tax or legislation of a similar type. I would be in favor of it though everyone would receive a check from the Government which would be returned together with more than you ever before put in with you income tax payment, but the mechanism of putting each member of society on a per capita basis—some kind of a valued item like money—this might solve some of our undercount. It would probably give us an overcount, but would serve as a possible entry justifying the collection of needed information without quite giving rise to the hostility and suspicion received knocking on the same door twice. The situation is structured by the respondent in a totally fantastic way. He thinks the FBI is on him or somebody else is after him and you can't undo this structure. You are caught, as Dick Irwin says, in a pathology to where you may get no information or lies. I think one of the long-range schemes in the UN has been for a worldwide sampling procedure but it is not in the planning stage. What is the stage before you plan? You evade, you conjecture—a kind of sampling device you have in weather but on a worldwide scale, getting at people's conditions. A register, as Dick Irwin says, is probably too much although it seems to function well in countries like Sweden and Denmark. It is likely not to be feasible in most places, but a sampling technique on a worldwide scale is not entirely inconceivable. If we can process data for 50,000 households, publish data in about 2 weeks and if we had the real technology in operation we could probably do it faster, there's no reason we can't do it with other households.

Major MARTINO. I'd like to raise another possibility. Is there any reason why the census people could not take into account the same privacy rules and make use of data obtained from banks, credit bureaus, and so on, where in general the social security number is available?

Mr. IRWIN. You could do it with social security. We've made many tests trying to improve the enumeration—I should say in defense of ourselves—I mentioned the undercount—I think the Bureau has accomplished a great deal trying to count everybody and I want to mention—if we had the social security number it would work—but in test after test matching lists against lists, the information is not complete enough. You can't do it and it hasn't worked. You should be aware that the Bureau is making a dramatic change in this next census—the decision had definitely been made to do a mail-out mail-back census to all urban areas in the Nation, covering 80 percent of the population. Jim Hacke mentioned one of the basic problems that came under my observation in the Philadelphia pretest. First, we found in the 1960 census that self-enumeration was the best. The way it worked—there was an enumerator checked on every house, and every fourth house in the sample household an enumeration check-a-box-type questionnaire was left and was mailed in. If there were any problems she talked to them again. We used self-enumeration on the sample data; on 100-percent data there was also a 100-percent pickup so it was not exactly a self-enumeration. We are asking for a positive schedule this time, there's little doubt from it, and the schedule is tough. I am perfectly confident—I am speaking for myself not the Bureau—there are a lot of people who are extremely experienced working in this complicated schedule. We are going to get extremely good information from people with high educational attainment. But I observed a few ghetto interviews of people with an eighth grade education and the household situation was such that you couldn't concentrate. For instance in an academic atmosphere similar to yours, I can use complicated sentence structure and you people pretty well understand what I am saying. However, in the ghetto kids are running around wishing you were somewhere else. The fine print goes flying out the window, and these are a few of the problems in collecting data. I think self-enumeration is the right way to do it.

Dr. LINSTONE. The more I hear about this, the more puzzled I am, considering, this country is the most advanced technologically. I know in Germany before Hitler, every person was required to register with the police. It was standard procedure to reregister with the police every time you moved. One of the reasons for the low crime rate in Japan is the police have an up-to-date dossier on all individuals, which is standard operating procedure. If these countries can do it, I don't understand why we can't.

Mr. IRWIN. First, it is impossible psychologically in this country. Since the beginning of this whole organization, you don't do that kind of thing and that's why the Census Bureau is reluctant to get a social security number and I, as an American citizen, am opposed to it. I think one of the strengths of this country is the freedom.

Dr. EBER. I was in Germany—which is about the size of Texas—for many years and the main thing—if you look at the size of your country—I imagine the reason you couldn't do it is the bigness of the country.

Mr. IRWIN. I don't know much about foreign countries but suspect we are more mobile than anybody. For one thing, we've so many more automobiles.

Dr. LINSTONE. I don't understand the philosophic objection—which sounds like it is anti-American. I don't see why everyone—nobody seems to object to the idea that you are part of the system, however, you mentioned your own opposition to it. Does it inhibit your movements?

Mr. HACKE. I can answer that in a semiphilosophical or theological way by saying Americans suspect that every human being, if you have enough on him, would be sent to jail, and you are not sent to jail unless you are caught. It goes back to the frontier psychology. You don't ask a man where he has been. He makes his way on what he is currently doing.

Mr. IRWIN. In the West nobody asks who your relatives are or where you came from. Nobody cares. In California, they could care less where you are from.

Dr. OBERBECK. Another aspect of this in the reverse sense of the investigation in the ghetto is a personal reaction to giving data to people whose education you suspect may be less than yours. As an example, I find locally getting license plates for your automobile, the people rendering this service aren't on the educational level to understand you want this done quickly. They let you stand around all morning; they're doing their job and could care less about your time. I believe the population data could be done by mail, and wouldn't be nearly as objectionable as having to present myself, say, before a police officer to tell him who I am and answer questions of the kind necessary for a population survey.

Major MARTINO. I would like to expand on your earlier question, Tom. You pointed out that we have mammoth weather service because somebody decided he wanted it enough to pay for it. Your original question was: What would it take to convince somebody he should be willing to pay for data and in this case not only the monetary cost but the psychological cost—what would we have to do to propagandize people so they will go after this census question? What is necessary to make it economically and socially feasible to do what we know could be done, with census takers? I would like to expand that to take in the interests of the other people as well. What needs to be and can be done, economically or socially feasible, to gather the kinds of data we think we need on technology, population and on all the other things that most of us seem to be interested in forecasting? Does anyone wish to address or comment on it?

Dr. OBERBECK. Let me amplify your amplification with a question. In what sense is the Gallup poll economically productive?

Dr. SLAFKOSKY. Newspapers buy it.

Dr. OBERBECK. It's economically feasible, I guess and has been going on for a long time.

Major MARTINO. When somebody is running a political campaign nowadays, he takes a sample poll. To reduce the cost a member of one party will combine with a member of another party, whom he is not running against and is a candidate for a different office, to jointly pay the cost of the poll. It is an accepted method of doing business in politics. Here is somebody who wants it badly enough to pay.

Mr. SMITH. The military has not been unwilling to pay money for forecasts. The Air Force Project Forecast is not cheap, never was. CNM is about to invest—in the millions of dollars—to get a Navy technological forecast. Apparently the concept of the value of a technological forecast has been well established, at least throughout the military. Companies represented by Lockheed and others are investing considerable amounts of money in forecasting. I think we are addressing the wrong question. It isn't what it takes to sell its worth, it is more a question of implementation than a question of need. It apparently has been accepted, perhaps we should address the question of what is the psychological impact or how do we overcome that. In the case of the census, it appears to be a stumbling block and possibly in other services it is also psychological.

Major ERB. I wonder if part of the problem is that some of the disciplines—in the area of international relations and political science—don't have the proper methodology in which to apply the data. There are many people—we've come in contact with groups at Yale and at USC—trying to get contracts to collect data. One reason they want to collect data is to figure what types of models you could build to describe the world if you had the data. I think if the time came where there were sufficiently reliable models available to explain parts of the world people would be interested and willing to pay for the data. Right now we are at the stage where the data are not good enough but the techniques used for handling the data aren't there either.

Major MARTINO. This sounds like a vicious circle. Regarding initiation of operation research studies, without data, you can't build a model, and without a model, you don't know what data to gather.

Dr. SLAFKOSKY. I think we are asking the wrong questions. Tom Oberbeck is asking about the Gallup polls. Dick Irwin is talking about census people recognizing that people have the data and the problem of extricating it from them because they are reluctant. But here it isn't that kind of problem. The real problem is we don't know enough yet. The people who have been working in this area aren't kidding themselves about how good they are. They recognize we are at the beginning of this whole process and it is not a question of how much money is spent but that there aren't enough people who have mastered the technique. It is not only a question of not enough people, also that techniques have not been perfected—if we were able to perfect them, then the data would be relatively easy to get. This is my feeling. I don't know if we could ever compare ourselves with the census, the Gallup poll or anything else. It's a much more difficult process that we are trying to get.

Major ERB. Part of the problem, is that in some areas you don't know what to collect. If you are trying to understand the international system as a system, what are you going to look for? Are you going to look for an area, population, or are you looking for messages transferred between various divisions? We don't have a good enough system to determine what we want to collect even if we had the wherewithal to collect it.

Mr. HACKE. One comment—essentially until you know what you are looking for, you can't see it, so to speak. The other is, I wonder if the question has not been phrased backwards: About how can we get people to invest more money. In census taking, it isn't the question. It's the optimum level of effort for census taking, in general and detail, and how you arrive at that optimum.

Major MARTINO. I think we all suspect we are short of the optimum.

Dr. SLAFKOSKY. It seems we have at least three kinds of information or insights we are looking for. One insight the Major pointed out, namely, where are the conflicts going to be, what is going to be the nature of the conflicts which depends where they are going to take place. The second question is: Given that we know who is going to be doing it how do we come to grips with that nation or that group of people? The thing people have been calling the social technology, which I am always questioning, because it sounds too much like a mechanical thing, it is really relevant. The third: Let's assume we know who is going to be involved. What are the best instruments from the technological standpoint? I am talking about hardware, that will do the job most efficaciously and most efficaciously doesn't necessarily mean what so many of our congressmen now seem to feel, "bomb them off the map" or "overcome them somehow." I think most efficaciously means how do you impose your will—maybe the word "will" is wrong—how do you get the people to work with you as a nation or a group of nations for the betterment of the world. I may sound a little idealist but unless we more toward an idealistic goal, this is going to be a rat race and will end with the destruction of everybody. I may be only talking about three kinds of things; there may be more. But these are the obvious.

Major MARTINO. You raised a point we have to look at. As an aside, it is not clear that we are doing as well by dropping bombs as we might do by dropping money, counterfeit ration cards or whatever most easily disrupts their society. But you are right.

Dr. SLAFKOSKY. The British did this in Malaya, they didn't drop money but, boy, did they use it.

Major SWETT. We might try dropping Sears Roebuck catalogs. It seems to be an implicit consensus that social problems underly technological forecasting. It should be noted that social sciences are dealing with two classes of problems. One is fact and the other normative, "What is" and "what should be;" the should-be side is the leading bias for intentions. A decisionmaker looks at his perception of present and future reality and looks at his perception of what ought to be now and in the future. Between these is the momentum for his action. Then he starts looking for means, he has some that he prefers; and then looks at his capabilities to see which can be used. This question of what ought to be is the same question of utility and drops back into the underdeveloped area of axiology in philosophy and psychology. If we would unlock anything in that area, we could unlock a considerable chain of interdependent problems. I think as far as the implementation of what we can do about our problems is concerned, as a suggestion we might use something like the cohort theory from demography and see if we couldn't study what values, what views of reality, are going into the minds of these cohorts. We have seen the Red Guards in Communist China. We know that 15 to 20 years ago the young children were being taught to kill the "dirty capitalist flies" and shoot the "dirty imperialist rats." They were weaned from milk onto revolution. Revolution and violence became a positive value there. But now the people that sowed the wind are reaping a whirlwind. This type of approach, I think, might be fruitful to all of our problems. The Red Guard, at least, might have been predicted.

Major MARTINO. I would like to ask Dr. Dalkey about his comment. The first reaction is to say, "let's do more social science," and the second reaction is to suggest maybe we don't need to know, or understand, if we make use of the insights we have acquired. People have been looking at people for a long time and doing pretty well. Would you care to expand on your earlier comments?

Dr. DALKEY. If you look over technology and science it is pretty clear that technology as a whole has led science rather than vice versa. Most scientists are inclined to think the reverse—they have been brought up thinking the reverse. As a matter of fact, science has followed technology, and for a very good reason. The steam engine came before thermodynamics. The telephone came before information theory. In short, one of the motivations for scientific research is the demonstration of a controlled capability. In the case of social sciences, there is a very good chance they will follow the same route if we can demonstrate it is possible to be effective in influencing social systems. That would be a tremendous impetus for looking into the reason why. At the moment we could make more use of what has come to be known as social inventions, rather than more social sciences. A great social invention is money. The question is with regard to problems of international conflict. Can we find a device which will enable international transactions to take place without conflict. I don't know of such a device. William James made a point of the moral equivalent of war. Nobody has a moral equivalent yet. I don't see why such a thing shouldn't be invented without the intervention of social science.

Mr. HACKE. Before I make my point, I note Dr. Dalkey's example that technology came before science, from the 19th century, and I wonder if that is still true. There may be something in terms of what kind of social invention might lead to a moral equivalent of war. I don't intend to be facetious but have you noticed how much of the organization of service industries and merchandising has been toward control of depersonalizing the relationship where you go to a gas station, ask for credit and the man says, "It's not the station's policy to give credit." What he means is he is not giving credit, but puts it in an impersonal form and much of the merchandising drive has been toward depersonalizing. That's all right in a study of conflicts in the restaurant situation which come from the fact that waitresses are lower on the ladder than cooks, give orders to the cooks and invented that wonderful gadget you see in some restaurants—whirligig where they put the orders. The reason that gadget is there is so the waitress doesn't tell the cook or hand him the order. She puts it on and he takes it off impersonally. Maybe we can find a whirligig where people can put their aggressive international signals into an impersonal mechanism and get them picked up on the other side of the outfit. The point I originally wanted to make was: Social scientists seem to be overly apologetic about their lack of hardnosed methodology and particularly in psychophysics.

Colonel VAUGHN. I'm not trying to bring the conversation away from Olympian levels, but I want to ask this question because it may possibly lead back to the Olympian levels. I was quite intrigued in Commander Hamilton's talk about the Russians and Chinese cooperating with us in this particular endeavor. This, of course, is out of the usual pattern and when anything is out of the usual pattern, you look twice, because there may be clues to other interesting tangents and we might go through several tangent actions. Would you care to comment why, particularly the Chinese?

Commander HAMILTON. The only reason is the weather in Western Europe. The weather over the United States will end up in Europe and eventually affect Russia and China, so it's a mutual cooperation—mutual benefit, you might say. Beside weather data, which is the main interchange, there is a hotline from Washington to Moscow and we transmit our weather satellite broadcasts to them and they in turn transmit them to us. I don't think anybody knows exactly why but it must be the old philosophy: If it wasn't doing them any good, they wouldn't be doing it. It must be based on this because of the weather as I showed you the whole hemispheric map—now going to the whole globe you can get a complete weather picture. The larger the area the better your forecast.

Colonel VAUGHN. A very significant thing is the fact not only do we think so, but apparently they do also, otherwise, why would they do it? It would be most interesting to know particularly in the last year with the Red Guard eruptions, many things have happened that look like sheer insanity yet they treat it as an every day occurrence.

Commander HAMILTON. Take any data last week and compare it to 1, 2 years ago—it won't vary, they may have their Red Guards, revolution, and everything else, but it hasn't affected their weather communications.

Dr. LINSTONE. I was curious as to what this cooperation actually consists since we have no diplomatic relations.

Commander HAMILTON. Through the auspices of the World Meteorological Organization, the nations throughout use a common weather code. In other words, five digit teletype groups. There are local variations which are authorized by the World Meteorological Organization. We all know the other's variations which is a fairly standard code. When we receive this information at 6,000-words-a-minute from Tinker Air Force Base, it goes into our ADP, the data processing, and is never touched by hand. It is edited, reformatted, and run through the complete data processing based on standard codes, and comes out as raw data. This may not be your point but they do cooperate in common code. For example, you could report at different heights but everybody has agreed to report at a certain standard reporting level. This type of cooperation makes it easier.

Dr. SLAFKOSKY. Let me rephrase the question. What you said was they are cooperating—agreeing to standard forms and terminology; and—in the sense that they are receiving our data and analyses. How are they cooperating by giving us information? The inference I got from Colonel Vaughn's statement was they were in fact providing some information heartily.

Commander HAMILTON. In the last 3 years there have been three World Meteorological Centers: One in Australia, Moscow, and Washington which are common links. I mentioned the hotline from Washington to Moscow. All their teletype data besides this automated weather work was set up by the Air Force although the Weather Bureau is also using it. There is a standard teletype weather broadcast and radio facsimile even in the Mediterranean.

Major STEPHENSON. Are you saying the cooperation exists not because of a need for cooperating between the two groups, but because within their society they must transmit weather data back and forth and we ride the circuit to get it by intercept?

Commander HAMILTON. We do take it by intercept but there are standard teletype circuits into the central collecting centers as in Moscow and then are transmitted to the stations in Europe and the United States.

Major MARTINO. You say they voluntarily provide weather information?

Commander HAMILTON. Yes.

Mr. HACKE. They provide data in the Soviet Union and China?

Commander HAMILTON. Yes, I might say zero-zero and 1200 are the standard times for observation and they cooperate. It would be useless if they had staggered times because you couldn't draw a weather map but all are taking their observations at the same time.

Major MARTINO. So they cooperate by using the same format and ways of making measurements as everybody else?

Commander HAMILTON. Right. They tell us and there's no problem.

Major ERB. I was going to comment on something that interested me when I was in Korea a few years back. When filing for a flight we received a weather briefing and in every military weather station you see a series of yellow pages with 12-hour reports. The military weather station in Korea always had the latest weather report from Pyong Yang, North Korea, which was picked up on their normal teletype sequence.

Major SWETT. This question of science versus technology is another chicken-and-egg problem. The idea of social technology in the blind is what worries me. When you don't know the likely results of your input, you get the same thing Mr. Hacke was discussing, unforeseen impacts whose result may or may not be what you had in mind. This is my point, "Let's find out a little more about what we are doing before jumping into a big program." In the jargon of the social sciences, this is called social tinkering, which I like better than social technology.

Dr. LINSTONE. You mentioned money as a social tool and this tinkering question come in—in the sense that the aid money we are expending may be tinkering. It may create more problems than it solves. Unless we are careful, and this is an important point as in meteorology with the ocean currents and air currents, we don't know what we need to do about the interactions, therefore, tinkering may have an adverse effect on the whole system. The same is true here. It is quite dangerous to look for tools in technology that will do something—in a sense you are tinkering on a public scale, as in climatology.

Dr. DALKEY. In this circumstance, the caricature would be somebody saying to James Watt, "Look, have you thought about the social consequences of your invention," and his answer probably would have been, "No." Assume someone perceptive would have been able to point out the horrors of the early industrial revolution. The picture would have been pretty Biblical and if I had been James Watt and someone had been able to convince me they thought a revolution would be the outcome of it, I might have been inclined to forget it. I doubt if the judgment of history is that it would have been better if he had forgotten it. That's my caricature.

Dr. OBERBECK. Following that line of discussion, the Wright brothers, sitting in judgment of modern airports, might have done the same.

Major MARTINO. I think the term "social tinkering" has buried in it some value judgments which is a bit sneaky.

Major SWETT. The value judgment of it is the idea of making a small move to see what happens; and is essentially a pragmatic value. As far as social tinkering we have been doing it ad infinitum, from time out of mind. We can look at the experiment we call America or the experiment we call Russia which are really the James Watt type social technology approaches: If we don't learn from them, we are irresponsible to say the least. Let's look at the results the people before us received—who had to make the decisions. They had to tinker and invent; let's see the results of their attempt.

Dr. SLAFKOSKY. I am not sure the mass of people—we don't like to call it social sciences to make predictions as to what would follow from Watt's invention, but they would have foreseen the things you are talking about were going to happen. The fact that these happened, I am convinced, is the result of certain happenings and events and people took hold and did certain things with it. It isn't something you or anyone could have foreseen. I don't know that we can use this type of quasihistorical argument. I purposely call it "quasi" because I don't think it's really one—in looking at possible implications. You are not saying, "Look at all the implications before you leap." You are saying, "if you want to accomplish certain objectives, they are more than technological," and I'm talking about hardware, ways of getting them if you take the hardware bit, you are not sure of getting there. Examine it thoroughly; look at all the other avenues of approach you might use and try to see which ones will marry up the best. Am I understanding you properly?

Major SWETT. Yes. There is another jargon term from my own discipline. In international relations the approach is essentially pragmatic like this: "Dear Prince, Yours truly, Machiavelli." Michiavelli has had a bad press but was essentially pragmatic and says "if you want to kick over a monarchy, here's what you do. If you want to kick over a republic, do this. Here's what you do not do and why." That's the value we could contribute to tech forecasting and planning.

Dr. SLAFKOSKY. He pointed out the implication of these things orally and every other way.

Dr. JOHNSTON. I suggest all this sounds Utopian and that computer stimulation would afford a substitute for social tinkering. We had a bad experience, trying to look at society as an experiment because we never had control over the variables with respect to past action. This is the problem of the historian versus the scientist. Our problem is we are tied up with historical processes that never repeat themselves and have the same problem when we extrapolate. We would like to have a typical period of prosperous times with full employment and extrapolate, but it can't be found. The closer you look at history, the more you see: You had recession, then a recovery, another abnormal period, and then a baby boom, everything is abnormal in that respect, it may be that computer stimulation ultimately would be the alternative.

Major SWETT. This is a good point. Such work has gone on at Northwestern with Dr. Guetzkow and been very stimulating in this area. In other words, you don't have to wait for the earth model to run itself out, if you can come up with decent approximations to speed the time factor. A run was made several years ago with four exchange students taking part as decisionmakers in the simulated countries. Dr. Levine, an anthropologist, asked to predict in writing how they would run their countries based on his knowledge of their ethnic background. He predicted nine points of what each would do. The first thing he asked was: "What tribes are they from?" He predicted from that basis and correlated exactly with what they did in subsequent runs of the simulation and how they went about it. One was from a tribe that had been victorious in battle for many years. Most of their holdings were acquired by clubbing somebody which was precisely how he operated. I checked Dr. Levine's predictions with current newspaper reports. The same tribes are still doing the same things.

Dr. SLAFKOSKY. I couldn't agree with you more when you are dealing with individual incidents, repetition of which will never occur because the act, the time, and everything else is different. We may learn something from this field but we will never learn it in the same sense that we are supposed to learn from science, especially the mathematical sciences where we have complete control of the assumptions, the postulates, and the rest is simply logic. Even the physical sciences where we are dealing with inert matter and can repeat things. However, I want to bring to your attention a statement I have heard several times in the last few days: Mainly, that sciences have laws. The physical sciences do not have laws any more so than the social sciences. If we look at the physical sciences you will say, "Yes, they have laws. These laws are by and large patterns that account for the phenomena available to us." These laws change and have changed. For over two thousand years they have been changing as well as the last 10 years, and will continue to change.

Major STEPHENSON. One reason why the physical sciences have been successful is they have been careful with questions they addressed. Many questions within the physical sciences were no less complex than the current questions within social sciences except those in the physical sciences people know better than to ask those questions.

Dr. SLAFKOSKY. The physical scientist has it easy and can cut off what he wants to look at more easily than the social scientist dealing with an individual person who is theoretically unique from anybody else, whereas the only uniqueness about an atom of one kind as opposed to another of the same kind is spatial.

Commander HAMILTON. You will agree that the whole atmosphere is a big laboratory and you can't cut it down but I agree this is true in most physical sciences and you have a control laboratory.

Dr. DALKEY. There is a great deal of wisdom to be gained from using meteorology. In your illustration, if the physical scientists had taken the problem of managing the atmosphere in the 19th century, they would have been about in the same position as the social scientists are who are trying to manage a large society, and physical scientists just refused to accept that as their problem. I don't see any reason why social scientists couldn't refuse to accept this tremendously mobile problem and try something smaller. However, there is one small counter implication and that's the case of the Soviet Union, as I understand it, economists in the United States have taken a great deal of satisfaction with the shift toward western types of economic management going on in the Soviet Union. The conclusion is the great experiment of controlled economy has failed. The United States is doing a good deal better economically than the Soviet Union. However, look at it from another point, it is anything but a failure. Consider the incredible problem they took on themselves: to manage that economy. Maybe they didn't do it perfectly or optimally, or as well as the United States but, they did it. It makes you think of the story of Johnson and Boswell. They went to a vaudeville show where there was, among other things, a dog act, the dog prancing around on his hind legs and after the show Boswell said, "Wasn't it wonderful they way that dog walked around on his hind legs?" Johnson said, "That's not the way to put it. It wasn't wonderful the way he did it; it was wonderful he did it at all." You can make this same comment with regard to the management of an economy. The fact they did it at all is something. I think we can say, out of that comes the lesson something as large as an economy can be mastered. It's not beyond the capabilities of economic science.

One of the reasons for having a technological forecast it to use it in making plans for allocation of R. & D. resources. Mr. Cetron and Mr. Smith describe a procedure currently being used by the Navy to allocate a portion of its R. & D. resources which incorporates a technological forecast as part of the information going into the allocation process.

—Editor

TECHNOLOGICAL FORECASTING IN MILITARY PLANNING

Marvin J. Cetron, Headquarters, Naval Material Laboratory

Donald F. Smith, Naval Ship Research and Development Center

FORECAST: *It is possible to achieve by 1980 fuel-cell powerplants having a lifespan of 2 years.*

FORECAST: *In the same time period, marine gas turbines operating with inlet temperatures of 2,300° F. are also achievable.*

FORECAST: *A 50-percent reduction in the volume of marine steam powerplants is technically feasible by 1980.*

These forecasts, and others, stem from the work of a team of U.S. Navy scientists and engineers at the Annapolis Division of the Naval Ship Research and Development Center, where they are using technology itself to detail future technological developments. Technological forecasting approaches are helping the U.S. military services decide which avenues of research and development offer the largest capability payoffs in terms of military missions. For example, if the military mission is to seek out and destroy other submarines, it might be asked whether it is better to do research on quieting the engines of a submarine or on increasing the power. Clearly, if money and time were unlimited, both pieces of research could go on in parallel, and similarly, all possible research could be pursued on all subjects. But with military systems becoming more and more complex, planners are having to make increasingly more difficult choices. They need a better tool than "gut feeling." The new art or science of technological forecasting is sure to become the new modus vivendi.

The most obvious and perhaps easiest method to use in the attempt to foretell the future is to assume that past trends will continue. Most intuitive forecasts of progress are probably based on this relatively inaccurate extrapolation technique. The less obvious but more scientific techniques are based on quantitative extrapolations that are reproducible by knowledgeable workers in the field.

Over the period of life of a technical development, progress as a function of time most often assumes an S-shaped curve. The growth of a technology is similar to the growth of bacteria. Advance is slow during the initiation period. Then an accelerated growth occurs exponentially. At later stages, approaching and during maturity, the effects of limitations, due perhaps to theoretical limits, cause a deceleration, and the progress curve levels off.

The U.S. Navy must perform certain wartime missions such as target detection and destruction, strategic deterrence (Polaris), and amphibious operations. The Mission/Technology organization chart (fig. IX–1) shows how the design characteristics of vehicles that would be needed to accomplish the given missions are derived. While each mission would call for vehicles with different characteristics optimized, all vehicles would have many functions in common, such as detection, propulsion, offense, defense, etc.

A forecast can be made for each of these functions. For instance, propulsion forecasts would be in terms of specific weight of the propulsion system, its reliability, noise, and whatever other parameters are considered significant for the mission. It should be understood that the U.S. Navy uses the same ships for many types of missions and therefore tradeoffs are required. The subsystem segments examined would be the propulsion transmission system, energy converter, thrust producer, and whatever other segments are needed.

In this fashion, we can work our way down the chart, eventually going into any degree of detail we wish. This information is used for very practical ends. For example, here's part of the forecast for gas turbines.

Gas turbines that have high power and light weight and that can operate in a salt sea-air environment, could well be the powerplants that make it possible for

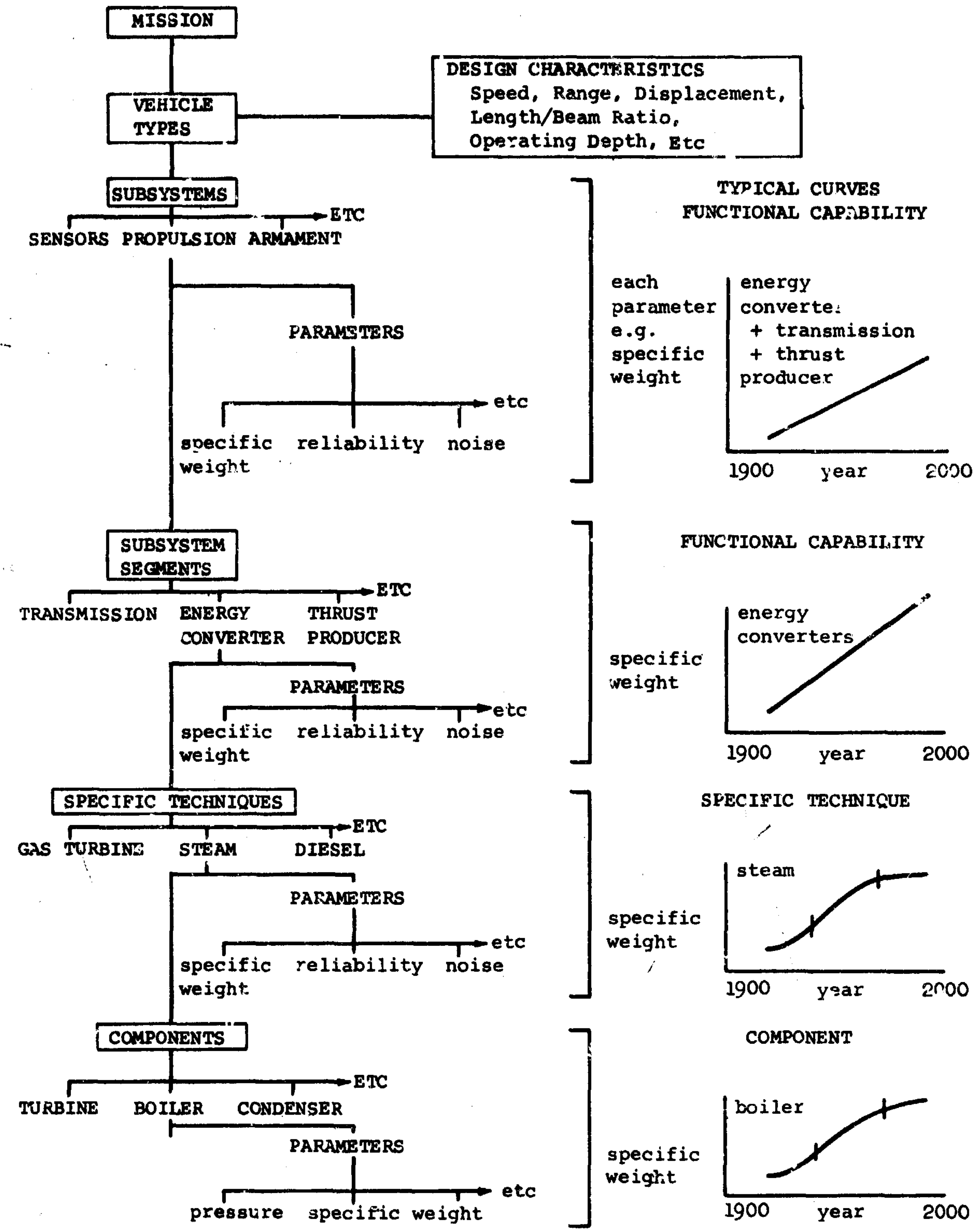


FIGURE IX-1.—*Mission/Technology organization chart.*

us to have advanced hydrofoil craft, subtender hydroskimmers and other high speed (100 kts. and better) naval vessels.

Marine gas turbines have a tremendous potential for development. The possibilities for high-power, lightweight, compact powerplants are unmatched in any other type of unit. Powerplants with these characteristics are particularly vital for powering new-concept vessels such as hydrofoils and aircushion craft. See illustrations IX–1, IX–2, and IX–3.

A rapid growth in the power capabilities of gas turbine units has occurred. Engines as large as 43,000 horsepower have been built, and units exceeding 50,000 horsepower are projected. This growth trend will probably continue but at a lesser rate as limitations of mechanical, thermal, and ducting size factors are approached. However, much larger power outputs will be obtained by using multiple gas generators to drive a single power turbine. Power outputs as high as 150,000 horsepower have already been attained by this method.

The history of development in areas of weight, volume, and fuel consumption for the simple cycle gas turbine are shown in figure IX–2, IX–3, and IX–4. The engine components, namely the compressor, combustion chamber, and turbine, were investigated to determine their development trends. As shown in figure IX–5 potential improvements in compressor, combustion, and turbine efficiency are slight. Any future improvements extending those already made will bring about little increase in efficiency; consequently, work to improve the efficiency of these components will have an insignificant effect on the future engine performances.

Compressor pressure ratio has increased significantly during recent years, but any further increase will be small. Because of improvements in blade loading, compressors are now designed to an optimum pressure ratio determined by turbine inlet temperature. Blade loading, which has enabled engines to obtain higher pressures with fewer stages, appears to be approaching a practical limit.

The addition of more heat energy within the same basic engine configuration has been the major contributing factor to the recent and future improvement in the engine characteristic parameters. Therefore, the parameter that will have a significant effect on the engine is the turbine inlet temperature. The increase of inlet temperature is shown in figure IX–6. Extrapolation of the curve to temperatures in excess of 3,000° F is based on laboratory tests in which operating temperatures as high as 4,000° F have been achieved.

It can be seen when comparing the figures that the specific weight of the simple gas turbine, volume, and fuel-consumption improvements parallel that of the turbine inlet temperature.

Additional savings in weight and volume of gas turbines can be expected by using improved materials and better structural designs and blade-cooling techniques. These practices have been applied to increase inlet temperatures to today's figures, but major increases in this area are still possible.

Thus, from a practical point of view, research and development effort should be concentrated in the following areas on the basis of the high payoff possibilities for improvement of the gas turbine for marine purposes:

- Cooling of turbine blades and other components in high temperature ambients. This will allow higher turbine inlet temperatures that will, in turn, lead to significantly improved performance levels.
- New materials and protective coatings for these high ambient-temperature components. This will increase life expectancy by increasing resistance to high-temperature oxidation and sulfidation. An increased resistance to thermal fatigue and creep is also required.
- Improved materials, designs, and fabrication techniques for regenerative gas turbines to reduce their cost, weight, and bulk.
- Further application and adaptation of aircraft gas turbines and technologies to marine service.

Technical areas where relatively low payoffs for the expended effort could be expected include:

- Attempts to improve efficiency of combustion, compressor, and turbine. These efficiencies in the best existing units are about as high as can reasonably be obtained.
- Attempts to increase significantly the blade-loading or compressor-pressure ratio unless accompanied by major design changes.

We can match technologies against the needs of the U.S. Navy. The planning matrix shown in figure IX–7, based on hypothetical values, shows that in the gas-turbine area, there are still major problems to be solved with respect to small boats, integrated Fleet propulsion, integrated ship auxiliaries, ship-noise control, and surface effects ships. The matrix also shows the functional areas where gas turbines are applied.

Three factors used by the U.S. Department of Defense to evaluate systems programs are military utility, technical feasibility, and financial acceptability. These factors are also important when planners evaluate re-

ILLUSTRATION IX-1.

ILLUSTRATION IX-2.

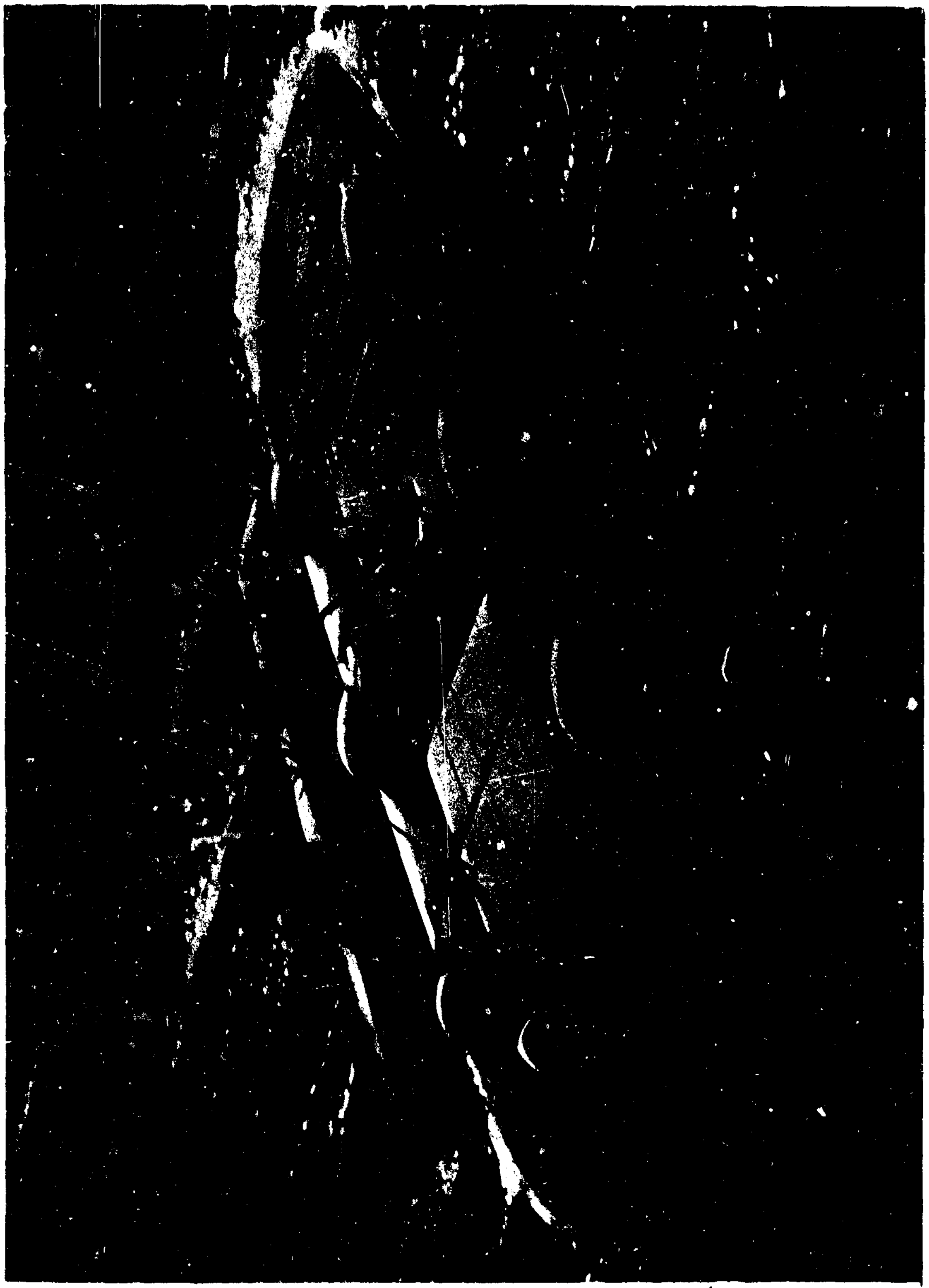


ILLUSTRATION IX-3.

SPECIFIC TECHNIQUE

SIMPLE CYCLE GAS TURBINE

SHAFT HORSEPOWER PER UNIT WEIGHT

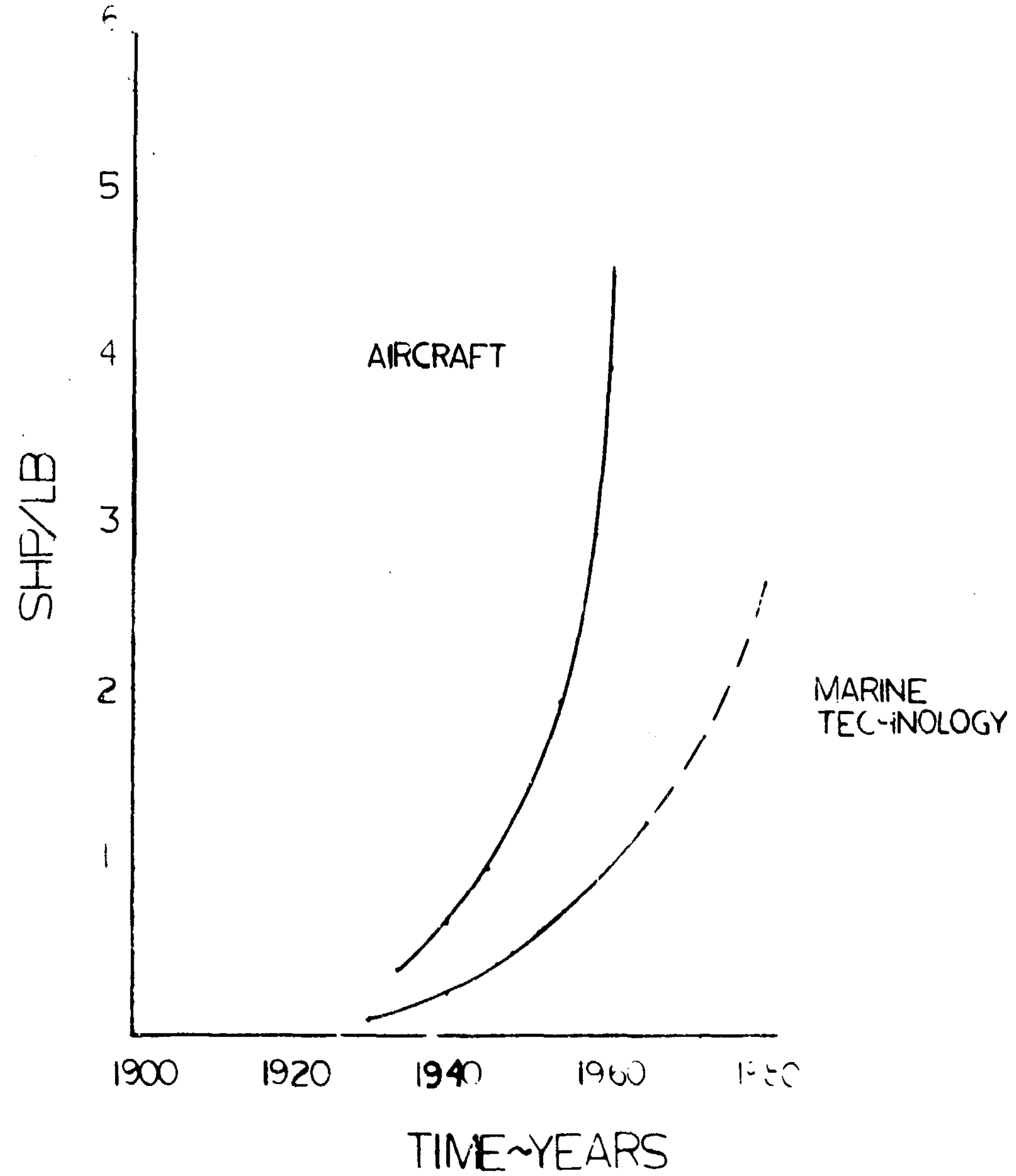


FIGURE IX-2.

SPECIFIC TECHNIQUE

SIMPLE CYCLE GAS TURBINE

SHAFT HORSEPOWER PER UNIT VOLUME

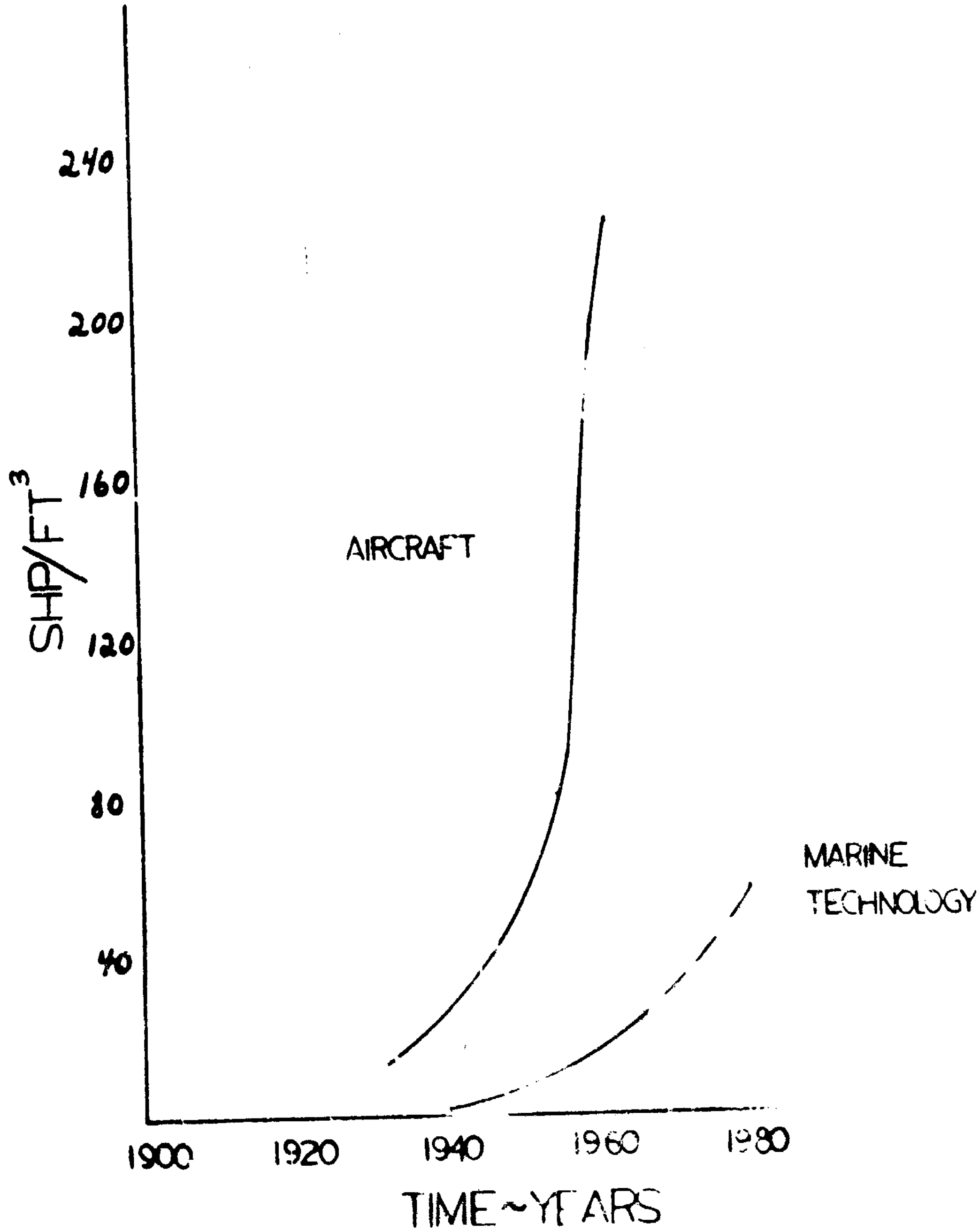


FIGURE IX 3.

SPECIFIC TECHNIQUE
SIMPLE CYCLE GAS TURBINE

SPECIFIC FUEL CONSUMPTION

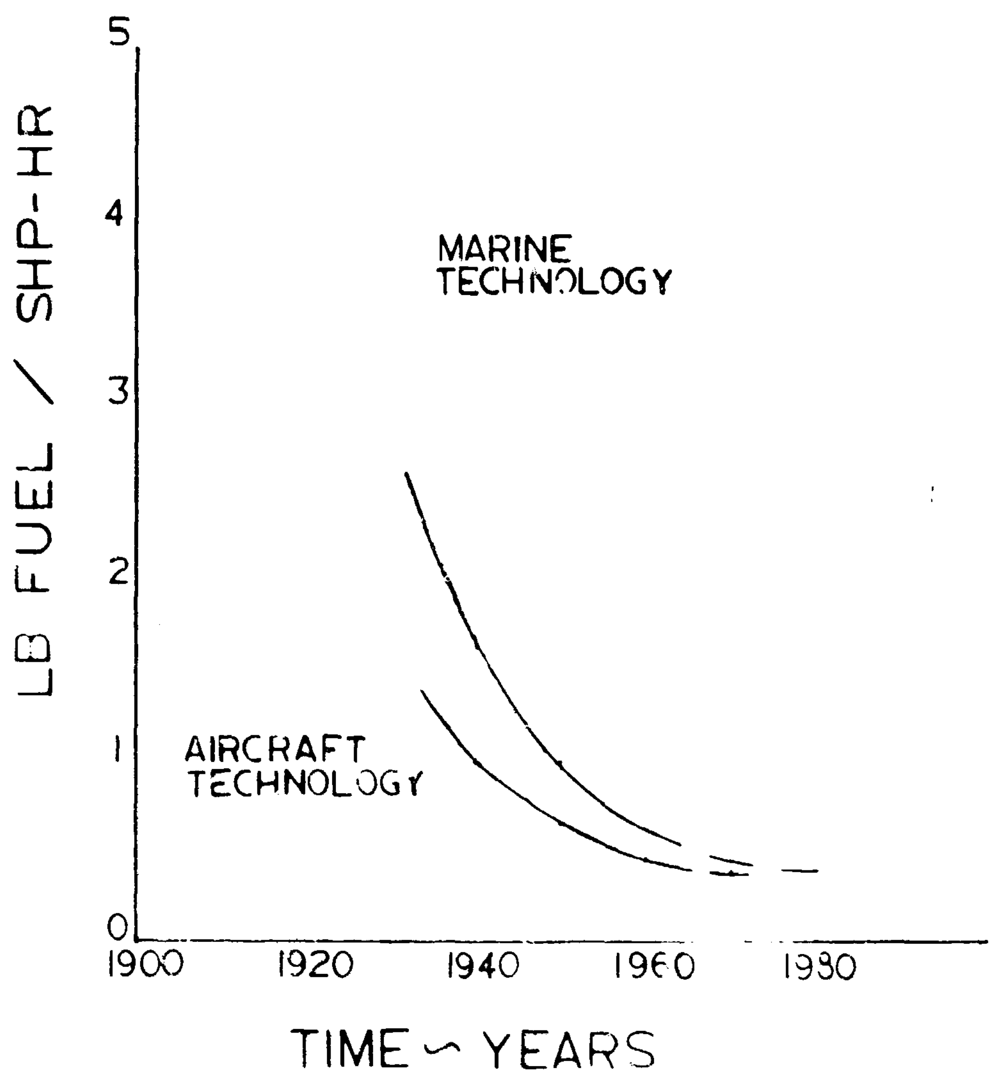
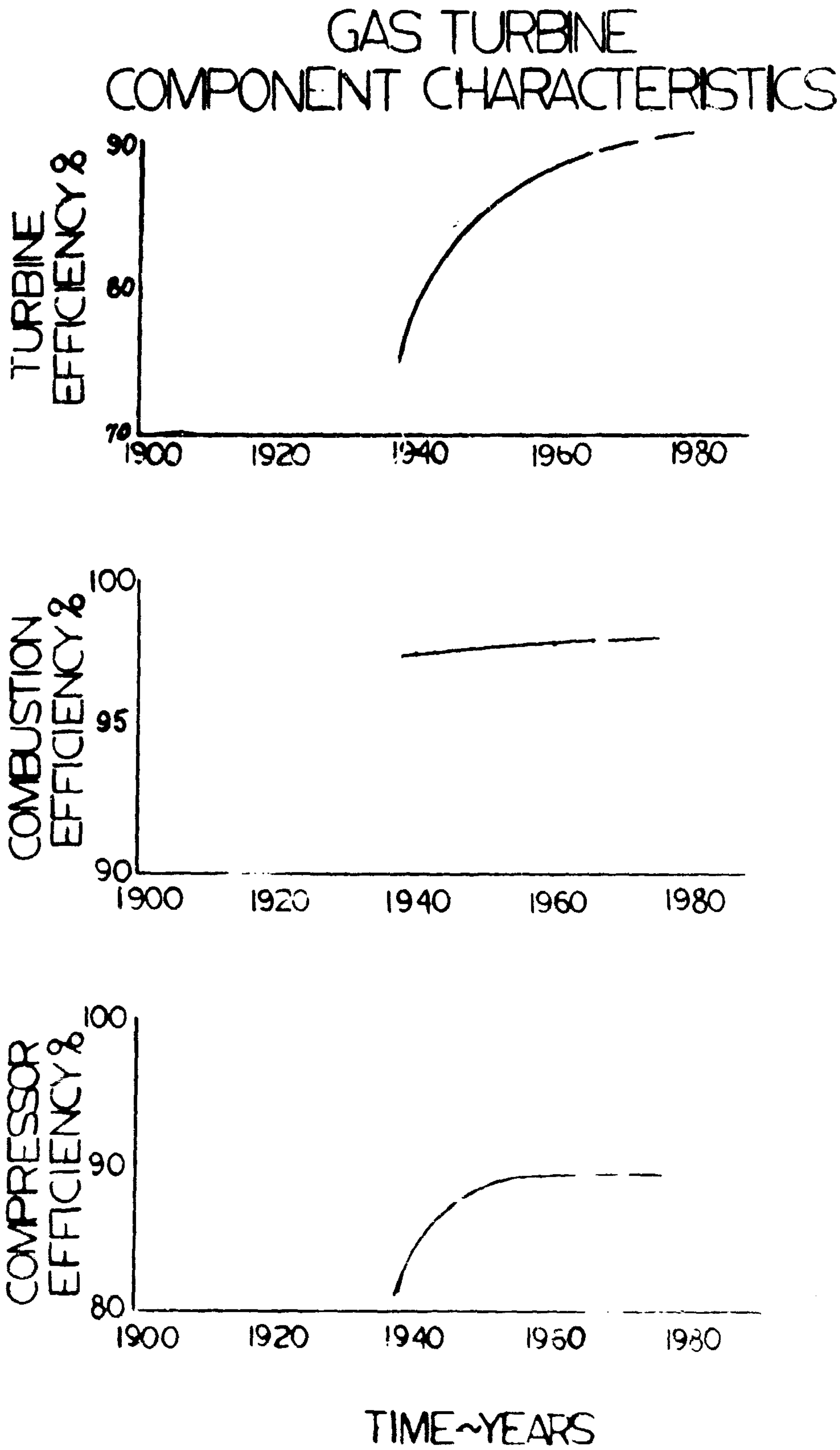
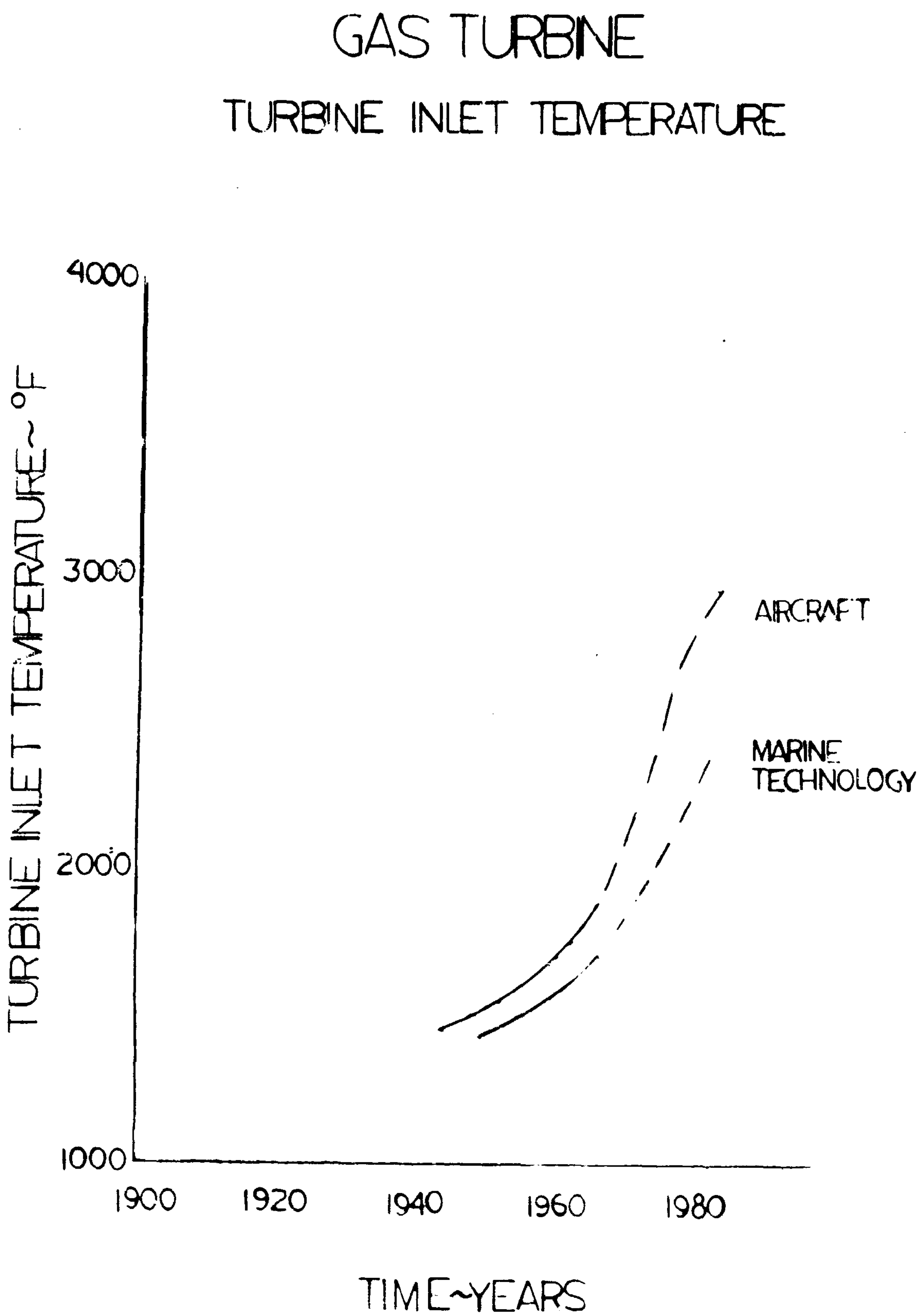


FIGURE IX-4.



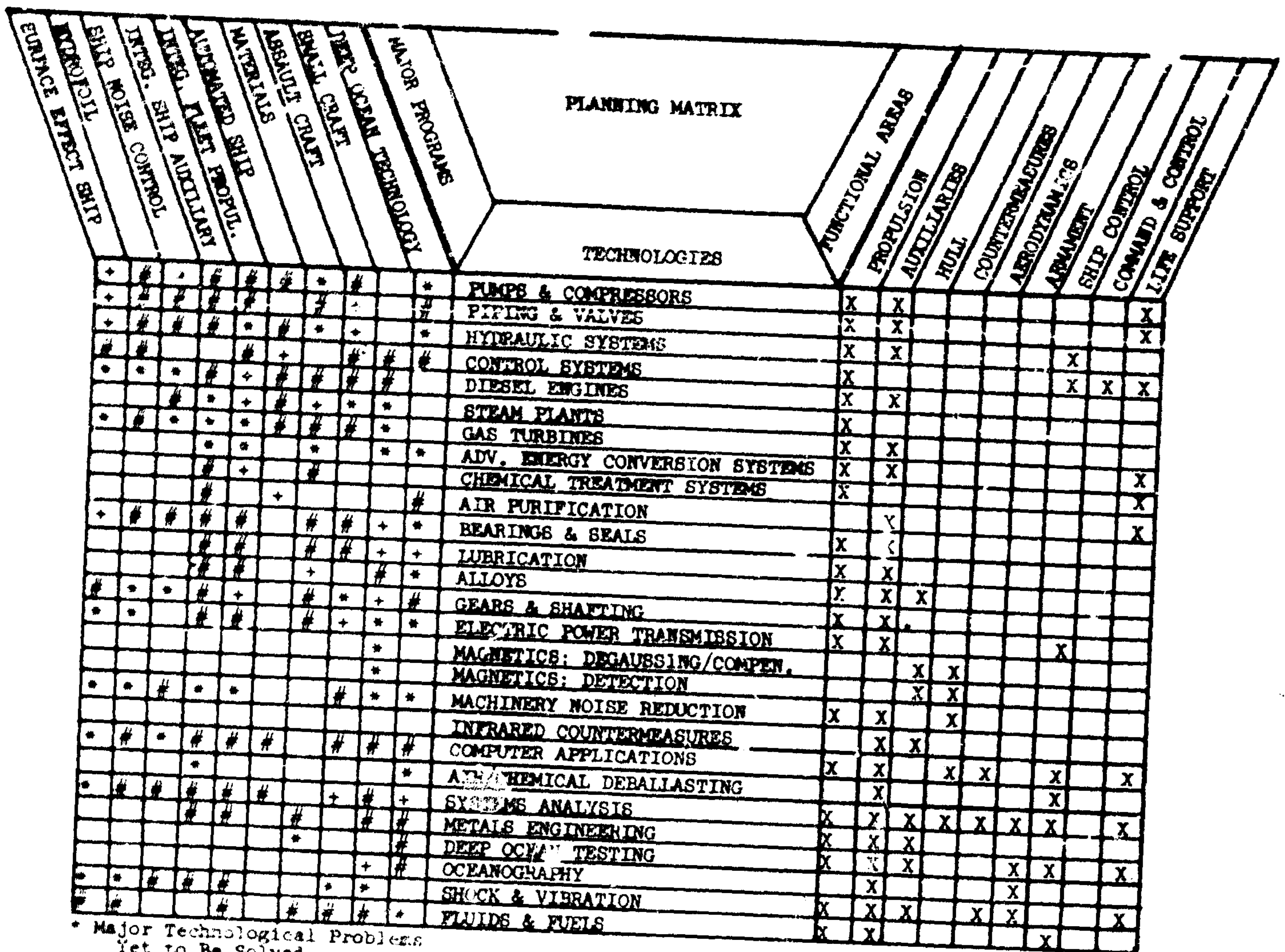
GAS TURBINE
COMPONENT CHARACTERISTICS
TURBINE EFFICIENCY %
90
80
70
1900
1920
1940
1960
1980
COMBUSTION EFFICIENCY %
100
95
90
1900
1920
1940
1960
1980
COMPRESSOR EFFICIENCY %
100
90
80
1900
1920
1940
1960
1980
TIME~YEARS

FIGURE IX-5.



GAS TURBINE
TURBINE INLET TEMPERATURE
TURBINE INLET TEMPERATURE~ °F
4000
3000
2000
1000
AIRCRAFT
MARINE
TECHNOLOGY
1900
1920
1940
1960
1980
TIME~YEARS

FIGURE IX-6.



* Major Technological Problems Yet to Be Solved

\# Technology in Hand - Application Problems Remain

\+ No Major Problems Unsolved

FIGURE IX-7.

search and development. However, it is necessary to quantize these factors so that they may be compared for different Research and Development programs.

Appraisal sheet IX–1 addresses the problems of military utility. Military utility with respect to development atmosphere is a measure of R. & D. work in terms of its usefulness in meeting U.S. Navy's General Operational Requirements (GOR). To be useful, hardware or information must provide a new or improved capability in the shortest possible time after its need is recognized. Thus, military utility is made up of three interdependent criteria: value to naval warfare, responsiveness, and timeliness. In this condensed version, we will consider value to naval warfare.

This criterion considers the extent of the contribution of a task area objective (TAO), a unit of work, in terms of its inherent value as well as its military operational value. The importance of a task is measured by its relative impact on any individual naval warfare category as well as the number of categories receiving a contribution from the task objective. This is done by multiplying the assigned value of the warfare category by the impact value of the contribution to arrive at a value for each individual category. The sum of these values will determine the value of the task area objective to the operator's guidance. *Note:* The figures of merit, or point values assigned to each naval warfare category (col. 1) are dummy figures, they were assigned for this example only. The actual total number of points assigned these 29 naval categories are equal to 100, and they are assigned for test purposes on the basis of the importance of each of these categories in the 1975–80 time frame since this is when most of our current exploratory development work will find its way into the Fleet. The operational users provided the test figures based on the present world situation and their estimates of the most probable future situations.

When the warfare area specialist filled in column 2, the impact of the task area objective contributions, he considered the descriptors at the bottom of the page (Scale of Definitions). In some cases the four descriptors do not adequately describe the contribution; in those cases he interpolates between these numbers.

The credibility of the ratings of technical feasibility and the probability of success increase if they are rated by personnel who have the necessary technical expertise and competence, as they can best judge these factors on the basis of the ability and experience of the individuals and or organizations carrying on the development efforts under consideration.

The top half of appraisal sheet IX–2 solicits the opinion of the technical specialist regarding the probability of achieving the total task area objective that is being undertaken. It considers whether the task could be successfully accomplished from a scientific and technical feasibility point of view. Technical risk also takes into consideration the degree of confidence or prediction that the remaining portion of the total objective can be attained. The degree of confidence or prediction that the remaining portion of the total task objective can be attained usually assesses the factors of the present state-of-the-art, either implicit or explicit. This technical appraisal is naturally based on technical forecasts and includes time factors and resource levels.

Therefore, the technical specialist checks the box that best describes his opinion regarding the task area objective being evaluated, as well as the number of different concurrent approaches being taken which are also a measure of probability of success.

The area called "sacred cow?" and "who says?" was also considered in what we call the "management environment." This section solicits opinions on the acceptability of the effort in the management structure. Here, the evaluator is asked to give what he believes to be the Washington environment considerations concerning this effort, and he checks the applicable box.

The bottom of appraisal sheet IX–2 is then analyzed. The total program is calculated by value, expected value, and desirability index for three funding levels, by the computer. The inputs for military utility come from Appraisal Sheet IX–1.

For example: Suppose the proposed task area objective (TAO), or R. & D. effort, is to devise a system able to detect submerged submarines a given distance away from a sensor, say 20 miles. We shall consider the criterion "Value to Naval Warfare." Of the 29 naval general operational requirements shown in column 1 of appraisal sheet IX–1, the TAO would be of value and contribute only to five GOR's: airborne ASW, surface ASW, submarine ASW, undersea ASW, and ASW ancillary support.

With respect to airborne ASW, the success of the R. & D. venture in this hypothetical example is considered a revolutionary extension of capabilities, and is accorded 0.7 point. At the same time, airborne ASW is said to contribute 5 out of the 100 units assigned to all the GOR's. Thus, the value of the TAO to naval warfare with respect to airborne ASW is $0.7 \times 5 = 3.5$. The other categories can be similarly evaluated for their contributions, and the total value of this TAO to naval warfare is summed at 11.4, as shown on the appraisal sheet.

For our calculation of the Probability of Success (P_s) in meeting the TAO, we use the probability chart

VALUE TO NAVAL WARFARE

Column 1 - Categories	Column 2 Impact of Task Contributions 1.0	.9	.8	.7	.6	.5	.4	.3	.2	.1	Column 3 Value to Individual Category
31 - STRIKE WARFARE											
6 - Airborne Attack											
3 - Surface Attack											
5 - Submarine Attack											
4 - Amphibious Assault											
7 - Sea Based Strategic Deterrence											
3 - Airborne Anti-Air Warfare											
3 - Surface Anti-Air Warfare											
31 - ANTISUBMARINE WARFARE											
5 - Airborne ASW				✓							3.5
4 - Surface ASW						✓					2.0
5 - Submarine ASW						✓					2.5
10 - Undersea Surveillance								✓			3.0
2 - Mining											
3 - Mine Countermeasures											
2 - ASW Ancillary Support									✓		0.4
23 - COMMAND SUPPORT											
3 - Command and Control											
4 - Naval Communications											
4 - Electronic Warfare											
- Navigation											
- Ocean Surveillance											
5 - Reconnaissance & Intelligence											
1 - Environmental Systems											
1 - Special Warfare											
15 - OPERATIONAL SUPPORT											
2 - Logistics											
4 - Personnel											
2 - Astronautics											
2 - Aviation Support											
2 - Ship Support											
2 - Ordnance Support											
1 - NBC Defense											

4. TOTAL VALUE TO NAVAL WARFARE = 11.4

Scale of Definitions for "Impact of Task Contribution" (Column 2):

Points - Descriptors

1.0 Creation of radically new mission concepts (meets overriding critical need)
.7 Revolutionary extension of capabilities
.4 Incremental or marginal improvement of capabilities
.2 Increase in economy

APPRAISAL SHEET IX-1.

Probability of Success

- [ ] 80 - 100% Chance of Meeting TAO
- [x] 30 - 80% Chance of Meeting TAO
- [ ] 0 - 30% Change of Meeting TAO

Number of Different Concurrent Approaches

☐ 1	☐ 3	☐ 5	☐ 7	☐ 9
☐ 2	☑ 4	☐ 6	☐ 8	☐ 10 or more

Sacred Cow? Who Says?

- S-1 ☐ President
- S-2 ☐ Congress
- S-3 ☐ DOD (Department of Defense)
- S-4 ☐ ASN (R&D) (Assistant Secretary of Navy for Research and Development)
- S-5 ☐ JCS (Joint Chiefs of Staff)
- S-6 ☐ CNO (Chief of Naval Operations)
- S-7 ☐ CND (Chief of Naval Development)
- S-8 ☐ Other ____________

Appraisal Summary

No. of GOR's 5

Value (V) 11.4

Probability of Success (P_s) 0.9375

Expected Value (EV) 11.4 x 0.9375 = 10.7

Optimum Funding $2 million

Desirability Index (D) 5.35

APPRAISAL SHEET IX-2.

TABLE IX-1.

Tabulation of P_s

n \ C	0.8	0.5	0.2
1	0.80000	0.50000	0.20000
2	0.96000	0.75000	0.36000
3	0.99200	0.87500	0.48800
4	0.99840	0.93750	0.59040
5	0.99968	0.96875	0.67230
6	0.99993	0.98438	0.73786
7	0.99997	0.99219	0.79029
8	0.99999	0.99609	0.83223
9	0.99999	0.99805	0.86578
10	0.99999	0.99902	0.89263

shown on table IX–1. In this chart, n is the number of concurrent approaches used to accomplish the TAO, and C is a number arbitrarily assigned to the chances of succeeding in a given approach. We use:

80 – 100% chance of success: $C = 0.8$
30 – 80% chance of success: $C = 0.5$
0 – 30% chance of success: $C = 0.2$

We assume that all approaches n have the same chance of success, and therefore the same value of C. If each n were to have a different C, a more involved calculation would have been necessary.

The number assigned to the probability of one approach failing is then $(1 - C)$.

The number assigned to the probability of all approaches failing is $(1 - C)^n$.

Further, if we assume that at least one of the approaches taken will succeed, then the number assigned to the probability of success P_s is $1 - (1 - C)^n$.

This figure for P_s is filled in on appraisal sheet No. 2 under the Probability of Success column.

Example: On an appraisal sheet IX–2, we might have had four approaches ($n = 4$) with a 30–80-percent chance of meeting TAO ($C = 0.5$). Then the number corresponding to the probability of success is 0.93750 or 93.75 percent. From our previous example we calculated the total value of a given TAO to be 11.4. Therefore, the expected value is $11.4 \times 0.9375 = 10.7$.

The preceding has been a discussion of concurrent approaches. If the task area were made up of phased or sequential operations, these probabilities would be handled in a different manner.

Three funding levels are utilized in the concurrent approach: the actual/optimum, maximum, minimum.

The actual/optimum consists of the latest approved fiscal data. For each subsequent year, funds are entered based on what is estimated as necessary to achieve the completion date if the task area is supported at an optimum rate. An optimum rate is one which permits aggressive prosecution using orderly developmental procedures—not a crash program.

The maximum consists of what could effectively be expended in advancing the task area completion date. Maximum funding is the upper limit in which unlimited resources are assigned in order to accelerate the accomplishment of a task area.

The minimum consists of what could be effectively utilized to maintain continuity of effort and some progress toward fulfilling the task area objective. Minimum funding is the threshold limit below which it would not be feasible to continue further efforts in the task area.

The simplified formula is:

$$\text{Value } (V) \times \text{Prob. of success } (P_s) = \text{Expected Value } (EV)$$

$$\frac{\text{Expected Value } (EV)}{\text{Funding Level } (C)} = \text{Desirability Index } (D)$$

These techniques are intended, not to yield decisions, but rather to furnish information which will facilitate making decisions. Indeed these techniques are merely thinking structures to force methodical, meticulous analysis. The *data* plus the *analysis* only gave us *information*. It takes this *information* plus *judgment* to render the *decisions*.

There are certainly technical problems in making forecasts even if the methodology is firmly established. One of these areas is in selecting the parameters that are to be forecast. This, incidentally, is the first step one uses to prepare a technological forecast for any area of interest.

The experience of such experts in the field as Ralph Lenz, Jr., of the U.S. Air Force, and Prof. James Bright of Harvard University, points up the benefit of choosing parameters that are or can be considered independent of any specific technology and not constrained by man-generated rules or regulations. The approach used at the Annapolis Division of the Naval Ship Research and Development Center is to have appropriate technical experts choose parameters characteristic of a functional capability, for example, pounds/horsepower for energy converters, and which are sufficiently descriptive to afford a scientific assessment of specific techniques, applicable to the systems under consideration.

To establish the future possibilities of an energy converter for a marine propulsion system, one must have forecasts of at least the following parameters—

specific weight, specific volume, efficiency, and total power capacity per unit. These predictions or forecasts are associated with marine energy converters in general. By summing up the optimums of each capability, we arrive at an idealized energy converter. However, it must be realized that a unit incorporating the optimum value of each of the forecasted capabilities will probably never be built because some of the capabilities arrived at are often contradictory. The lightest unit is generally not the highest power unit, as an example. However, the value of the analysis is that these functional capabilities do represent a set of potentially achievable goals for development programs if one is willing to invest sufficient money.

When choosing the parameters to be forecast for specific techniques, planners must be certain to select those parameters that are necessary to discriminate between alternative techniques. With respect to the example of energy converters for a ship propulsion system, if a selection is to be made on other than a subjective basis, forecasts of the following parameters must be made: specific weight, specific volume, efficiency, reliability, maintainability, ease of automation, noise and cost. Moreover, if the analysis is to be meaningful, each parameter must be related to each other for each technique. It would certainly be faulty to select a given specific technique, for example, diesel engines, as the best powerplant on the basis of efficiency and ease of automation without, at the same time, accepting the fact that among other detriments the diesels are not the lightest or quietest units.

Once the planners select their parameters and circumscribe the areas for data gathering, they must face the problem of what constitutes valid data. The researcher gathering the data must be sufficiently knowledgeable to reject fluke data points but open-minded enough to accept valid points perhaps beyond his personal expectations. The U.S. Navy's approach to this problem is to use engineers trained in the technical area (electrical engineers for electric drives, mechanical engineers for steam and diesel), but not personnel considered expert, to do the literature search and plot the historic data.

To derive the functional capability curves, researchers examines the record setters and experimental units in order to provide the best that can be accomplished for any given function. On the other hand, for the specific technique curves, only data from operational equipment are used so that the curves reflect only practical capabilities. Each data point is throughly documented and background material filed. These files are invaluable when curves are reviewed by the experts, because many arguments occur when data points do not agree with subjective opinions.

The technical library is invariably the starting point in the search for supporting data. Early data are obtained from classic texts, which also indicate the life span of the technology. Technical reports usually are the source of recent and recent-past data, particularly reports of ship trials for Navy projects. Obviously, the most time-consuming element in generating a technological forecast is in this data gathering, and it can often be costly as well. In some instances, library research is not adequate, for example, where reliability data are needed. Here, no good data exist in recognizable form and must be accumulated at the source, that is, from ship records, shipyards, and manufacturers, and reduced before it is useful for forecasting.

For the extrapolated portions of the specific technique curves, which are in fact the forecasts, several constraints are imposed. First, wherever possible, a theoretical limit is established, which indicates a capability that can be approached only asymptotically and never equalled or exceeded. Second, the functional capability curves are monitored because without a scientific breakthrough, these could not be exceeded by any specific technique. Third and of highest significance, the design functions associated with the specific techniques under investigation are examined for those components and/or subparameters which have the most influence on the performance of the equipment. After the most important contributors are identified, forecasts involving them are made to show the degree of improvement that can be expected in the basic equipment as a result of expected improvement in the component or design limits.

When forecasts are made at the subparameter level, experimental units, laboratory tests, and basic research results are used to gain an insight into the advances that can be expected in the technology. An example of this can be seen in several of the gas-turbine curves accompanying this article.

Finally, technical fields related or contributing to the one under investigation are studied to find out if technological advances in these fields may be ahead of the one being investigated. An instance of this procedure is shown by the curves of aircraft gas-turbine technology versus marine. This procedure bears fruit only in relatively few cases, such as when comparisons are made with advances in the aircraft and space fields, where progress is the result of spending unusually large amounts of money. well beyond that normally expected under less urgent conditions.

The human element in forecasting is perhaps the most difficult aspect to handle. When the subject of technological forecasting is brought up, the first reaction one usually gets is skepticism. Surely, nobody claims that the procedures we have described give a clear look into the future. Nevertheless, the fact that our window may be fogged a little should not lead one to believe that we have no window at all. And the lack of certainty with respect to the future should not take away from the value of technological forecasting for long-range planning purposes.

There is little doubt that if we can delineate factors that have contributed to state-of-the-art advancement, and then plot their progress over the years to establish trends, these trends will continue unless deformed by external constraints such as the reaching of a theoretical limit or the arrival of a new technique of clearly superior capabilities.

In the broadest sense, if technological forecasting is looked upon as movement or progress rather than the upward march of numbers, then the quantitative aspects of the forecasts become relatively unimportant. A trend will have been established, and errors, even as much as 50 percent, will have little effect on the usefulness of the material.

Another problem, which is essentially a human one, is to assess where the levels of effort which have been applied in the past to some techniques will continue in the future. The knowledge held by some workers in a particular field might indicate to them that a certain technique is in disfavor with policy makers. Therefore support for this technique is apt to be lessened, thereby imposing an artificially depressed slope in the curve of progress. However, periods of favor and disfavor are apt to cancel each other out, and where progress is needed and is possible, progress will usually occur.

Nobody disputes that all subjectivity should be ferreted out in gathering historical data and in generating forecasts. It is equally true that this is a difficult thing to ask. We do not argue that subjectivity cannot lead in some instances to good forecasts. However, such forecasts are not reproducible in the scientific sense and therefore are difficult to generalize or apply broadly. In a practical sense, the Navy attempts to walk the middle ground between subjectivity and statistical extrapolation, and is guided by as much common sense as a situation will permit. The future, after all, is simply a logical extension of the past and present, and all we have to do is find the curve to extrapolate.

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Monterey, Calif. 93940

BIOGRAPHICAL SKETCHES

Norman C. Dalkey

Dr. Dalkey received the B.A. degree from San Jose State College in 1937, and the Ph. D. from the University of California at Los Angeles in 1942. He was an instructor in meteorology at UCLA in 1942–43. Subsequently, he went to the Radiation Laboratory of the University of California at Berkeley, where he worked as a physicist until 1946. From 1946 to 1948 he was assistant director, Sherman H. Dryer Productions, New York. In 1948 Dr. Dalkey joined the RAND Corp. as a mathematician, and is currently with the Mathematics Department of RAND. In 1957–58 he was on leave from RAND as Chief, Planning Analysis Team, Operations Analysis Office, Hq U.S. Air Force. Dr. Dalkey has done extensive work in game theory, large-scale military simulation, and in group information processes.

Calvin W. Taylor

Dr. Taylor received his B.A. and M.A., University of Utah; Ph. D., University of Chicago, 1946, under L. L. Thurstone.

During World War II Dr. Taylor was Military Chief, Military Personnel Subsection of the Army personnel research program. Since 1946 he has worked in Psychology at the University of Utah and has taught various measurement, research technique, personnel, and human engineering courses. In 1952–54, on a leave of absence from the University of Utah, he served as director of research, Office of Scientific Personnel, National Academy of Sciences-National Research Council in Washington, D.C. In 1964–65 he was on a year's leave from the University of Utah, serving as Director, Personnel Measurement Research Department, U.S. Naval Personnel Research Activity in San Diego. Dr. Taylor has initiated and supervised numerous research projects with support from the Air Force, National Science Foundation, National Institutes of Health, Peace Corps, National Aeronautics and Space Administration, Richardson Foundation, and the U.S. Office of Education. He has produced numerous unpublished reports and has published over 70 articles, six large monographs, and four books.

Dr. Taylor is a member of the Special Education National Committee of the United Cerebral Palsy Association, of the Nursing Research Study Section of the National Institutes of Health, and a consultant to various organizations.

Richard Irwin

Mr. Irwin has been employed by the Bureau of the Census since 1966. At present he is working on national and local population projections. Mr. Irwin has developed population estimates and projections for the States of Oregon and California, 1960–66, and has received his B.A. in 1941 at Reed College and his M.A. in 1953 at the University of California (Berkeley).

Donald S. Akers

Mr. Akers has been employed by the Bureau of the Census since 1950. His present assignment is to develop a demographic model using computer simulation. Mr. Akers received his B.A. at Columbia University in economics with work in demography under Kingsley Davis.

Harper Q. North

Harper Q. North, vice president, Research and Development for TRW, Inc., received his B.S. degree in physics from the California Institute of Technology in 1938. He received his M.A. degree in 1940, and his Ph. D. degree in physics from the University of California in 1947. In 1958 he completed the Executive Program in Business Management at UCLA.

Dr. North was a research assistant at the General Electric Research Laboratory at Schenectady, N. Y., in 1940 and 1941, working on electron diffraction and physical chemistry. In 1942, as a research associate at GE, he initiated work on radar mixer crystals and demonstrated principles employed in today's germanium varactor diodes. Dr. North joined the Hughes Aircraft Co. in 1949, initiating semiconductor work there, and he became director of the Hughes Semiconductor Division in 1953. He founded Pacific Semiconductors, Inc. (now TRW Semiconductors), a subsidiary of TRW, in 1954. He was company president until 1961. In the semiconductor field, Dr. North holds many fundamental patents, and since 1950, has served as consultant to the Department of Defense as a member of its Advisory Group on Electron Devices.

In 1962, Dr. North became a TRW corporate vice president; and is responsible for the intergroup coordination of all research and development activities of TRW.

He is a Fellow of both the American Physical Society and of the Institute of Electrical and Electronic Engineers and is a member of the Technical Board of the Society of Automotive Engineers. He is past president of the Electronic Industries Association and was the recipient of its 1966 Medal of Honor. Dr. North's current civic activities include the presidency of the California Institute for Cancer Research.

Harold A. Linstone

Harold A. Linstone was born in 1924 in Hamburg, Germany. He received his B.S. (Phi Beta Kappa) in 1944 from City College of New York, has a master's degree from Columbia University, and a Ph. D. in mathematics from the University of Southern California.

Dr. Linstone was at Hughes Aircraft Co. for 11 years where he was head of the Military Missions Analysis staff and later became senior scientist. There, he conceived and directed the first of the *Mirage* Long-Range Defense Planning studies. For 1 year he was on leave to the Institute for Defense Analyses. His assignments there included a tour of 5 months in Dr. Enthoven's systems analysis office in OSD. Subsequently, he was at the RAND Corp. and while there served as scientific advisor to the Howze Board. Dr. Linstone has been a consultant to the Center for Naval Analyses, the Hudson Institute, and Stanford Research Institute. Since 1963, he has been senior scientific advisor for the corporate offices of Lockheed Aircraft Corp., and is now adjunct professor in Industrial and Systems Engineering at the University of Southern California.

Dr. Linstone has lectured at the Army Air Defense School (Fort Bliss), Office of Naval Research, and U.S. Army War College. In 1965, he participated in a lecture course to the Israeli Defense Forces.

Dr. Linstone is a member of the American Mathematical Society, the Operations Research Society, Institute of Management Sciences, and an Associate Fellow of the American Institute of Aeronautics and Astronautics.

Denis F. Johnston

Dr. Johnston is chief, Branch of Labor Force and Demographic Projections, Division of Labor Force Studies, Bureau of Labor Statistics. Before coming to the Bureau, he was a demographic statistician in the population division of the Bureau of the Census. Dr. Johnston also teaches courses in Population and Manpower and Sociological Theory at Howard University and at the U.S. Department of Agriculture Graduate School. He received a Ph. D. in sociology from the American University in 1961. His dissertation, "An Analysis of Sources of Information in the Population of the Navaho," was published in 1966 as Bulletin 197 of the Bureau of American Ethnology, Smithsonian Institution. He has also been the author or coauthor of several articles in the Monthly Labor Review on the Educational Attainment of Workers and on Long-Range Labor Force Projections. He is a Fellow of the American Sociological Association and a member of the Population Association of America, the International Union for the Scientific Study of Population, and the Philosophy of Science Association.

James E. Hacke, Jr.

Mr. Hacke received an A.B. in 1940 and an M.S. in 1941 from the University of Georgia, and has done 3 years' additional graduate work at Columbia University and at Pennsylvania State College, all in physics and mathematics. He worked on the rugged radio proximity fuse at Johns Hopkins University Applied Physics Laboratory from 1943 to 1946 and did basic ionosphere research at Pennsylvania State College from 1946 to 1946. Mr. Hacke received a B.D. *cum laude* from Seabury-Western Theological Seminary in 1950 and then served as an Episcopal rector and college chaplain in Illinois, New York, Arizona, and California. He continues to serve as an Episcopal priest on a volunteer basis. Beginning in 1957, he served as a communication physicist on the staff of the General Electric Co. Technical Military Planning Operation, forecasting electronics technology and conditions in the radio spectrum. He served as a member of the U.S. delegation to the I. T. U. Ordinary Administrative Radio Conference in 1959, and to various C.C.I.R. meetings. In 1963, Mr. Hacke joined the Technology Management Programs Division of Stanford Research Institute, and there has studied communication, command, and control aspects of large systems and organizations; telecommunication management; and future conditions in electronics and telecommunication. His most recent study has been of the methodological aspects of forecasting technological change and innovation and its economic and social consequences. Mr. Hacke is a member of the American Physical Society, Phi Beta Kappa, and Sigma Xi, and is a senior member and former subsection chairman of the Institute of Electrical and Electronic Engineers.

Glenn D. Hamilton

Commander Hamilton received the degree of B.A. in meteorology from the University of California at Los Angeles in 1953. In 1954 he was designated as a naval aviator, and served as a patrol-plane pilot in the Pacific area until 1957. From 1957 to 1959 he attended the U.S. Naval Postgraduate School, receiving the degree of M.S. in meteorology in 1959. From 1959 to 1962 Commander Hamilton served as a meteorologist aboard the Carrier USS *ESSEX* (CVA–9), in the Atlantic and the Mediterranean. In 1962 he was assigned as a research meteorologist at the Navy Weather Research Facility, Norfolk, Va. From 1963 to 1966 he studied at the University of Stockholm, Sweden, receiving the degree Filosofie Licentiat in 1966. Since 1966 Commander Hamilton has held his present assignment as assistant officer in charge, Fleet Numerical Weather Facility, Monterey, Calif.

Marvin J. Cetron

Mr. Cetron is currently responsible for the Navy's Technological Forecast efforts and is located in the Advanced Concepts Branch, Headquarters Naval Material Command. He holds a B.S. from Pennsylvania State University, an M.S. from Columbia University and is currently completing his Doctoral studies in R. & D. Management at American University. His technical experience includes positions as Head, Planning Branch, Navy Marine Engineering Laboratory; Engineering Assistant to the Technical Director, Naval Applied Science Laboratory; and Head of the Management Planning Review Branch of the Applied Science Laboratory. Mr. Cetron has published extensively in the fields of Planning, Resource Allocation, Technological Forecasting, and Operations Research. He is a member of the Military Operations Research Society; American Institute of Industrial Engineers; the Interservice Technological Forecasting Methodology Group; and the Interagency Group for Research on Research.

Donald F. Smith

Mr. Smith received the degree of B.S. in mechanical engineering from Tufts University. His experience includes 16 years with private industry, including General Electric Co., American Engineering Co., North American Rayon Corp., Globe Aircraft Corp., and the Johns-Manville Corp. His work included powerplant design and construction, aircraft manufacturing, machine design, process equipment design, and field engineering. He served 2½ years on active duty with the Navy as an ordnance office, and training officer. Mr. Smith worked for 14 years at the Naval Air Development Center, Johnsville, Pa., starting as a project engineer in the Aircraft Armament Laboratory, later as Branch Head in the Anti-Submarine Warfare Laboratory, then became Project Manager for the A-NEW System of airborne ASW electronics. He also served as Program Administrator for the ASW Laboratory, and has been with the Annapolis Division, Naval Ship Research and Development Center for 2½ years. Currently, Mr. Smith is the Head, Technical Planning Staff, of the Annapolis Division of the Naval Ship R. & D. Center, where he is concerned with developing small craft and air cushion vehicle R. & D. programs.

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13. ABSTRACT

This document consists of the Proceedings of the Second Symposium on Long Range Forecasting and Planning held at Alamogordo, New Mexico, 11-12 October 1967. The papers presented discussed technological forecasting, population forecasting and weather forecasting. Discussion of each paper is included.

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